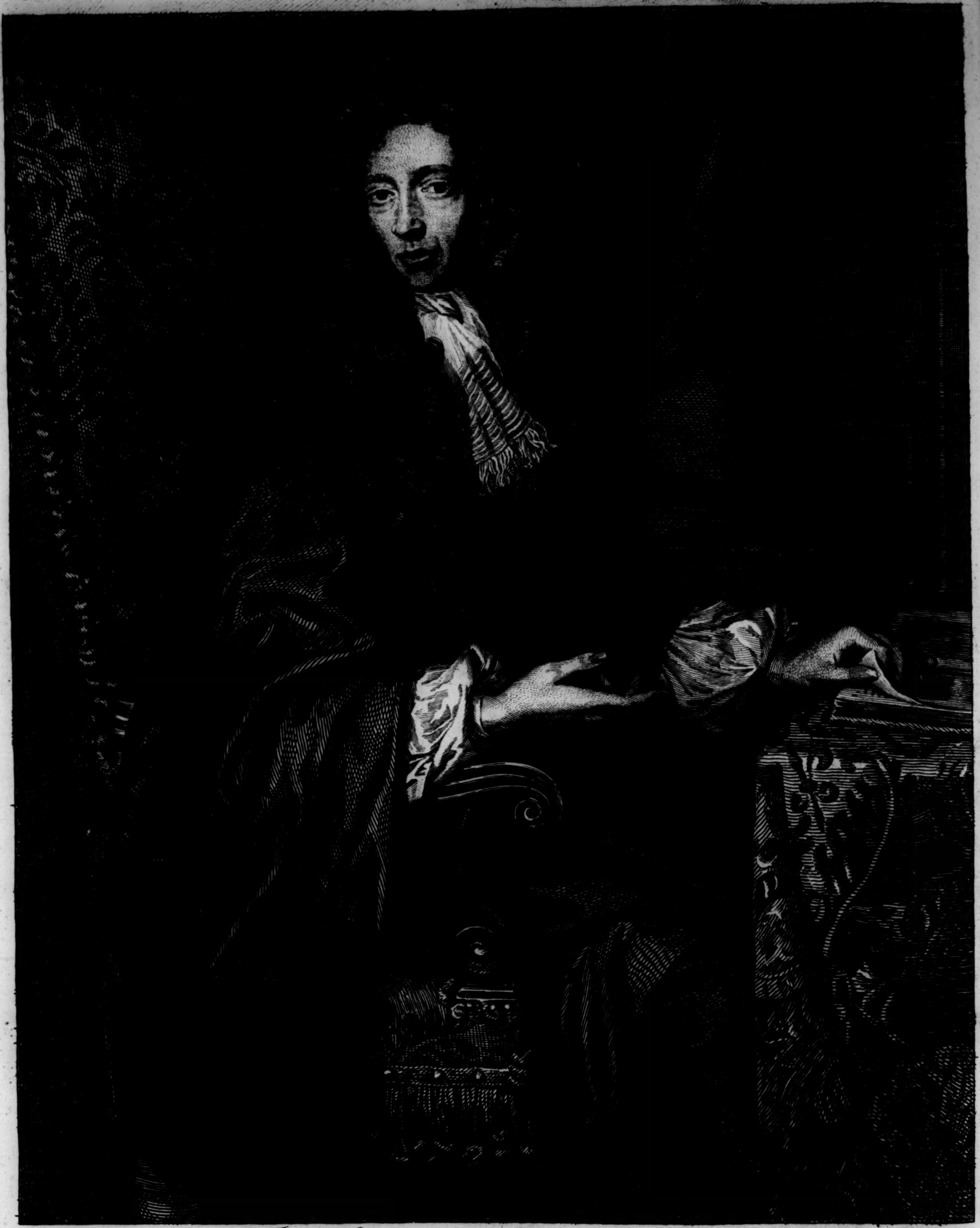

J. Kerſseboom pinxit. The Honourable **ROBERT BOYLE.** *B. Baron Sculp.*

THE

WORKS

OF THE HONOURABLE

ROBERT BOYLE.

In SIX VOLUMES.

To which is prefixed

The LIFE of the AUTHOR.

VOLUME THE FIRST.

A NEW EDITION.

Ex rerum Causis Supremam noscere Causam.

H. Gravelot inv. et delin.

LONDON:
Printed for J. and F. Rivington, L. Davis, W. Johnston, S. Crowder, T. Payne,
G. Kearsley, J. Robson, B. White, T. Becket and P. A. De Hondt,
T. Davies, T. Cadell, Robinson and Roberts, Richardson
and Richardson, J. Knox, W. Woodfall, J. Johnson,
and T. Evans. MDCCLXXII.

MVSEVM
BRITAN
NICVM

THE

PREFACE.

(*To the Folio Edition of* 1744.)

THE general reaſons for collecting into one body the works of the honourable *Robert Boyle* are as obvious, as the excellence of the ſeveral parts of them is univerſally acknowledged. It may ſuffice therefore to acquaint the reader, that as a complete ſet of thoſe before publiſhed is not perhaps to be found any where, except in the vaſt library of his learned friend Sir *Hans Sloane*, Bart. and as the whole contains a very large collection of philoſophical eſſays on a great variety of ſubjects, full of curious experiments and obſervations, with ſeveral valuable diſcourſes upon the moſt important ſubjects of religion; ſuch a collection had been long deſired, both for the public benefit, and as a juſtice due to the memory of that great reſtorer of the mechanical philoſophy; and is now executed in the following manner:

I. The ſeveral tracts formerly printed are taken from the moſt improved editions with the utmoſt correctneſs, and diſpoſed in the order of time in which they were firſt publiſhed. This diſpoſition of them was determined upon, as well from the reaſon of the thing, and with a view of ſhewing the riſe, progreſs, and dependencies of the Author's diſcoveries; as in conformity to his own judgment: who, upon occaſion of a Latin edition of his works at *Geneva* in 1677, complained publickly by his friend Mr. *Oldenburg*, ſecretary of the Royal Society, in the *Philoſophical Tranſactions*, No. CXXX. p. 766, 767. "That the year in the frontiſpiece of that edition is one and the ſame, as if the ſeveral books contained in this Latin volume had been publiſhed in one year; and the enumeration of the ſeveral treatiſes, made in the catalogue of this Latin edition, is not according to the time wherein they were firſt printed. For the *firſt* of the books mentioned in the ſaid catalogue was publiſhed in Engliſh A. 1660, the *fifth* and *ſixth* A. 1661, the *ſecond* A. 1662, the *ſeventh* A. 1664, the *fourth* A. 1666, the *third* A. 1670, the *eighth* A. 1671, the *tenth* A. 1672, the *ninth* A. 1673. So prepoſterouſly are thoſe books ranged in this catalogue and volume: which the reader was to be

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“ informed of, that by comparing the ſeveral true dates of the firſt edition
“ of this Author's works with the books of others ſince printed, the
“ priority of the experiments and conſiderations, reſpectively contained in
“ them, may be truly ſtated.”

II. Some very conſiderable additions are made in this edition, which were never before publiſhed; namely, fragments of an *Appendix* to the *firſt Part* of the *Chriſtian Virtuoſo*, and of the *ſecond Part* of that work, preſerved purſuant to the Author's own deſire; with a large collection of letters of Mr. *Boyle* and his friends upon various ſubjects, ſelected from about fifteen hundred written by moſt of the great men of the laſt age both at home and abroad, with whom he correſponded. Theſe additions the public owes to the reverend and learned Mr. *Henry Miles* of *Tooting* in *Surrey*, and F. R. S. who is poſſeſſed of thoſe manuſcripts of Mr. *Boyle*, which were put into his hands, with leave to make uſe of them for the public good, by the late worthy Mr. *Thomas Smith*, apothecary in the *Strand*, who lived ſeventeen years with Mr. *Boyle*, and was with him at his death; and which have been lately increaſed by a part of the original collection, that had been communicated to Dr. *William Wotton*, author of *the Reflections upon ancient and modern Learning*, and were reſtored by his ſon-in-law the reverend Mr. *William Clarke*, canon reſidentiary of *Chicheſter*. Theſe manuſcripts are very numerous, but many of them written while the Author was very young, and few completed. However I ſhall ſubjoin to the *Contents* the liſt of them as drawn up by his own order, all of them being ſtill extant, except thoſe marked with an aſteriſk before them.

III. The copper-plates illuſtrating the ſeveral pieces are engraved with much greater exactneſs and elegance than thoſe in any former edition.

IV. There being only two original pictures of Mr. *Boyle* now known to be extant, it was thought proper to have them both engraved. One, which repreſents him in the 38th year of his age, is placed in the title-page of each volume, copied from a drawing of Mr. *Faithorne*, communicated by Sir *Hans Sloane*, from which likewiſe Mr. *Faithorne* himſelf engraved his print, with the inſtruments accompanying the head, according to the deſign of Dr. *Robert Hooke*, who thought the *face very carefully and well done, and very like*; as appears by his letters to Mr. *Boyle* of Auguſt 25, September 8, and December 15, 1664, printed in Vol. VI. p. 487, 488, and 501, of this edition. The other, which fronts the title-page of the firſt volume, is taken from an original painting done in the latter part of his life, and now in the collection of Dr. *Mead*.

V. THE *Life* contains Mr. *Boyle's* own *Account of himſelf during his minority*, extending to the year 1642; and is continued to his death from the beſt materials that could be procured at this diſtance of time, and without moſt of thoſe advantages which Dr. *Burnet* biſhop of *Saliſbury*, and Dr. *William Wotton*, who had the ſame deſign near fifty years ago, might have obtained for the execution of it. The only qualities I can engage for are induſtry and fidelity; but the genius and abilities of a *Gaſſendus* would be requiſite to do juſtice to a character ſuperior even to that of the celebrated *Peireſkius*.

I MUST not conclude this preface without returning my ſincereſt acknowledgments to Mr. *Miles* abovementioned, to whoſe great labour, judgment, and ſagacity, the conduct and improvements of this edition are chiefly to be aſcribed; and to Mr. *John Ward*, profeſſor of rhetoric at *Greſham* college, whoſe advice and aſſiſtance I have found extremely ready and uſeful to me upon this and all other occaſions.

THOMAS BIRCH.

London, Nov. 16, 1743.

THE
LIFE
OF THE HONOURABLE
ROBERT BOYLE.

By THOMAS BIRCH, M.A. and F.R.S.

THE honourable *Robert Boyle* was deſcended from a family, whoſe name before the conqueſt was written *Biuvile*. The firſt footing, that we find of them in the county of *Hereford*, was at *Pixely* court, near *Ledbury*, of which *Humphrey de Biuvile* was lord, as is recorded in Doom's-day book, tit. 28. *Terra Humfridi de Biuvile in Radelau Hund. Humfridus de Biuvile tenet de Rege Pichelei Auſhil tenuit.* Theſe words *Auſhil tenuit*, in the general conſtruction of antiquaries, import, that it was in the time of *Edward* the Confeſſor *(a)*.

Lodowick Boyle (b), who lived in the reign of king *Henry* III. was father of *John Boyle*, and he of *James*, who had iſſue *Lodowick*, whoſe ſon was ſucceeded by *James* his ſon and heir, father *(c)* of *Lodowick Boyle* of *Bidney*, and of the Friers in the city of *Hereford*, in the reign of king *Henry* VI. This *Lodowick* married *Elizabeth*, daughter of *William Ruſſel*, Eſq; and had iſſue *(d)* a daughter, *Eleanor*, married to *Watkin Ruſſel*, and two ſons, *John Boyle*, Eſq; who had the eſtate in *Herefordſhire*, and *Roger Boyle*, ſecond ſon. This *Roger* married *Jane*, daughter of *Thomas Pattiſhal* of the county of *Hereford*, and had iſſue *John Boyle* of *Hereford*, *Roger*, ſecond ſon, and *Michael Boyle* of *London*, third ſon, who left a numerous iſſue, whereof *Richard*, his ſecond ſon, was biſhop of *Corke* and *Roſs*, being allowed alſo to keep the See of *Cloyne* in commendam, was afterwards archbiſhop of *Tuam*, and died March 19, 1644. He left iſſue *Michael Boyle*, archbiſhop of *Armagh*, and lord chancellor of *Ireland*, who died, aged 93, in the year 1702, and was father to *Murrough Boyle*, created lord viſcount *Bleſſington*. *Michael*, another ſon of the ſaid *Roger*, was biſhop of *Waterford* and *Liſmore*; and Sir *George Boyle*, his fourth ſon, died without iſſue. *Roger Boyle*, the ſecond ſon, married *Joan*, daughter of *John Naylor* of *Kenvile* in the county of *Kent*, and had iſſue *John Boyle*, biſhop of *Corke* and *Roſs* in *Ireland*; *Richard Boyle*, ſecond ſon, earl of *Corke*, and father of that great man, of whom I am to treat; *Hugh Boyle*, third ſon, who died without iſſue; and a daughter married to Sir *Pierce Power* of *Ireland*, Knt.

Richard, the ſecond ſon, was one of the greateſt men of his age; and as he has left ſome memoirs of himſelf, written in 1632, in the ſixty-ſeventh year of his age, which were never yet publiſhed entire, I ſhall inſert ſo valuable a piece here in juſtice to his character and memory.

" *Sir*

(*a*) Letter of Doctor *John Beale* to Mr. *Samuel Hartlib*.

(*b*) Viſitat. Co. Hereford. in Biblioth. Harleyanâ 90. A. 17. p. 72, 73. apud *Collins*'s Peerage of *England*, Vol. II. p. 358. 2d. edit. *London*, 1741.

(*c*) *Seager*'s Baron. M. S. in Biblioth. Cotton. apud *Collins*, p. 359.

(*d*) Viſitat. Co. Hereford, *ut antea*.

“ *Sir* RICHARD BOYLE, *Knt. earl of* CORKE, *his true Remembrances.*

I Sir *Richard Boyle*, Knt. lord *Boyle*, baron of *Youghall*, viſcount *Dungarvan*, earl of *Corke*, lord high treaſurer of *Ireland*, one of his majeſty's honourable privy council, and one of the two lords juſtices for the government of this kingdom, do commend theſe *true remembrances* to poſterity this 23d day of June, *anno Domini* 1632, having lived in this kingdom of *Ireland* full forty-four years, and ſo long after as it ſhall pleaſe almighty God.

My father, Mr. *Roger Boyle*, was born in *Herefordſhire*. My mother *Joan Naylor*, daughter to *Robert Naylor* of *Canterbury* in the county of *Kent*, Eſq; was born the 15th of *October* in the 21ſt year of king *Henry* VIII. and my ſaid father and mother were married in *Canterbury* the 16th of *October* in the 8th year of queen *Elizabeth*. My father died at *Preſton* near *Feverſham* in *Kent* the 24th of March 1576. My mother never married again, but lived ten years a widow, and then departed this life at *Feverſham* aforeſaid, the 20th of March 1586. And they both are buried in one grave in the upper end of the chancel of the pariſh church of *Preſton*; in memory of which my deceaſed and worthy parents, I, their ſecond ſon, have *in anno Domini* 1629 erected a fair alabaſter tomb over the place where they were buried, with an iron gate before it for the better preſervation thereof.

Sir *Richard Boyle*, now earl of *Corke*, the ſecond ſon of *Roger Boyle*, Eſq; was born in the city of *Canterbury*, as I find it written by my father's own hand, the 3d of October 1566.

After the deceaſe of my father and mother, I being the ſecond ſon of a younger brother, having been a ſcholar in *Bennet* college *Cambridge*, and a ſtudent in the *Middle Temple*, *London*, finding my means unable to ſupport me to ſtudy the laws in the inns of court, put myſelf into the ſervice of Sir *Richard Manwood*, Knt. lord chief baron of her majeſty's court of Exchequer, whom I ſerved as one of his clerks; and perceiving, that the employment would not raiſe a fortune, I reſolved to travel into foreign kingdoms, and to gain learning, and knowledge, and experience abroad in the world. And it pleaſed the Almighty by his divine providence to take me, I may ſay juſtly, as it were, by the hand, and lead me into *Ireland*, where I happily arrived at *Dublin* on the Midſummer-Eve, the 23d day of June, 1588. I was married at *Limerick* to Mrs. *Joan Apſley*, one of the two daughters and coheirs of *William Apſley*, Eſq; the 6th of November 1595, who brought me in 500 *l.* lands *per* year, which I ſtill enjoy, it being the beginning and foundation of my fortune; and ſhe died at *Moyallo* the 14th of December 1599, in travel of her firſt child, who was born a dead ſon, and both of them buried together in *Buttavant* church.

When firſt I arrived at *Dublin* in *Ireland*, the 23d of June 1588, all my wealth then was twenty-ſeven pounds three ſhillings in money, and two tokens, which my mother had given me, viz. a diamond ring, which I have ever ſince and ſtill do wear, and a bracelet of gold worth about ten pounds; a taffety doublet cut with and upon taffety, a pair of black velvet breeches laced, a new *Milan* fuſtian ſuit laced and cut upon taffety, two cloaks, competent linen and neceſſaries, with my rapier and dagger. And ſince, the bleſſing of God, whoſe heavenly providence guided me hither, hath enriched my weak eſtate in beginning with ſuch a fortune, as I need not envy any of my neighbours, and added no care or burthen of my conſcience thereunto. And the 23d of June 1632, I have ſerved my God, queen *Elizabeth*, king *James*, and king *Charles*, full forty-four years, and ſo long after, as it ſhall pleaſe God to enable me.

WHEN God had bleſſed me with a reaſonable fortune and eſtate, Sir *Henry Wallop* of *Wares*, Sir *Robert Gardiner*, chief juſtice of the King's Bench, Sir *Robert Dillam* chief juſtice of the Common Pleas, and Sir *Richard Bingham*, chief commiſſioner of *Connaught*, being diſpleaſed for ſome purchaſes I had made in the province, they all joined together, by their lies complaining againſt me to queen *Elizabeth*, expreſſing, that I came over a young man, without any eſtate or fortune; and that I had made ſo many purchaſes, as it was not poſſible to do it without ſome foreign prince's purſe to ſupply me with money: that I had acquired divers caſtles and abbeys upon the ſea-ſide fit to receive and entertain Spaniards; that I kept in my abbeys fraternities and convents of friers in their habits, who ſaid maſs continually: and that I was ſuſpected in my religion; with divers other malicious ſuggeſtions, whereof having ſome ſecret notice, I reſolved to go into *Munſter*, and ſo into *England*, to juſtify myſelf. But before I could take ſhipping, the general rebellion in *Munſter* broke out: all my lands were waſted, as I could ſay, that I had not one penny of certain revenue left me, to the unſpeakable danger and hazard of my life. Yet God preſerved me, as I recovered *Dingle*, and got ſhipping there, which tranſported me to *Briſtol*, from whence I travelled to *London*, and betook myſelf to my former chamber in the Middle Temple, intending to renew my ſtudies in the law, till the rebellion were paſt over. Then *Robert* earl of *Eſſex* was deſigned for the government of this kingdom, into whoſe ſervice I was recommended by Mr. *Anthony Bacon*. Whereupon his lordſhip very nobly received me, and uſed me with favour and grace, in employing me in the iſſuing out his patent and commiſſions for the government of *Ireland*; whereof Sir *Henry Wallop*, treaſurer, having notice, and being conſcious in his own heart, that I had ſundry papers and collections of *Michael Kettlewell*'s his late under-treaſurer, which might diſcover a great deal of wrong and abuſe done to the queen in his late accounts; and ſuſpecting, if I were countenanced by the earl of *Eſſex*, that I would bring thoſe things to light, which might much prejudice or ruin his reputation or eſtate, although I vow to God, until I was provoked, I had no thought of it; yet he, utterly to ſuppreſs me, renewed his former complaints againſt me to the queen's majeſty. Whereupon, by her majeſty's ſpecial directions, I was ſuddenly attached and conveyed cloſe priſoner to the Gate-houſe; all my papers ſeized and ſearched; and although nothing could appear to my prejudice, yet my cloſe reſtraint was continued till the earl of *Eſſex* was gone to *Ireland*, and two months afterwards; at which time, with much ſuit, I obtained the favour of her ſacred majeſty to be preſent at my anſwers, where I ſo fully anſwered and cleared all their objections, and delivered ſuch full and evident juſtifications for my own acquittal, as it pleaſed the queen to uſe theſe words, viz. "By God's death, theſe are but inventions againſt this young man; and all his ſufferings are for being able to do us ſervice, and thoſe complaints urged to foreſtal him therein. But we find him to be a man fit to be employed by ourſelves; and we will employ him in our ſervice; and *Wallop* and his adherents ſhall know, that it ſhall not be in the power of any of them to wrong him; neither ſhall *Wallop* be our treaſurer any longer." Thereupon ſhe directed her ſpeech to her lords in her council there preſent, and commanded them preſently to give her the names of ſix men, out of which ſhe might chuſe one to be treaſurer of *Ireland*, her election falling upon Sir *George Carew*, of *Cookington*. And then the queen aroſe from council, and gave orders, not only for my preſent enlargement, but alſo diſcharging all my charges and fees during my reſtraint, and gave me her royal hand to kiſs, which I did heartily, humbly thanking God for that deliverance.

Being commanded by her majeſty to attend at court, it was not many days before her highneſs was pleaſed to beſtow upon me the office of clerk of the council of *Munſter*, and to recommend me over to Sir *George Carew*, after earl of *Totneſs*, then lord preſident of *Munſter*. Whereupon I bought of Sir *Walter Ralegh* his ſhip called the *Pilgrim*, into which I took a freight of ammunition and victuals, and came in her myſelf by long ſeas, and arrived at *Carrig-Foyl-Kerry*, where the lord preſident and the army were at the ſiege of that caſtle; which when he had taken, I was there ſworn clerk of the council of *Munſter*, and preſently after made a juſtice and quorum throughout all that province. And this was the ſecond riſe that God gave to my fortune.

Then, as clerk of the council, I attended the lord preſident in all his employments, and waited upon him all the whole ſiege of *Kingſale*, and was employed by his lordſhip to her majeſty, with the news of that happy victory; in which employment I made a ſpeedy expedition to the court; for I left my lord preſident at *Shannon* caſtle, near *Corke*, on the Monday morning about two of the clock, and the next day, being Tueſday, I delivered my packet, and ſupped with Sir *Robert Cecil*, being then principal ſecretary of ſtate, at his houſe in the *Strand*; who after ſupper held me in diſcourſe till two of the clock in the morning; and by ſeven that morning called upon me to attend him to the court, where he preſented me to her majeſty in her bedchamber, who remembered me, calling me by my name, and giving me her hand to kiſs, telling me, that ſhe was glad, that I was the happy man to bring the firſt news of that glorious victory. And after her majeſty had interrogated with me upon ſundry queſtions very punctually, and that therein I had given her full ſatisfaction in every particular, ſhe again gave me her hand to kiſs, and recommended my diſpatch for *Ireland*, and ſo diſmiſſed me with grace and favour.

At my return into *Ireland*, I found my lord preſident ready to march with the army to the ſiege of *Beer-haven* caſtle, then fortified and poſſeſſed by the *Spaniards* and ſome Iriſh rebels; which after battering we had made aſſaultable, we entered, and put all to the ſword. His lordſhip fell then to reducing thoſe weſtern rebels of the province to ſubjection and obedience of her majeſty's laws; and having placed garriſons and wards in all places of importance, made his return to *Corke*, and in his way homewards acquainted me with his reſolution; it being preſently to employ me into *England*, to obtain licence from her majeſty, for his repair to her royal preſence: at which time he propounded unto me the purchaſe of all Sir *Walter Ralegh*'s lands in *Munſter*, offering me his beſt aſſiſtance for the compaſſing thereof; which he really performed. For, upon my departure for *England*, he wrote by me two effectual letters, one to Sir *Robert Cecil*, wherein he was pleaſed to magnify my ſervice and abilities; and concluding with a requeſt, that he would make interceſſion with Sir *Walter Ralegh* to ſell me all his lands in *Ireland*, that were then altogether waſte and deſolate. To Sir *Walter Ralegh* he alſo wrote, adviſing him to ſell me all his lands in *Ireland*, then untenanted, and of no value to him; mentioning withal, that, in his lordſhip's knowledge, his eſtate in *Ireland* never yielded him any benefit, but contrariwiſe ſtood him in two hundred pounds yearly for the maintenance and ſupport of his titles. Whereupon there was a meeting between Sir *Robert Cecil*, Sir *Walter Ralegh*, and myſelf; where Sir *Robert Cecil* mediated and concluded the purchaſe between us. Accordingly my aſſurances were perfected; and this was a third addition and riſe to my eſtate.

Then I returned to *Ireland* with my lord preſident's licence to repair to court, where in his way to *Dublin* (where he propoſed to embark) he dealt very nobly and fatherlike by me, in perſuading me, it was high time for me to take a wife, in hopes of poſterity to inherit my lands; adviſing me to make choice of Sir *Geoffry Fenton*'s daughter; and that if I could affect her, he would treat with her parents to have the match between us; wherein he prevailed ſo far, as the 9th of March 1602, I was, in his lordſhip's preſence, contracted to her in her father's houſe at *Dublin*[*].

The 25th of July, 1603, I was married to my ſecond wife, Mrs. *Catharine Fenton*, the only daughter of Sir *Geoffry Fenton*, principal ſecretary of ſtate, and privy counſellor in *Ireland*, with whom I never demanded any marriage portion, neither promiſe of any, it not being in my conſideration; yet her father, after my marriage, gave me one thouſand pounds in gold with her. But that gift of his daughter unto me I muſt ever thankfully acknowledge as the crown of all my bleſſings; for ſhe was a moſt religious, virtuous, loving, and obedient wife unto me all the days of her life, and the happy mother of all my hopeful children, whom with their poſterity I beſeech God to bleſs.

The 10th of July, 1620, my eldeſt brother, Dr. *John Boyle*, lord biſhop of *Corke* and *Roſs*, departed this life at *Biſhop's-Court* near *Corke*; and on the 12th of that month was buried in my new tomb, erected in the chapel, which I re-edified at *Youghall*. After whoſe death I obtained thoſe biſhoprics from his majeſty for my uncle *Michael Boyle*'s ſon, *Richard Boyle*, for whom I formerly obtained the deanery of *Waterford*, who now ſucceeds my brother in thoſe biſhoprics.

I *Richard*, earl of *Corke*, was knighted by Sir *George Carew*, lord deputy of *Ireland*, at St. *Mary*'s abbey, near *Dublin*, the 25th of July 1603, being St. *James*'s day, and the very day that I was married to my ſecond wife, Mrs. *Catharine Fenton*, I was ſworn a privy counſellor to King *James* for the province of *Munſter* at the council table at *Dublin* by the lord *Chicheſter*, then lord deputy of *Ireland*, the 12th of March, 1606, with commandment from the lord deputy and council to *Henry Dunkard*, then lord preſident of *Munſter*, to admit me into that council; who, upon former direction from this ſtate, had refuſed either to ſwear or admit me a counſellor of that province.

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[*] Dr. *Anthony Walker*, in p. 45, 46, of his funeral ſermon on *Mary*, counteſs dowager of *Warwick*, daughter to the earl of *Corke*, relates a very remarkable ſtory, concerning this marriage, which he had from that lady's own mouth, and is as follows: "Mr. *Boyle*, after earl of *Corke*, who was then a widower, came one morning to wait upon Sir *Geoffry Fenton*, at that time a great officer of ſtate in the kingdom of *Ireland*; who being engaged in buſineſs, and not knowing who it was who deſired to ſpeak with him, a while delayed him acceſs, which time he ſpent pleaſantly with his young daughter in her nurſe's arms. But when Sir *Geoffry* came and ſaw whom he had made ſtay ſomewhat long, he civilly excuſed it. But Mr. *Boyle* replied, he had been very well entertained, and ſpent his time much to his ſatisfaction in courting his daughter, if he might obtain the honour to be accepted for his ſon-in-law. At which Sir *Geoffry* ſmiling, to hear one, who had been formerly married, move for a wife carried in arms, and under two years old, aſked him, if he would ſtay for her? To which he frankly anſwered him, he would; and Sir *Geoffry* as generouſly promiſed him, he ſhould then have his full conſent. And they both kept their words honourably; and by this virtuous lady he had thirteen children." Mr. *Evelyn*, in a letter to Dr. *William Wotton*, dated at *Wotton* in *Surrey*, Sept. 10th, 1703, mentions likewiſe this ſtory in the following terms; "that Mr. *Boyle* coming to adviſe with Sir *Geoffry Fenton*, now knighted, finding him engaged with another client, and ſeeing a pretty child in the nurſe's arms, entertained himſelf with them, till Sir *Geoffry* came to him, making his excuſe for cauſing him to wait ſo long. Mr. *Boyle* pleaſantly told him, he had been courting a young lady for his wiſe. And ſo it fortuned, that ſixteen years after, Mr. *Boyle* made his addreſſes in good earneſt, and married the young lady." Both theſe accounts are attended with great difficulties, eſpecially that of Dr. *Walker* which is abſolutely irreconcilable with the earl of *Corke*'s own narrative, from which it appears, that his firſt lady died on the 14th of December 1599, and that he married his ſecond, Sir *Geoffry Fenton*'s daughter, on the 25th of July, 1603; ſo that it is impoſſible, that while he was a *widower*, he ſhould enter into any engagements for the latter, while ſhe was *in her nurſe's arms*, and *under two years old*, ſince there was only an interval of three years and eight months between the death of his firſt wife and his marriage with the ſecond.

I WAS ſworn a privy counſellor of ſtate of the kingdom of *Ireland* by the lord *Chicheſter*, then lord deputy, at *Chicheſter* houſe, the 15th of February, being the day that I arrived out of *England* at *Dublin*, 1612.

I WAS created lord *Boyle*, baron of *Youghall*, on Michelmas day, the 29th of Sept. 1616.

I WAS created lord viſcount of *Dungarvan* and earl of *Corke*, the 26th of October, 1620.

Adam lord *Loftus* of *Ely*, and lord chancellor of *Ireland*, and I, *Richard*, earl of *Corke*, were ſworn lords juſtices for the joint government of this kingdom of *Ireland*, the 26th of October, 1629, with the entertainment of one hundred pounds ſterling per month to each of us.

I WAS made lord high treaſurer of *Ireland*, and ſworn the 9th of November, 1631.

The ſeveral days of the births of all the children, that God hath bleſſed me withall, with the places where they were born, and the names of their godfathers and godmothers.

1. MY firſt ſon *Roger*, born at *Youghall* the 1ſt of Auguſt 1606. Sir *Allen Apſley*, Sir *Thomas Brown* godfathers, and lady *Alice Fenton* godmother. He died at *Deptford* in *Kent*, the 10th of October 1615, and was buried there*.

2. MY firſt daughter, *Alice Boyle* (*e*), born at *Youghall*, the 20th of March, 1607; Sir *Robert Tynt* godfather, lady *Fenton* and Mrs. *Bernard* godmothers.

(*e*) Married to the earl of *Barrimore.*

3. MY ſecond daughter, *Sarah Boyle*, born at *Dublin*, the 29th of March, 1609; Sir *William Uſher* godfather, lady *Winch* and lady *Ely* godmothers. She was married to Sir *Thomas Moore* firſt, and then to lord *Robert Digby*, the 15th of December, 1626.

4. MY third daughter, *Lettice* (*f*), born the 25th of April, 1610; earl of *Thomond* godfather, lady *Chicheſter* and lady *Moore* godmothers.

(*f*) Married to the lord *Goring.*

5. MY fourth daughter, *Joan*, born the 14th of June, 1611; Sir *William Fenton* godfather, and —— *Brown* and lady *Fenton* godmothers. She was married to the earl of *Kildare*, Aug. 15, 1628, and had two children, *Richard* and *Elizabeth*.

6. MY ſecond ſon, *Richard*, was born at the college of *Youghall*, the 20th of October, 1612; earl of *Thomond*, Sir *Richard Aldworth*, and Mr. *Thomas Ball* of *London*, godfathers, and lady *Ann Parſons* godmother. God grant he may ſerve and fear him religiouſly, and be a faithful ſubject and ſervant to the king's majeſty and his heirs, and live many years full of good works and of virtuous children, and be a worthy pillar and patriot in this kingdom. He being viſcount of *Dungarvan*, was knighted in my houſe in *Youghall* the 13th of Aug. 1624, by the lord *Falkland*, lord deputy general of *Ireland*; and my ſaid ſon departed *Dublin*, to begin his travels into foreign kingdoms, the 4th of June, 1632, I allowing him one thouſand pounds a year in his travels.

7. MY daughter *Catharine* (*g*), born the 22d of March, 1614; Sir *Robert Bolton* godfather, lady *Fenton* and lady *Harris* godmothers.

(*g*) Married to the lord viſcount *Ranelagh.*

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* He died at *Say's-Court* in that town, and was interred in the pariſh church, were a monument was erected to him with the following inſcription (*h*):

H. S. E. *ROGERUS BOYLE, Richardi Comitis Corcacienſis Filius primogenitus; qui in Hiberniâ natus, in Cantio ſolo patris natali denatus, dum hic ingenii cultum capeſſit, puer eximiæ indolis præcacitatem ingenii funere luit immaturo. Sic luculenti, ſed terreni patrimonii factus exhæres, cæleſtem crevit hæreditatem. Deceſſit Anno* CIƆDCXV. VI. *Eid.* VIII*bris.*

(*h*) Doctor *George Stanhope*'s letter to *John Evelyn*, Eſq; dated Sept. 6, 1703, and Mr. *Evelyn*'s letter to Dr. *W. Wotton*, dated at *Wotton* in *Surrey*, Sept. 10, 1703.

8. My ſon *Jeoffry* born at *Youghall* the 10th of April, 1616. He died.
9. My daughter *Dorothy*, born the 31ſt of December, 1617. She was married to lord *Loftus*.
10. My ſon *Lewis*, born the 23d of May, 1619, and in the year 1628 he was created baron of *Bandonbridge*, and lord viſcount *Boyle* of *Kenelmeaky*.
11. My ſon *Roger*, born the 25th of April, 1621. He was created lord *Boyle*, baron of *Broghill*, 1628.
12. My ſon *Francis* (*i*), born the 25th of June, 1623.
13. My daughter *Mary* (*k*), born the 11th of November, 1624.
14. My ſeventh ſon *Robert*, born the 25th of January, 1626.
15. My eighth and laſt daughter *Margaret* (*l*), born in *Channel Row* in *Weſtminſter* the 30th of April, 1629.

(*i*) Afterwards lord *Shannon*.
(*k*) Married to the earl of *Warwick*.
(*l*) She died unmarried.

The great God of heaven I do humbly and heartily beſeech, to bleſs all theſe my children, whom he hath in his mercy ſo graciouſly beſtowed upon me, with long and religious lives, and that they may be fruitful in virtuous children and good works, and continue till their lives end loyal and dutiful ſubjects to the king's majeſty and his heirs, and approve themſelves good patriots and members to his commonwealth; which is the prayer and charge of me their father in the 67th year of my age, 1632.

My dear wife, the crown of all my happineſs, and mother of all my children, *Catharine* counteſs of *Corke*, was tranſlated at *Dublin* from this life into a better the 16th of February 1629-30, and was the 17th privately buried in the night in the upper end of the choir of St. *Patrick*'s church in *Dublin*, in the grave or vault, wherein Dr. *Weſton*, her grandfather, and good lord chancellor of *Ireland*, and Sir *Geoffry Fenton*, his majeſty's principal ſecretary of ſtate for this realm, were entomb'd. Her funerals were honourably ſolemnized in publick the 11th of March *Anno Domini* 1629. In the perpetual memory of which my virtuous and religious deceaſed wife, and of her predeceſſors and poſterity, I have cauſed a very fair tomb to be erected, with a cave or cellar of hewed ſtone underneath it.

I have purchaſed from the dean and chapter of St. *Patrick*'s church the inheritance of the upper part of the chancel, wherein the cave or cellar under ground is made, and wherein the tomb is built, to be a burying place for me and my poſterity, and their children."

This noble earl continued in great proſperity till the breaking out of the rebellion in *Ireland* in 1641; and the county of *Corke* was the laſt that ſuffered under the violences of the Papiſts, being the beſt inhabited with Engliſh of any part of that kingdom, by the plantations made by his lordſhip, and was in a great meaſure preſerved by his generoſity and diligence. He was then juſt returned out of *England*, and on this irruption immediately raiſed two troops of horſe, which he put under the command of his ſons, the lord viſcount *Kinelmeaky* and the lord *Broghill*, maintaining them and 400 foot for ſome months at his own charge (*m*); and at the battle of *Liſcarrol*, on the 3d of September 1642, wherein the Engliſh obtained a compleat victory, had no leſs than four of his ſons engaged, viz. his eldeſt ſon the lord viſcount *Dungarvan*, the lord viſcount *Kinelmeaky*, the lord *Broghill*, and Mr. *Francis Boyle*, afterwards created lord viſcount *Shannon*. But in this engagement he had the misfortune of loſing one of his ſons, the lord *Kinelmeaky*, governor of *Bandon*, who in ſeveral encounters had defeated the rebels, and with his father and brothers preſerved the county of *Corke* (*n*). At length this great man, in the midſt of theſe confuſions, departed this life at *Youghall*, and was interred there near the date (if not on the day) of

(*m*) Cox's Hiſtory of *Ireland*. Vol. II. p. 95.
(*n*) Ibid. p. 112.

of the ceſſation of arms concluded at *Sigginſtown*, Sept. 15, 1643 (*o*) *unwilling to ſurvive what he ſuſpected might not be auſpicious to the Engliſh, or conducible to the end for which it was deſigned*; *wherein he propheſied not ill* (*p*). Mr. *Borlaſe* tells us (*q*) that his lordſhip "was a perſon, for his abilities and knowledge in the affairs of the world, eminently obſervable, inaſmuch, as though he was no peer of *England*, he was admitted to ſit in the lords houſe upon the wool-ſacks, *ut conſiliarius*. And for all the eſtate he arrived at, (which was the greateſt in the memory of the laſt age) none ever taxed him with exorbitances, but ſuch as thought princes had too little, and religious men not enough." Another writer (*r*) likewiſe gives him this character, that he was "one of the moſt extraordinary perſons, either that or any other age hath produced, with reſpect to the great and juſt acquiſitions of eſtate that he made, and the publick works that he began and finiſhed, for the advancement of the Engliſh intereſt and the proteſtant religion in *Ireland*; as churches, alms-houſes, free-ſchools, bridges, caſtles, and towns, *viz. Liſmore*, *Tallow*, *Cloghmakilty*, *Iniſkeen*, *Caſtletown*, and *Bandon*, (which laſt place coſt him 14,000*l.*) inſomuch that when *Cromwell* ſaw theſe prodigious improvements, which he little expected to find in *Ireland*, he declared, that *if there had been an earl of* Corke *in every province, it would have been impoſſible for the Iriſh to have raiſed a rebellion.* And whilſt he was carrying on theſe ſolid works, he lived in his family at a rate of plenty, that exceeded thoſe who conſumed great eſtates in the laviſh ways of ill-ordered exceſs. His motto, *God's providence is my inheritance*, ſhews, from whence he derived all his bleſſings; the greateſt of which was the numerous and noble poſterity he had to leave his eſtate unto."

(*o*) *Borlaſe*, Reduction of *Ireland*. p. 209.

(*p*) Id. ibid.

(*q*) Ibid.

(*r*) *Cox*, Introduction to the 2d volume of his Hiſtory of *Ireland*.

ROBERT BOYLE, Eſq; his ſeventh and youngeſt ſon, and one of the greateſt ornaments of this noble family, as well as the age and country in which he lived, has left us ſome memoirs of the younger part of his life, drawn up ſoon after his return from his travels, which the reader will undoubtedly chooſe to read in his own words.

An Account of PHILARETUS, [i. e. Mr. R. BOYLE,] during his Minority.

NOT needleſsly to confound the herald with the hiſtorian, and begin a relation by a pedigree, I ſhall content myſelf to inform you, that the immediate parents of our *Philaretus* were, of the female ſex, [*Catherine*, daughter of Sir *Geoffry Fenton*] a woman, that wanted not beauty, and was rich in virtue, and on the father's ſide, that *Richard Boyle*, earl of *Corke*, who, by God's bleſſing on his proſperous induſtry, from very inconſiderable beginnings, built ſo plentiful and ſo eminent a fortune, that his proſperity has found many admirers, but few parallels.

HE was born the 14th child of his father (of which five women, and four men, do yet ſurvive) in the year 1626-7, upon St. *Paul*'s converſion day, at a country-houſe of his father's, called *Liſmore*, then one of the nobleſt ſeats and greateſt ornaments of the province of *Munſter*, in which it ſtood; but now ſo ruined by the ſad fate of war, that it ſerves only for an inſtance and a lecture of the inſtability of that happineſs, that is built upon the uncertain poſſeſſion of ſuch fleeting goods, as itſelf was.

To be ſuch parents ſon, and not their eldeſt, was a happineſs, that our *Philaretus* would mention with great expreſſions of gratitude; his birth ſo ſuiting his inclinations and deſigns, that, had he been permitted an election, his choice would ſcarce have altered God's aſſignment. For as on the one ſide, a lower birth would have too much expoſed him to the inconveniencies of a mean deſcent, which are too notorious to need ſpecifying; ſo on the other ſide, to a perſon, whoſe humour indiſpoſes him to the diſtracting hurry of the world, the being born heir to a great family is but a glittering kind of ſlavery, whilſt obliging him to a public entangled courſe of life, to ſupport the credit of his family, and tying him from ſatisfying his deareſt inclinations, it often forces him to build the advantages of his houſe upon the ruins of his own contentment.

A MAN of mean extraction is ſeldom admitted to the privacy and ſecrets of great ones promiſcuouſly, and ſcarce dares pretend to it, for fear of being cenſured ſaucy, or an intruder; and titular greatneſs is ever an impediment to the knowledge of many retired truths, that cannot be attained without familiarity with meaner perſons, and ſuch other condeſcenſions, as fond opinion, in great men, diſapproves and makes diſgraceful.

BUT now our *Philaretus* was born in a condition, that neither was high enough to prove a temptation to lazineſs, nor low enough to diſcourage him from aſpiring. And certainly to a perſon, that affected ſo much an univerſal knowledge, and arbitrary viciſſitudes of quiet and employments, it could not be unwelcome to be of a quality, that was a handſome ſtirrup to preferment, without an obligation to court it, and which might at once both protect his higher pretenſions from the guilt of ambition, and ſecure his retiredneſs from contempt.

WHEN once *Philaretus* was able, without danger, to ſupport the incommodities of a remove, his father, who had a perfect averſion for their fondneſs, who uſed to breed their children ſo nice and tenderly, that a hot ſun, or a good ſhower of rain, as much endangers them, as if they were made of butter, or of ſugar, ſends him away from home, and commits him to the care of a country nurſe, who by early inuring him, by ſlow degrees, to a coarſe but cleanly diet, and to the uſual paſſions of the air, gave him ſo vigorous a complexion, that both hardſhips were made eaſy to him by cuſtom, and the delights of conveniences and eaſe were endeared to him by their rarity.

SOME few years after this, two great diſaſters befel *Philaretus*; the one was the deceaſe of his mother, whoſe death would queſtionleſs have exceſſively afflicted him, had but his age permitted him to know the value of his loſs; for he would ever reckon it amongſt the chief misfortunes of his life, that he did never know her that gave it him, her free and noble ſpirit (which had a handſome manſion to reſide in) added to her kindneſs and ſweet carriage to her own, making her ſo hugely regretted by her children, and ſo lamented by her huſband, that not only he annually dedicated the day of her death to ſolemn mourning for it, but burying in her grave all thoughts of after marriage, he rejected all motions of any other match, continuing a conſtant widower till his death.

THE ſecond misfortune, that befel *Philaretus*, was his acquaintance with ſome children of his own age, whoſe ſtuttering habitude he ſo long counterfeited, that at laſt he contracted it; poſſibly a juſt judgment upon his deriſion, and turning the effects of God's anger into the ſubject matter of his ſport. Divers experiments, believed the probableſt means of cure, were tried with as much ſucceſsleſſneſs as diligence; ſo contagious and catching are men's faults, and ſo dangerous is the familiar commerce of thoſe condemnable cuſtoms, that being imitated but in jeſt, come to be learned and acquired in earneſt.

BUT to ſhow, that theſe afflictions made him not leſs the object of heaven's care, he much about this time eſcaped a danger, from which he owed his deliverance wholly to providence, being ſo far from contributing to it himſelf, that he did his endeavour to oppoſe it: for waiting on his father up to *Dublin*, there to expect the return of his eldeſt brother (then landed out of *England*, with his new wife, the earl of *Cumberland*'s heir) as they were to paſs over a brook, at that time ſuddenly by immoderate ſhowers ſwelled to a torrent, he was left alone in a coach, only with a foot-boy, where a gentleman of his father's very well horſed accidentally eſpying him, in ſpite of ſome others and his own unwillingneſs and reſiſtance, (they not believing his ſtay dangerous) carried him in arms over the rapid water, which proved ſo much beyond expectation both ſwift and deep, that horſes with their riders were violently hurried down the ſtream, which eaſily overturned the unloaded coach, the horſes (after by long ſtruggling they had broke their harneſs) with much ado ſaving themſelves by ſwimming.

As ſoon as his age made him capable of admitting inſtruction, his father by a Frenchman, and by one of his chaplains, had him taught both to write a fair hand, and to ſpeak French and Latin, in which, eſpecially the firſt, he proved no ill proficient, adding to a reaſonable forwardneſs in ſtudy a more than uſual inclination to it.

THIS ſtudiouſneſs obſerved in *Philaretus* endeared him very much unto his father, who uſed highly to commend him both for it and his veracity, of which latter he would often give him this teſtimony, that he never found him in a lie in all his lifetime. And indeed lying was a vice both ſo contrary to his nature, and ſo inconſiſtent with his principles, that as there was ſcarce any thing he more greedily deſired than to know the truth, ſo was there ſcarce any thing he more perfectly deteſted, than not to ſpeak it: which brings into my mind a fooliſh ſtory I have heard him jeered with by his ſiſter my lady *Ranelagh*, how ſhe having given ſtrict order to have a fruit-tree preſerved for his ſiſter-in-law, the lady *Dungarvan*, then big with child, he accidentally coming into the garden, and ignoring the prohibition, did eat half a ſcore of them, for which being chidden by his ſiſter *Ranelagh*, (for he was yet a child) and being told by way of aggravation, that he had eaten half a dozen plums, Nay truly, ſiſter, (anſwers he ſimply to her) I have eaten half a ſcore. So perfect an enemy was he to a lie, that he had rather accuſe himſelf of another fault, than be ſuſpected to be guilty of that. This trivial paſſage I have mentioned now, not that I think, that in itſelf it deſerves a relation, but becauſe as the ſun is ſeen beſt at his riſing and his ſetting, ſo men's native diſpoſitions are clearlieſt perceived, whilſt they are children, and when they are dying. And certainly theſe little ſudden accidents are the greateſt diſcoverers of mens true humours; for whilſt the inconſiderateneſs of the thing affords no temptation to diſſemble, and the ſuddenneſs of the time allows no leiſure to put diſguiſes on, men's diſpoſitions do appear in their true genuine ſhape, whereas moſt of thoſe actions, that are done before others, are ſo much done for others; I mean moſt ſolemn actions are ſo perſonated, that we may much more probably gueſs from thence, what men deſire to ſeem, than what they are; ſuch publick formal acts much rather being adjuſted to men's deſigns, than flowing from their inclinations.

Philaretus had now attained, and ſomewhat paſt the eighth year of his age, when his father (who ſupplied what he wanted in ſcholarſhip himſelf, by being both a paſſionate affector, and eminent patron of it) ambitious to improve his early ſtudiouſneſs, and conſidering, that great men's children breeding up at home tempts them to nicety, to pride, and idleneſs, and contributes much more to give them a good opinion of themſelves, than to make them deſerve it, reſolves to ſend over *Philaretus* in the company of Mr. *F. B.* his elder brother, to be bred up at *Eton* college near *Windſor*, whoſe provoſt at that time was Sir *Henry Wotton*, a perſon, that was not only a fine gentleman himſelf, but very well ſkilled in the art of making others ſo, betwixt whom and the earl of *Corke* an ancient friendſhip had been conſtantly cultivated by reciprocal civilities. To him therefore the good old earl recommends *Philaretus*, who having laid a week at *Youghall* for a wind, when he firſt put to ſea, was by a ſtorm beaten back again, not only a taſte, but an omen of his future fortune. But after eight days further ſtay, upon the ſecond ſummons of a promiſing gale, they went aboard once more, and (though the Iriſh coaſts were then ſufficiently infeſted with Turkiſh gallies) having by the way touched at *Ilford-combe*, and *Minehead*, at laſt they happily arrived at *Briſtol*.

Philaretus, in the company of his brother, after a ſhort ſtay to repoſe and refreſh themſelves at *Briſtol*, ſhaped his journey directly for *Eton* college, where a gentleman of his father's, ſent to convey them thither, departing, recommended him to the eſpecial care of Sir *Henry Wotton*, and left with him, partly to inſtruct, and partly to attend him, one *R. C.* one that wanted neither vices, nor cunning to diſſemble them; for though his primitive fault was only a dotage upon play, yet the exceſſive love of that goes ſeldom unattended with a train of criminal retainers; for fondneſs of gaming is the ſeducingeſt lure to ill company, and that the ſubtleſt pander to the worſt exceſſes. Wherefore our *Philaretus* deſervedly reckoned it, both amongſt the greateſt and the unlikelieſt deliverance he owed providence, that he was protected from the contagion of ſuch precedents; for though the man wanted not a competency of parts, yet perverted abilities make men but like thoſe wandering fires, philoſophers call *ignes fatui*, whoſe light ſerves not to direct, but to ſeduce the credulous traveller, and allure him to follow them in their deviations. And it is very true, that during the minority of judgment, imitation is the regent in the ſoul, and thoſe that are leaſt capable of reaſon, are moſt ſwayed by example. A blind man will ſuffer himſelf to be led, though by a dog, or a child.

NOT long our *Philaretus* ſtaid at ſchool, ere his maſter, Mr. *Harriſon*, taking notice of ſome aptneſs, and much willingneſs in him to learn, reſolved to improve them both by all the gentleſt ways of encouragement; for he would often diſpenſe from his attendance at ſchool at the accuſtomed hours, to inſtruct him privately and familiarly in his chamber. He would often as it were cloy him with fruit and ſweetmeats, and thoſe little dainties, that age is greedy of, that by preventing the want, he might leſſen both his value and deſire of them. He would ſometimes give him unaſked play-days, and oft beſtow upon him ſuch balls, and tops, and other implements of idleneſs, as he had taken away from others, that had unduly uſed them. He would ſometimes commend others before him, to rouſe his emulation, and oftentimes give him commendations before others, to engage his endeavours to deſerve them. Not to be tedious, he was careful to inſtruct him in ſuch an affable, kind, and gentle way, that he eaſily prevailed with him to conſider ſtudying, not ſo much as a duty of obedience to his ſuperiors, but as the way to purchaſe for himſelf a moſt delightſome and invaluable good. In effect, he ſoon created in *Philaretus* ſo ſtrong a paſſion to acquire knowledge, that what time he could ſpare from a ſcholar's taſk, which his retentive memory made him not find uneaſy, he would uſually employ ſo greedily in reading, that his maſter would ſometimes be neceſſitated to force him out to play, on which, and upon ſtudy, he looked, as if their natures were inverted. But that, which he related to be the firſt occaſion that made him ſo paſſionate a friend to reading, was the accidental peruſal of *Quintus Curtius*, which firſt made him in love with other than pedantick books, and conjured up in him that unſatisfied appetite of knowledge, that is yet as greedy, as when it firſt was raiſed. In gratitude to this book, I have heard him hyperbolically ſay, that not only he owed more to *Quintus Curtius*, than *Alexander* did; but derived more advantage from the hiſtory of that great monarch's conqueſts, than ever he did from the conqueſts themſelves.

Whilst our youth was thus buſied about his ſtudies, there happened to him an accident, that ſilence muſt not cover: for being one night gone to bed, ſomewhat early, whilſt his brother was converſing with ſome company by the fire-ſide, without giving them the leaſt warning or ſummons to take heed, a great part of the wall of their chamber, with the bed, chairs, books, and furniture of the next chamber over it, fell down, at unawares, upon their heads. His brother had his band torn about his neck, and his coat upon his back, and his chair cruſhed and broken under him; but by a luſty youth, then accidentally in the room, was ſnatched from out the ruins, by which *Philaretus* had, in all probability, been immediately oppreſſed, had not his bed been curtained by a watchful providence, which kept all heavy things from falling on it; but the duſt the crumbled rubbiſh raiſed was ſo thick, that he might there have been ſtifled, had not he remembered to wrap his head in the ſheet, which ſerved him as a ſtrainer, through which none but the purer air could find a paſſage. So ſudden a danger, and haſty an eſcape, *Philaretus* would ſometimes mention, with expreſſions both of gratitude and wonder. To which he would add the relation of divers other almoſt contemporary deliverances: of theſe, one was, that, being fallen from his horſe, the beaſt ran over him, and trod ſo near his throat, as within leſs than two inches of it to make a hole in his band, which he long after reſerved for a remembrancer. Another was, that riding through a town, upon a nag of his own, whoſe ſtarting quality he never obſerved before, his horſe, upon a ſudden fright, roſe bolt upright upon his hinder feet, and falling rudely backward with all his weight againſt a wall, had infallibly cruſhed his rider into pieces, if, by a ſtrange inſtinct, he had not caſt himſelf off at firſt, and was quit of it for a ſlight bruiſe. The laſt was, *Philaretus* being newly recovered of a flux, the doctor had preſcribed him a refreſhing drink: the fellow, that ſhould adminiſter it, inſtead of it, brings him a very ſtrong vomit, prepared and intended for another. *Philaretus* was that morning viſited by ſome of his ſchool-fellows, who (as he was not ill beloved amongſt them) preſented him with ſome ſweet-meats, which having eaten, when afterwards he would have eaten his breakfaſt, his ſtomach, whether out of ſqueamiſhneſs, or divination, forced him to render it again. To which lucky accident the phyſician aſcribed his eſcape from the apothecary's error; for in the abſence of thoſe that tended him, his phyſick caſt him into hideous torments, the true cauſe of which he never dreamed of, and remained long unconjectured, until the effects betrayed it; for after a long ſtruggling, at laſt the drink wrought with ſuch violence, that they feared that his life would be diſgorged together with his potion. This accident made him long after apprehend more from the phyſicians, than the diſeaſe, and was poſſibly the occaſion, that made him afterwards ſo inquiſitively apply himſelf to the ſtudy of phyſick, that he might have the leſs need of them, that profeſs it. But *Philaretus* would not aſcribe any of theſe reſcues unto chance, but would be ſtill induſtrious to perceive the hand of heaven in all theſe accidents; and indeed he would profeſs, that in the paſſages of his life, he had obſerved ſo gracious and ſo peculiar a conduct of providence, that he ſhould be equally blind and ungrateful, ſhould he not both diſcern and acknowledge it.

Philaretus having now for ſome two years been a conſtant reſident at *Eton* (if you except a few viſits, which, during the long vacations, he made his ſiſter my lady *Goring* at *Lewis* in *Suſſex)* when about *Eaſter* he was ſent for up to *London* to ſee his eldeſt brother the lord *Dungarvan*, where being viſited with a tertian ague, after the queen's and others doctors remedies had been ſucceſsleſsly aſſayed, at laſt he returned again to *Eton* to derive that health from a good air and diet, which phyſic could not give him. Here to divert his melancholy, they made him read the ſtate adventures of *Amadis de Gaule*, and other fabulous and wandering ſtories, which much more prejudiced him, by unſettling his thoughts, than they could have advantaged him, had they affected his recovery; for meeting in him with a reſtleſs fancy, then made more ſuſceptible of any impreſſions by an unemployed penſiveneſs, they accuſtomed his thoughts to ſuch a habitude of roving, that he has ſcarce ever been their quiet maſter ſince, but they would take all occaſions to ſteal away, and go a-gadding to objects then unſeaſonable and impertinent; ſo great an unhappineſs it is for perſons, that are born with ſuch buſy thoughts, not to have congruent objects propoſed to them at firſt. It is true, that long time after *Philaretus* did in a conſiderable meaſure fix his volatile fancy, and reclaim his thoughts, by the uſe of all thoſe expedients he thought likelieſt to fetter, or at leaſt to curb the roving wildneſs of his wandering thoughts. Amongſt all which the moſt effectual way he found to be the extraction of the ſquare and cube roots, and eſpecially thoſe more laborious operations of algebra, which both accuſtom and neceſſitate the mind to attention, by ſo entirely exacting the whole man, that the ſmalleſt diſtraction, or heedleſſneſs, conſtrains us to renew our trouble, and rebegin the operation. Six weeks had *Philaretus* been troubled with his ague, when he was freed from it by an accident, which is no ſlender inſtance of the force of imagination; for the phyſician having ſent him a purge, to give (as he ſaid) the fatal blow to the diſeaſe, our *Philaretus* had ſo perfect an averſion to all phyſic, and had newly aſſayed it ſo unſucceſsfully, that his complaints induced the maid ſervants of the houſe he lodged in (partly out of complaiſance to him, and partly out of a belief that phyſic did but exaſperate his diſeaſe) unknown to him, to pour out the potion, and fill the vial with ſyrup of ſtewed prunes, a liquor ſo reſembling it, that *Philaretus* (ſee the force of fancy!) ſwallowed it with the ſame reluctancy, and found the taſte as loathſome as if it had been the purge; but being after acquainted with the couſenage, whether it was that his ſickneſs (as having already reached its period) would have expired of itſelf, or that his mirth diſpatched it, I pretend not to determine; but certain it is, that from that hour to this, agues and he have ſtill been perfect ſtrangers, and he had much ado to refrain from laughter, when going to thank and reward the doctor for his recovery, he found it wholly aſcribed to the efficacy of the potion he had never ſwallowed, but in imagination.

HE had now ſerved well near half an apprenticeſhip at ſchool, when there arrived intelligence of his father's being landed in *England*, and gone to *Stalbridge*, a place in *Dorſetſhire* then newly purchaſed by him. Thither *Philaretus* accompanies his ſiſter the counteſs of *Kildare* to wait upon him. The good old earl welcomed him very kindly, for whether it were to the cuſtom of old people (as *Jacob* doted moſt on *Benjamin* and *Joſeph)* to give their eldeſt children the largeſt proportions of their fortunes, but the youngeſt the greateſt ſhare of their affections; to a likeneſs obſerved in *Philaretus*, both to his father's body and his mind; or, as it ſeems moſt likely, to his never having lived with his father to an age that might much tempt him to run in debt, and take ſuch other courſes to provoke his diſlike, as in his elder children he ſeverely diſreliſhed; to which if theſe cauſes the effect is to be aſcribed, it is not my taſk to reſolve, but certain it is, that from *Philaretus*'s birth, until his father's death, he ever continued very much his favourite. But after ſome weeks enjoyment of the ſummer diverſions at *Stalbridge*, when his father removed to *London*, he left him by the way at *Eton* college, from whence at his return in the weſt, ſome few months after, he took him abſolutely away, after *Philaretus* had ſpent in that ſchool (then very much thronged with young nobility) not much beneath four years, in the laſt of which he forgot much of that *Latin* he had got, for he was ſo addicted to more ſolid parts of knowledge, that he hated the ſtudy of bare words naturally, as ſomething that reliſhed too much of pedantry to conſort with his diſpoſition and deſigns; ſo that by the change of his old courteous ſchoolmaſter for a new rigid fellow, loſing thoſe encouragements that had formerly ſubdued his averſion to verbal ſtudies, he quickly quitted his Terence and his grammar, to read in hiſtory their gallant acts, that were the glory of their own, and the wonder of our times. And indeed it is a much nobler ambition to learn to do things, that may deſerve a room in hiſtory, than only to learn, how congruouſly to write ſuch actions in the gown-men's language.

As ſoon as *Philaretus* was arrived at *Stalbridge*, his father aſſigned the care of teaching him to one Mr. *W. Douch*, then parſon of that place, and one of his chaplains; and, to avoid the temptations to idleneſs, that home might afford, made him both lodge and diet where he was taught, though it were not diſtant from his father's houſe above twice a muſket-ſhot. This old divine inſtructing our youth both with care and civility, ſoon brought him to renew his firſt acquaintance with the Roman tongue, and to improve it ſo far, that in that language he could readily enough expreſs himſelf in proſe, and began to be no dull proficient in the poetic ſtrain; which latter he was naturally addicted to, reſenting a great deal of delight in the converſation of the Muſes, which nevertheleſs he ever ſince that time forbore to cultivate; not out of any diſlike or undervaluing of poetry, but becauſe in his travels having by diſcontinuance forgot much of the Latin tongue, he afterwards never could find time to redeem his loſſes by a ſerious ſtudy of the ancient poets. And then for Engliſh verſes, he ſaid, they could not be certain of a laſting applauſe, the changes of our language being ſo great and ſudden, that the rareſt poems within few years will paſs for obſolete; and therefore he uſed to liken the writers in Engliſh verſe to ladies, that have their pictures drawn with the clothes now worn, which, though at preſent never ſo rich, and never ſo much in faſhion, within a few years hence will make them look like anticks. Yet did he at idle hours write ſome few verſes, both in French and Latin, and many copies of amorous, merry, and devout ones in Engliſh; moſt of which, uncommunicated, the day he came of age, he ſacrificed to Vulcan, with a deſign to make the reſt periſh by the ſame fate, when they came within his power, though amongſt them were many ſerious copies, and one long one, amongſt the reſt, againſt wit profanely or wantonly employed; thoſe two vices being ever perfectly deteſted by him in others, and religiouſly declined in all his writings.

About this time alſo *Philaretus* began to be taught ſome ſkill in the muſic, both of the voice, and hand; but the diſcouragement of a bad voice quickly perſuaded him to deſiſt. It was now the ſpring of the year, when news was brought to *Stalbridge* of the approach of his ſiſter, the lady *Goring*, and in her company two of his brothers, the lords of *Kinelmeaky* and of *Broghill*, then newly returned from their three years travels. In their company arrived one Mr. *Marcombes*, a French gentleman, who had been their governor, and behaved himſelf ſo handſomely in that relation, that the old earl removed *Philaretus* (his brother lying ſick at a doctor's houſe) to his own houſe again, and intruſted his whole education with this gentleman. He was a man, whoſe garb, his mien, and outſide, had very much of his nation, having been divers years a traveller and a ſoldier; he was well faſhioned, and very well knew what belonged to a gentleman. His natural were much better than his acquired parts, though divers of the latter he poſſeſſed, though not in an eminent, yet in a very competent degree. Scholarſhip he wanted not, having in his greener years been a profeſſed ſtudent in divinity; but he was much leſs read in books than men, and hated pedantry as much as any of the ſeven deadly ſins. Thrifty he was extremely, and very ſkilful in the ſlights of thrift; but leſs out of avarice, than a juſt ambition, and not ſo much out of love to money, as a deſire to live handſomely at laſt. His practical ſentiments in divinity were moſt of them very ſound; and if he were given to any vice himſelf, he was careful by ſharply condemning it, to render it uninfectious, being induſtrious, whatſoever he were himſelf, to make his charges virtuous. Before company he was always very civil to his pupils, apt to eclipſe their failings, and ſet off their good qualities to the beſt advantage; but in his private converſation he was cynically diſpoſed, and a very nice critic both of words and men; which humour he uſed to exerciſe ſo freely with *Philaretus*, that at laſt he forced him to a very cautious and conſiderate way of expreſſing himſelf, which after turned to his no ſmall advantage. The worſt quality he had was his choler, to exceſſes of which he was exceſſively prone; and that being the only paſſion, to which *Philaretus* was much obſerved to be inclined, his deſire to ſhun claſhing with his governor, and his accuſtomedneſs to bear the ſudden ſallies of his impetuous humour, taught our youth ſo to ſubdue that paſſion in himſelf, that he was ſoon able to govern it habitually and with eaſe; the continuance of which conqueſt he much acknowledged to that paſſage of St. *James*, *For the wrath of man worketh not the righteouſneſs of God.* James i. 20. And he was ever a ſtrict obſerver of that precept of the apoſtle, *Let not the ſun go down upon your wrath*; for continued anger turns eaſily to malice; which made him, upon occaſion of this ſentence of St. *Paul*, to ſay, that anger was like the Jewiſh manna, which might be wholeſome for a day or two, but if it were kept long, it would breed worms, and corrupt. With this new governor our *Philaretus* ſpent the greateſt part of the ſummer, partly in reading and interpreting the *Univerſal Hiſtory*, written in Latin, and partly in familiar kind of converſation in French, which *Philaretus* found equally diverting and inſtructive, which was as well conſonant to the humour of his tutor as his own.

ABOUT this time his eldeſt brother, the lord of *Dungarvan*, having at his own charges raiſed a gallant troop of horſe for the king's ſervice in the Scotch expedition, his father ſent two other of his ſons, *Kinelmeaky* and *Broghill*, to accompany him in that ſervice, and deſigned *Philaretus* for the ſame employment; but the ſickneſs of his next elder brother, Mr. *F.* whom he was to go along with in that voyage, defeated all our young man's greedy hopes. During his ſtay at *Stalbridge* all that ſummer, his father, to oblige him to be temperate, by freely giving him the opportunity to be otherwiſe, truſted him with the keys of all his gardens and orchards. And indeed *Philaretus* was little given to greedineſs, either in fruits or ſweetmeats; in the latter he was almoſt abſtemious, and in the former he was very moderate; ſo valuing ſuch niceties and dainties, that though he enjoyed them with delight, he could want them without the leaſt regret. During this pleaſing ſeaſon, when the intermiſſion of his ſtudies allowed *Philaretus* leiſure for recreations, he would very often ſteal away from all company, and ſpend four or five hours alone in the fields, and think at random, making his delighted imagination the buſy ſcene, where ſome romance or other was daily acted; which, though imputed to his melancholy, was in effect but an uſual excurſion of his yet untamed habitude of roving, a cuſtom (as his own experience often and ſadly taught him) much more eaſily contracted, than deſtroyed.

Towards the end of this ſummer, the kingdom having now obtained a ſeeming ſettlement by the king's pacification with the Scots, there arrived at *Stalbridge* Sir *Thomas Stafford*, gentleman uſher to the queen, with his lady, to viſit their old friend, the earl of *Cork*, with whom, ere they departed, they concluded a match betwixt his fourth ſon, Mr. *F. B.* and *E. K.* [*Killigrew*] daughter to my lady *S.* by Sir — *K.* and then a maid of honour, both young and handſome. To make his addreſſes to this lady, Mr. *F.* was ſent (and *Philaretus* in his company) before up to *London*, whither within few weeks they were followed by the earl and his family, of which a great part lived at (the lady *Stafford*'s houſe) the *Savoy*; the reſt (for his family was much encreaſed by the acceſſion of his daughters, the counteſs of *Barrimore* and the lady *Ranelagh*, with their lords and children) were lodged in the adjacent houſes, but took their meals in the *Savoy*, where the old earl kept ſo plentiful a houſe, that in months his accompts for bare houſe-keeping exceeded pounds.

Not long after his arrival, *Philaretus*'s brother having been ſucceſsful in his addreſſes to his miſtreſs, was, in the preſence of the king and queen, publickly married at court, with all that ſolemnity that uſually attends matches with maids of honour. But to render this joy as ſhort as it was great, *Philaretus* and his brother were within four days after commanded away for *France*, and after having kiſſed their majeſty's hands, they took a differing farewel of all their friends; the bridegroom extremely afflicted to be ſo ſoon deprived of a joy, which he had taſted but juſt enough of to encreaſe his regrets, by the knowledge of what he was forced from; but *Philaretus* as much ſatisfied to ſee himſelf in a condition to content a curioſity, to which his inclinations did paſſionately addict him. With theſe differing reſentments of their father's commands, accompanied by their governor, two French ſervants, and a lacquey of the ſame country, upon the end of *October*, 1638, they took poſt for *Rye* in *Suſſex*, where the next day hiring a ſhip, though the ſea were not very ſmooth, a proſperous puff of wind did ſafely by the next morning blow them into *France*.

After a ſhort refreſhment at *Dieppe*, they travelled through *Normandy* to the chief city of it, *Rouën*; but by the way received advertiſement of a robbery freſhly committed in a wood, they muſt traverſe by night; but judging the fear of being apprehended would deter the robbers from a ſudden return to the ſame place, after ſo recent a crime, the company quietly continued on their journey to *Rouën*, and arrived ſafely at it; where, amongſt other ſingularities, *Philaretus* took much notice of a great floating bridge, which riſing and falling as the tide-water does, he uſed to reſemble to the vain amoriſts of outward greatneſs, whoſe ſpirits reſent all the floods and ebbs of that fortune it is built on. From *Rouën* they paſſed to *Paris*, and having ſpent ſome time in viſiting that vaſt chaos of a city, they ſhaped their courſe for *Lyons*, where, after nine days unintermitted travel, they arrived, having by the way (beſides divers conſiderable places) paſſed by the town of *Moulins* (here famed enough for the fine tweezes it ſupplies us with) a part of the French *Arcadia*, the pleaſant *Pays de Foreſt*, where the marquis *d'Urfé* was pleaſed to lay the ſcene of the adventures and amours of that *Aſtrea*, with whom ſo many gallants are ſtill in love, ſo long after both his and her deceaſe: being alſo by the way uſefully diverted by the company of two Polonian princes, who had as well a right unto that title by their virtue and their education, as their birth.

AFTER ſome ſtay at *Lyons* (a town of great reſort, and no leſs trading, but fitter for the reſidence of merchants than of gentlemen) they traverſed thoſe lofty mountains, that formerly belonging to the duke of *Savoy*, were now by ſtipulation (in exchange of the marquiſate of *Saluzzo)* devolved to the French king; and having by the way beheld that famous place, where the ſwifteſt and one of the nobleſt rivers of *Europe*, the *Rhoſne*, is ſo ſtreightened betwixt two neighbour rocks, that it is no ſuch large ſtride to ſtand on both his banks, the third day after their departure from *Lyons*, they arrived ſafely at *Geneva*, a little commonwealth, which their early embracing and conſtant profeſſion of the reformed religion, together with that peculiar care of providence, in ſo long and ſo unlikely a preſervation, has rendered very much the theme, not only of diſcourſe, but ſome degree of wonder.

Philaretus's inſtructions commanding him a long ſtay in this place, his governor (having both a wife and children in the town) provided him lodgings and entertainment in his own houſe, and at ſet hours taught him both rhetoric and logic, whoſe elements (not the expoſitions) *Philaretus* wrote out with his own hand; though afterwards he eſteemed both thoſe arts, as they are vulgarly handled, not only unſeaſonably taught, but obnoxious to thoſe inconveniences, and guilty of thoſe defects, he does fully particularize in his *Eſſays*. After theſe ſlighter ſtudies, he fell to learn the mathematics, and in a few months grew very well acquainted with the moſt uſeful part of arithmetic, geometry, with its ſubordinates, the doctrine of the ſphere, that of the globe, and fortification; in all which being inſtructed by a perſon that had a greater regard for his ſcholar's proficiency, than the gains he might derive from the common tedious and dilatory way of teaching, he quickly grew ſo enamoured of thoſe delightful ſtudies, that they very often proved both his buſineſs and his diverſion in his travels, and he afterwards improved his opportunities to the attainment of a more than ordinary ſkill in divers of them. He alſo frequently converſed with a voluminous, but excellent French book, called, *Le Monde*, which ſo judiciouſly informs its readers, both of the paſt and preſent condition of all thoſe ſtates that now poſſeſs our globe, and by a delectable and inſtructive variety not only ſatisfies men's curioſity, but ſo copiouſly ſupplies them with matter, both of curious and ſerious diſcourſe, that he uſed to ſay of that book, that it was worth its title, which means, *The World*.

BUT to employ his body, as well as his mind, becauſe *Philaretus*'s age was yet unripe for ſo rude and violent an exerciſe as the great horſe, he ſpent ſome months in fencing, and ten or twelve in learning to dance, the former of which exerciſes he ever as much affected, as he contemned the latter. His recreations, during his ſtay at *Geneva*, were ſometimes mall, tennis (a ſport he ever paſſionately loved) and, above all, the reading of romances, whoſe peruſal did not only extremely divert him, but (aſſiſted by a total diſcontinuance of the Engliſh tongue) in a ſhort time taught him a ſkill in French ſomewhat unuſual to ſtrangers. In effect, before he quitted France, he attained a readineſs in the language of that country, which enabled him, when he made concealment his deſign, to paſs for a native of it, both amongſt them that were ſo, and amongſt foreigners alſo; and in all his writings, whilſt he was abroad, he ſtill made uſe of the French tongue, not out of any intention to improve his knowledge in it, but becauſe it was that he could expreſs himſelf beſt in.

BUT during *Philaretus*'s reſidence at *Geneva*, there happened to him an accident, which he always uſed to mention as the conſiderableſt of his whole life. To frame a right apprehenſion of this, you muſt underſtand, that though his inclinations were ever virtuous, and his life free from ſcandal and inoffenſive, yet had the piety he was maſter of already ſo diverted him from aſpiring unto more, that Chriſt, who long had lain aſleep in his conſcience (as he once did in the ſhip) muſt now, as then, be waked by a ſtorm. For at a time, which (being the very heat of ſummer) promiſed nothing leſs, about the dead of night, that adds moſt terror to ſuch accidents, *Philaretus* was ſuddenly waked in a fright with ſuch loud claps of thunder (which are oftentimes very terrible in thoſe hot climes and ſeaſons) that he thought the earth would owe an ague to the air, and every clap was both preceded and attended with flaſhes of lightning ſo frequent and ſo dazzling, that *Philaretus* began to imagine them the ſallies of that fire that muſt conſume the world. The long continuance of that diſmal tempeſt, where the winds were ſo loud, as almoſt drowned the noiſe of the very thunder, and the ſhowers ſo hideous, as almoſt quenched the lightning, ere it could reach his eyes, confirmed *Philaretus* in his apprehenſions of the day of judgment's being at hand. Whereupon the conſideration of his unpreparedneſs to welcome it, and the hideouſneſs of being ſurpriſed by it in an unfit condition, made him reſolve and vow, that if his fears were that night diſappointed, all his further additions to his life ſhould be more religiouſly and watchfully employed. The morning came, and a ſerener cloudleſs ſky returned, when he ratified his determination ſo ſolemnly, that from that day he dated his converſion, renewing, now he was paſt danger, the vow he had made, whilſt he believed himſelf to be in it; that though his fear was (and he bluſhed it was ſo) the occaſion of his reſolution of amendment, yet at leaſt he might not owe his more deliberate conſecration of himſelf to piety to any leſs noble motive, than that of its own excellence.

THUS had this happy ſtorm an operation upon *Philaretus*, reſembling that it had upon the ground; for the thunder did but terrify, and blaſted not; but with it fell ſuch kind and genial ſhowers, as watered his parched and almoſt withered graces, and reviving their greenneſs, ſoon rendered them both flouriſhing and fruitful. And though his boiling youth did often very earneſtly ſolicit to be employed in thoſe culpable delights, that are uſeful in, and ſeem ſo proper for that ſeaſon, and have repentance adjourned till old age; yet did its importunities meet ever with denials, *Philaretus* ever eſteeming, that piety was to be embraced, not ſo much to gain heaven, as to ſerve God with. And I remember, that being once in company with a crew of mad young fellows, when one of them was ſaying to him, what a fine thing it were, if men could ſin ſecurely all their life-time, by being ſure of leiſure to repent upon their death-beds; *Philaretus* preſently replied, that truly for his part he ſhould not like ſinning, though on thoſe terms, and would not all that while deprive himſelf of the ſatisfaction of ſerving God, to enjoy ſo many years fruition of the world. In effect it is ſtrange, that men ſhould take it for an inducement to an action, that they are confident that they ſhall repent of it. But *Philaretus* himſelf having ſufficiently diſcourſed that point of early piety, in the ſixth treatiſe of his *Chriſtian Gentleman*, I ſhall at preſent only add to the arguments you may find there alledged, that he uſed to ſay, that it was a kind of meanneſs in devotion, to conſider the very joys of the other life more as a condition, than a recompenſe. But (as when in ſummer we take up our graſs-horſes into the ſtable, and give them ſtore of oats, it is a ſign that we mean to travel them) our *P.* ſoon after he had received this new ſtrength, found a new weight to ſupport; for ſpending ſome of the ſpring in a viſit to *Chambery*, the chief town of *Savoy*; *Aix* famed for its baths; *Grenoble*, the head town of *Dauphiné*, and reſidence of a parliament, his curioſity at laſt led him to thoſe wild mountains, where the firſt and chiefeſt of the Carthuſian abbies does ſtand ſeated; where the devil taking advantage of that deep raving melancholy, ſo ſad a place, his humour, and the ſtrange ſtories and pictures he found there of *Bruno*, the father of that order, ſuggeſted ſuch ſtrange and hideous thoughts, and ſuch diſtracting doubts of ſome of the fundamentals of Chriſtianity, that, though his looks did little betray his thoughts, nothing but the forbiddenneſs of ſelf-diſpatch hindered his acting it. But after a tedious languiſhment of many months in this tedious perplexity, at laſt it pleaſed God, one day he had received the ſacrament, to reſtore unto him the withdrawn ſenſe of his favour. But though ſince then *Philaretus* ever looked upon theſe impious ſuggeſtions, rather as temptations to be ſuppreſſed, than doubts to be reſolved; yet never after did theſe fleeting clouds ceaſe now and then to darken the cleareſt ſerenity of his quiet, which made him often ſay, that injections of this nature were ſuch a diſeaſe to his faith, as the tooth-ach is to the body; for though it be not mortal, it is very troubleſome. And however, as all things work together to them that love God, *Philaretus* derived from this anxiety the advantage of groundedneſs in his religion: for the perplexity his doubts created obliged him, to remove them, to be ſeriouſly inquiſitive of the truth of the very fundamentals of Chriſtianity, and to hear what both Turks, and Jews, and the chief ſects of Chriſtians could alledge for their ſeveral opinions; that ſo, though he believed more than he could comprehend, he might not believe more than he could prove, and not owe the ſtedfaſtneſs of his faith to ſo poor a cauſe, as the ignorance of what might be objected againſt it. He ſaid (ſpeaking of thoſe perſons, that want not means to enquire, and abilities to judge) that it was not a greater happineſs to inherit a good religion, than it was a fault to have it only by inheritance, and think it the beſt, becauſe it is generally embraced, rather than embrace it, becauſe we know it to be the beſt. That though we cannot always give a reaſon for what we believe, we ſhould be ever able to give a reaſon why we believe it. That it is the greateſt of follies to neglect any diligence, that may prevent the being miſtaken, where it is the greateſt of miſeries to be deceived. That how dear ſoever things taken up on the ſcore are ſold, there is nothing worſe taken up upon truſt than religion, in which he deſerves not to meet with the true one, that cares not to examine whether or no it be ſo.

AND now *Philaretus* having ſpent one and twenty months in *Geneva*, about the middle of September 1641 departed towards *Italy*, and having traverſed *Swizerland*, and by the way ſeen *Lauſanne* (an academy ſeated upon the great Genevan lake) *Zurich*, and *Soleurre*, the heads of Cantons wearing the ſame name, and wandered ſeven or eight days amongſt thoſe hideous mountains, where the *Rhoſne* takes it ſource, and he ſaw the *Rhine* but a brook; he at length arrived at the *Valtollina*, a ſpacious valley walled round with the ſteep *Alpes*, but ſo delicious, and (eſpecially in that ſeaſon) ſo crowned with all that *Ceres* and *Bacchus* are able to preſent, that it deſerved to be the motive, but not the ſtage of thoſe late wars it has occaſioned betwixt the rival crowns of *France* and *Spain*. There *Philaretus* had the curioſity to viſit the place on which ſtood *Piur*, a pleaſant little town, once eſteemed for its deliciouſneſs, but now much more and more meritedly famous for its ruin, which happened ſome two dozen of years ſince, by the ſudden and unexpected fall of a neighbouring hill upon it, which ſtruck the whole town ſo deep into the ground, that no after-ſearch by digging has ever prevailed to reach it. Having viſited the ſingularities of this earthly paradiſe, *Philaretus* and his company began to climb that mountain of the *Alpes*, which denominated from a town, that is ſeated upon its foot, is uſually called, *La montagna di Morbegno*. The hill was eight miles in aſcent, and double that number downwards. It was then free from ſnow; but all the neighbouring hills, where ſtore of cryſtal is digged, like perpetual penitents, do all the year wear white. Upon the top of this hill, which is entirely uninhabited, *Philaretus* had the pleaſure to ſee the clouds, which they paſſed through in their deſcent, beneath them darkening the middle of the mountain, while on the top they had a clear ſerenity. But notwithſtanding the fairneſs of the day, they ſpent it all in traverſing this hill, at the height of which they left the *Griſons* territory, and at the bottom entered a village belonging to that of the *Venetians*; but having paſſed through ſuch a purgatory as the *Alpes* to their Italian paradiſe, I cannot but ſuppoſe them ſomewhat weary, and ſo my pen obliged to let them and itſelf take ſome ſhort reſt.

Philaretus being thus entered into the vaſt and delicious plains of *Lombardy*, traverſed the greateſt part of that rich province, and having ſtayed the ſtomach of his curioſity with the obſervables of *Bergamo*, *Breſcia*, *Verona*, *Vincenza*, and *Padua* (a famous univerſity, but more peculiarly devoted to *Æſculapius* than *Minerva*'s arts) he gave it a full meal at *Venice*, where the great concourſe of foreign nations numerouſly reſorting thither for trade or nobler buſineſs, preſents the ſenſes with a no leſs pleaſing than conſtant variety. From *Venice* returning through *Padua*, and paſſing through *Bologna* and *Ferrara* (towns, whoſe names allow me to ſpare their characters) he at laſt arrived at *Florence*, with a determination (having diſpoſed of the horſes he rode on from *Geneva* thither) to paſs the winter there. *Florence* is a city, to which nature has not grudged a pleaſing ſituation, and in which architecture has been no niggard either of coſt or ſkill, but has ſo induſtriouſly and ſumptuouſly improved the advantages liberally conferred by nature, that both the ſeat and buildings of the town abundantly juſtify the title the *Italians* have given it of *Fair*. Here *Philaretus* ſpent much of his time in learning of his governor (who ſpake it perfectly) the Italian tongue, in which he quickly attained a native accent, and knowledge enough to underſtand both books and men; but to ſpeak and expreſs himſelf readily in that language was a ſkill he ever too little aſpired to acquire. The reſt of his ſpare hours he ſpent in reading the modern hiſtory in Italian, and the new paradoxes of the great ſtar-gazer *Galileo*, whoſe ingenious books, perhaps becauſe they could not be ſo otherwiſe, were confuted by a decree from *Rome*; his highneſs the Pope, it ſeems, preſuming, and that juſtly, that the infallibility of his chair extended equally to determine points in philoſophy as in religion, and loth to have the ſtability of that earth queſtioned, in which he had eſtabliſhed his kingdom. Whilſt *Philaretus* lived at *Florence*, this famous *Galileo* died * within a league of it, his memory being honoured with a celebrating epitaph, and a fair tomb erected for him at the public charges; but before his death being long grown blind, to certain friers (a tribe, whom for their vices and impoſtures he long had hated) that reproached him with his blindneſs, as a juſt puniſhment of heaven, incenſed for being ſo narrowly pried into by him, he anſwered, that he had the ſatisfaction of not being blind, till he had ſeen in heaven what never mortal eyes beheld before. But to return to *Philaretus*, the company of certain Jewiſh rabbins, who lodged under the ſame roof with him, gave him the opportunity of acquainting himſelf with divers of their arguments and tenets, and a riſe of further diſquiſitions in that point. When carnaval was come (the ſeaſon, when madneſs is ſo general in *Italy*, that lunacy does for that time loſe its name) he had the pleaſure to ſee the tilts maintained by the Great Duke's brothers, and to be preſent at the gentlemen's balls. Nor did he ſometimes ſcruple, in his governor's company, to viſit the famouſeſt *Bordellos*, whither reſorting out of bare curioſity, he retained there an unblemiſhed chaſtity, and ſtill returned thence as honeſt as he went thither, profeſſing, that he never found any ſuch ſermons againſt them, as they were againſt themſelves; the impudent nakedneſs of vice clothing it with a deformity deſcription cannot reach, and the worſt of epithets cannot but flatter. But though *Philaretus* were no fuel for forbidden flames, he proved the object of unnatural ones; for being at that time in the flower of youth, and the cares of the world having not yet faded a complexion naturally freſh enough, as he was once unaccompanied diverting himſelf abroad, he was ſomewhat rudely preſſed by the prepoſterous courtſhip of two friers, whoſe luſt makes no diſtinction of ſexes, but that which its preference of their own creates, and not without difficulty and danger forced a ſcape from thoſe gowned Sodomites, whoſe goatiſh heats ſerved not a little to arm *Philaretus* againſt ſuch people's ſpecious hypocriſy, and heightened and fortified in him an averſeneſs for opinions, which now the religieux diſcredit as well as the religion.

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* He died January the 8th, 1641-2, N. S. See *General Dictionary*, Vol. V. p. 373. in the article of *Galileo*.

Philaretus having thus ſpent the winter in *Florence*, towards the end of March began his journey to *Rome*; and having paſſed thorough and ſeen the ſingularities of *Sienna*, *Monteſiaſcone*, and ſome other remarkable places in his paſſage, at the end of five days he ſafely arrived at that imperious theme of fame, which deſtinated to ſome kind or other of univerſal monarchy, is now no leſs conſiderable by its preſent ſuperſtition, than formerly by its victorious arms; the modern Popes bringing it as high a veneration as the ancient Cæſars, and the Barberine bees flying as far as did the Roman eagles. The more conveniently to ſee the numerous rarities of this univerſal city, *Philaretus*, to decline the diſtracting intruſions and importunities of Engliſh Jeſuits, paſſed for a Frenchman, which neither his habit nor language much contradicted. Under this notion he delightfully payed his viſits to what in *Rome* and the adjacent villages moſt deſerved them, and amongſt other curioſities and antiquities had the fortune to ſee the Pope at chapel, with the Cardinals, who ſeverally appearing mighty princes, in that aſſembly looked like a company of common friers. Here *Philaretus* could not chuſe but ſmile to ſee a young churchman, after the ſervice ended, upon his knees carefully with his feet ſweep into his handkerchief the duſt his holineſs's gouty feet had by treading on it conſecrated, as if it had been ſome miraculous relic. Nor was *Philaretus* negligent to procure the Latin and Tuſcan poems of this Pope, whoſe name *Urbanus* his actions did not belie, he having more of the gentleman in him, than his pontifical habit would ſeem to let him wear. A poet he was, and a * * * * *

* * * HE never found the Pope leſs valued than in *Rome*, nor his religion fiercelier diſputed againſt than in *Italy*; and ſometimes added, that he ceaſed to wonder, that the Pope ſhould forbid the ſight of *Rome* to proteſtants, ſince nothing could more confirm them in their religion.

Philaretus having in a ſhort time ſurveyed the principal rarities of this proud miſtreſs of the world, was unwillingly driven thence by his brother's diſability to ſupport the encreaſing heats, which there prove often inſupportable to ſtrangers, the neighbouring country being very ſcorched, and barren, and uninhabited. Wherefore he took his way back towards *Florence*, by that delicious valley that ennobles *Perugia*, and paſſing by *Piſtoia* came to *Florence*, where, after a ſhort repoſe, they deſcended the river *Arno* into *Piſa*, and from thence to *Livorno*, where in a Felucca, with a good wind, they ventured, for expedition ſake, ſome fifteen or ſixteen miles into the ſea, and coaſting along the country, ſtill near the ſhore for fear of ſudden ſtorms, they each night lay in ſome town, drawing their boat aſhore (which was not uneaſy in regard of the inconſiderable tides of the Mediterranean there) and ſoon, though not without danger, reached proud *Genoa.* * * * * *

* * * * * The next day *Philaretus* proſecuted his journey, and paſſing by *Monaco* (a very ſtrong place, then newly betrayed by the prince to the French) and by *Mentone*, a little principality belonging to the ſame prince, and ſtopping a while at *Nizza*, a place extremely and meritoriouſly famous for that ſtrength, which nature and art have emulouſly given it, by night they landed at *Antibe*, one of the towns of *France*, that moſt approaches *Italy*.

The morning, that ſucceeded *Philaretus*'s arrival at *Antibe*, he left it to croſs the country to *Marſeilles*, but he was welcomed into *France*, by an accident, which was very hazardous, and might have proved tragical. During the whole time of his being a traveller, or reſident in *Italy*, *Philaretus* had religiouſly adhered to his * * * *

* * * * English gentlemen thinking it as much better as ſafer to take off their hats, than to venture their heads, complimented with the crucifix; but *Philaretus*, without the leaſt act of ſuperſtition, though not without ill words, and worſe menaces, ventured and paſt boldly thorough them all, as ever reſolving, that the ſoul ſhould not more tranſcend the body in its own value, than in his eſteem. This danger thus happily eſcaped, *Philaretus* continues his way to *Marſeilles*, where the third day he arrived, with intent there to expect bills of exchange promiſed to be then ſent thither, to enable him to proſecute his future travels. His detention here was ſhortened by his viſits of ſo excellent a harbour for gallies and ſmall veſſels. * * * Town every night like it aſſured with lock and key. Here *Philaretus* had the pleaſure to ſee the French king's fleet of gallies put to ſea, and about two thouſand poor ſlaves tug at the oar to row them.

Here unfortunately Mr. *Boyle*'s manuſcript memoirs break off abruptly, which I ſhall endeavour to continue from ſuch lights as his other papers afford me.

During his travels, he purſued his ſtudies with great vigour; and his brother *Francis*, afterwards lord *Shannon*, uſed to ſay, that even then he would never loſe any vacant time; for if they were upon the road, and walking down a hill, or in a rough way, he would read all the way; and when they came at night to their inn, he would ſtill be ſtudying till ſupper, and frequently propoſe ſuch difficulties, as he met with in his reading, to his governor (*s*).

In *Italy* he read over the lives of the ancient philoſophers with the utmoſt attention. The ſect, which then ſtruck him moſt, was that of the Stoics; and he tried his proficiency in their philoſophy, by enduring a long fit of the tooth-ach with great unconcernedneſs (*t*). At *Marſeilles*, he and his brother *Shannon* were obliged to wait for bills of exchange, in order to proſecute their journey; but, inſtead of that ſupply, they received from their father, in May 1642, a ſad relation of the general rebellion in *Ireland*, with this account, that it was with great difficulty he had procured for them two hundred and fifty pounds, to ſupply their expences in their return home. But this money being intruſted with one *Perkins*, a citizen of *London*, who was eſteemed a man of conſiderable ſubſtance, to be ſent them in bills of exchange, he proved unfaithful, ſo that they never received the leaſt part of it. Being thus left deſtitute in a ſtrange country, ſeven or eight hundred miles from home, they were by means of Mr. *Marcombes* their governor brought to *Geneva*, till ſupplies could be received to enable them to return; where, by reaſon of the dreadful confuſion of affairs in *Ireland* and *England*, they fruitleſsly waited for about two years; during all

(*s*) Dr. *Dent*'s letter to Dr. *W. Wotton*, dated from *Weſtminſter Abbey*, May 20, 1699.

(*t*) Mr. *Boyle*'s memorandums of his own life, dictated by himſelf to biſhop *Burnet*.

which time having no money, either to diſcharge Mr. *Marcombes*, or to return homewards, they were obliged to live at a great expence, and to run in debt to him on that account. " At length, adds Mr. *Boyle* (u), though I could not receive any ſupply at " all from my friends, yet being unable to ſubſiſt any longer, I was forced to remove " thence; and having upon his [Mr. *Marcombes*'s] credit made ſhift to take up ſome " ſlight jewels at a reaſonable rate, we made ſale of them from place to place, and " by their help, at laſt, by God's aſſiſtance, we got ſafe into *England* towards the " middle of the year 1644, where we found things in ſuch confuſion, that although " the manor of *Stalbridge* were, by my father's deceaſe, deſcended unto me, yet it " was near four months before I could get thither."

UPON his return to *England*, which was unexpected to all his relations, he went immediately to his beloved ſiſter *Catharine*, viſcounteſs *Ranelagh*, a lady remarkable for her uncommon genius and knowledge, in whoſe houſe he lodged four months and an half. It was by accident, that he found her out, but ſuch an accident, as he frequently afterwards, with great thankfulneſs to the providence of God, uſed to aſcribe a good part of his future happineſs to. For had he not been ſo detained, he had gone into the army, where though there were, beſides " the excellent king himſelf, " divers eminent divines, and many worthy perſons of ſeveral ranks; yet the gene- " rality of thoſe he would have been obliged to converſe with, were very debauched, " and apt, as well as inclinable, to make others ſo (x);" whereas by ſtaying in *London* in a family that was ſtrictly religious, his early years were ſeaſoned with ſuch principles of piety and virtue, as never forſook him during the reſt of his life. He reaped alſo a collateral advantage by it, which was, that a ſiſter-in-law of lady *Ranelagh*, who was with them in the houſe, and was wife of one of the principal members of the then houſe of commons, brought him into the acquaintance and friendſhip of ſome great men of that party, which was then growing, and ſoon after victorious; by whoſe means he got early protection for his Engliſh and Iriſh eſtates (y). However it is probable from a letter of his to his brother lord *Broghill*, dated from *London*, Auguſt 25th 1645, which begins thus, " The neceſſities of my affairs " calling me away (according to the leave the parliament has given me) into *France*," that he went abroad again, poſſibly to ſettle his accounts with Mr. *Marcombes*, who, as is already obſerved, had advanced money, and engaged his credit for him. But he did not continue long out of *England*; for he was at *Cambridge* for a ſhort time in December following. In March 1645-6, he retired to his manor at *Stalbridge*; where he reſided for the moſt part till May 1650; and upon his arrival there wrote the following letter to his ſiſter, the lady *Ranelagh*, which will ſhew his talent in the epiſtolary ſtyle, even at that early age.

" SISTER,

" IF the buſy idleneſs of receiving ſenſeleſs viſits (whoſe continuance, if otherwiſe " unavoidable, were capable, in my opinion, to juſtify the retiredneſs of an " hermit) had not ſo totally taken up my leiſure, you ſhould not ſo long have had a " reprieve from the importunity of my letters. But now at laſt, to make you amends " for my fault (if at leaſt the amends itſelf be not a fault) I will preſent you with a " piece of a real romance in the ſtory of my peregrination hither. The morning I " had the unhappineſs to take my leave of you and my lady *Molkin*, I bid farewel " to the city, and began my journey upon a courſer. Him I rid to dinner to *Egham*; " and at the end of the town, there it was my good fortune (as we are pleaſed to " miſcall it) to overtake an expreſs ſent from the parliament to the general, making

(u) Letter to the ſteward of his manor of *Stalbridge* in *Dorſetſhire*.

(x) Papers of Mr. *Boyle*'s life, delivered by him to biſhop *Burnet*.

(y) Ibid.

“ ceremonies with his horſe, whether of them two ſhould lead the way. I quickly
“ put an end to their difference, by making *Roger* take the honour upon him; and
“ we had not rid far, before we met with ſome carriers, that had lately left *Andover*,
“ who aſſured us, that the way was very ſecure; though preſently after we came to
“ diſcover, that thoſe youngſters would not have been very ſorry, that we had been
“ ſnapped by ſome of their party, and been made to take *Oxford* in our way to *Stalbridge*;
“ for we were no ſooner come to *Bagſhot*, but we met a carrier coming the
“ way we were going, who aſſured us, that the cavaliers had eaſed them at *Baſingſtoke*
“ of all their ſuperfluous moveables, and were advanced as far as *Hartley row*,
“ the very village that I had deſigned for that night's lodging. So believing, that
“ our quarters would be already taken up, we called a council of war to adviſe and
“ reſolve what to do. It was *Roger*'s opinion, to ſtay where we were, till the paſſage
“ were made ſecure (ſtrange, that ſo well-armed an head ſhould be fearful!) but the
“ meſſenger eaſily perſuaded us to go to *Farnham* (though both that and all the
“ country in the way were ſmartly alarmed) aſſuring *Roger*, that in caſe of danger he
“ could ſtrike into a way, out of the way, to bring us to the place we went to. As
“ we went along, we met divers little parties, with whom we exchanged fears, and
“ found, that the malignant humours, that were then abroad, had frighted the
“ country into a ſhaking ague, till we came to *Farnham*, which we found empty and
“ unguarded, all the townſmen being gone out to oppoſe the king's party, and chuſing
“ rather to have their houſes empty, than repleniſhed with ſuch gueſts, as otherwiſe
“ they were neceſſarily to expect. There invited by the coolneſs of the evening,
“ and the freſhneſs of the garden I was walking in, I almoſt loſt myſelf in meditating,
“ how fooliſhly raſh were our controlments of, and repinings at the wiſe contrivances
“ of that all-ſwaying providence, whoſe proceedings ſhould be as far above
“ our cenſures, as they are above our reach. How apt are we, upon the leaſt thwarting
“ of our deſigns, to murmur againſt providence, and in a pettiſh humour throw
“ the helve after the hatchet, *le manche apres la coignée*, and give ourſelves for gone,
“ when, had we had our will in the means, we ſhould have been fruſtrate in our expectation
“ of the end! Whereas that ſuperlative wiſdom, that we grumble at for
“ thwarting us in the means, by croſſing us in the way, brings us the ſooner to our
“ journey's end. With divers contemplations upon this ſubject, I went to ſupper, and
“ thence to bed, not without ſome little fear of having our quarters beaten up by
“ the cavaliers that night; when lo! to ſecond my apprehenſions, about the dead of
“ my ſleep, and that night, I heard a thundering at the door, as if they meant to
“ fright it out of the hinges, and us out of our wits. I preſently leaped out of my
“ bed, in my ſtockings and clothes (my uſual night-poſture, when I travel); and
“ while *Roger* was lighting a candle, got my bilboa and other inſtruments from under
“ my pillow: whereupon *Roger* opening the door, ſaw it beſet with muſketeers, who
“ no ſooner ſaw us, but ſaid aloud, that we were not the men they looked for; and
“ being intreated to come into the chamber, refuſed it, and he, that brought them
“ thither, excuſed their troubling us, with as tranſcendent compliments as the brown
“ bill could afford. I wondered at their courteſy, till I knew, that it was the town-conſtable,
“ that, making a ſearch for ſome ſuſpicious perſons, and coming by my
“ chamber, that wanted a lock, either had a mind to make us take notice of ſo conſiderable
“ an officer, or no mind that we ſhould ſleep, whilſt our betters watched;
“ and for his not coming in, ſome accents of fear, that fell from him, made me
“ ſuſpect I was obliged for that to myſelf: and I remember, that juſt at the opening
“ of the door, he peeping in, eſpied me drawing a piſtol out of one of my holſters,

“ which, I believe, made him ſo niggardly of his company. Well, away went my gentleman in proſecution of his ſearch, and I even took my bows and arrows, and went to ſleep. The next day we dined at *Wincheſter*, and ever and anon, by the trembling paſſengers we met, were as nicely catechized concerning our ways, as if we were to be elected in the number of the new lay elders. From thence we reached *Saliſbury* that night, though before we came thither, we were fain to paſs in the dark through a wood, where we had warning given us, that about an hundred woodmen (we have got wild Engliſh too now) lay leiger, where theſe night-birds uſed to exerciſe their charity in eaſing weary travellers of ſuch burthenſome things as money and portmanteaus. There was nothing in all my journey vext me ſo much as the gravity of my ſteed; for though he were none of the freeſt of his legs, yet he kept my body in a reſtleſs agitation, which was none of the pleaſanteſt in the world neither; and if now and then I did ſolicit his ſides, to perſuade him to gallop (a dialect, that his feet were utterly unacquainted with) it ſhould be ſhort itſelf, and both uſhered in and epilogized with ſo long and ſo jolting a trot, that the trouble of it was far greater than the eaſe. At *Saliſbury* I overtook my trunks I had ſent thither before; and the next morning took them along with me over the plain; where when we had gone about half the way, we were ſuddenly environed with a party of horſe (beyond whom we might diſcover a body of foot) who came powdering ſo furiouſly upon us, that they ſcarce gave us leiſure to draw; but coming nearer, and knowing the ſtate's meſſenger (as he called himſelf) they durſt not meddle, neither with us, nor with my trunks, which they eyed though very lovingly; and had not we been there, would, I believe, have opened to ſearch for malignant letters, ſuch as uſe to be about the king's picture in a yellow boy. The foot we ſaw were poor preſſed countrymen, whom this party of horſe were ſent, not to convey, but to guard. Amongſt them I ſaw one poor rogue, lacqueyed by his wife, and carrying a child upon his ſhoulders. A pretty device, methinks, to make thoſe, who have no goods, to fight for their wives and children! Good God! that reaſonable creatures, that call themſelves Chriſtians too, ſhould delight in ſuch an unnatural thing as war, where cruelty at leaſt becomes neceſſity, and unprocured poverty becomes a crime; and a man with his whole family muſt be ſubject to be unavoidably undone, becauſe the violence perhaps of thoſe very ſoldiers that preſs him, had made him poor. At laſt on Saturday night I arrived, God be praiſed, at *Stalbridge*, and found by experience the truth of that ſenſeleſs proverb, the longeſt way about is the neareſt way home. And here the fair weather, that had been my conſtant companion from *London* hither, as ſoon as it ſaw me houſed, took leave of me; in whoſe abſence winter weather has always ſo fully and uninterruptedly domineered, that we all ſuſpect the almanac-maker of a miſtake, in ſetting down March inſtead of January. It confines me to my chamber, and is ſo drooping, that it dulls me to all kinds of uſeful ſtudy, and (which is worſt of all) it renders me obnoxious to theſe country viſits (or viſitations rather) which, you know, uſe to ſupply with their length what they want in their goodneſs. As ſoon as the weather will give me leave, I intend to take *Marſton Bigot* in my way to *Briſtol*, to put ſome end or other to the buſineſs —— I am loaded with civil language and fair promiſes, but I have always obſerved, that in the troopers dictionary the pages are ſo cloſe and thick written with promiſes, that there is no room left for ſuch a word as performance.

“My *Ethics*[*] go very ſlowly on; neither have I been poſſibly able to do any other buſineſs, ſave to make my brother's ſixty trees bear him ſome golden fruit, of which (though I did my uttermoſt endeavour to ripen it) I muſt gather but one third at May-day next, the other at, &c.

“My ſtay here, God willing, ſhall not be long, this country being generally infected with three epidemical diſeaſes (beſides that old leiger ſickneſs, the troop-flux) namely the plague, which now begins to revive again at *Briſtol* and *Yeovil* ſix miles off, fits of the committee, and conſumption of the purſe; to which ſo violent expulſives, if ſo potent an attractive, as a letter from you, were but added, it would both extremely ſweeten the ſtay, and accelerate the departure of,

“My deareſt ſiſter,

“your moſt affectionate brother,

“and humble ſervant,

“ROBERT BOYLE.”

Stallridge, March 30,
1646.

DURING his retirement at *Stalbridge*, he applied himſelf with great vigour to his ſtudies of various kinds, particularly thoſe of natural philoſophy and chymiſtry. Nor did he neglect to keep up a correſpondence with his friends by letters, ſome of which are extant of ſo early a date as the year 1646, wherein he not only gives an account of his ſtudies, but likewiſe diſcovers a great penetration, and a very ſolid judgment, concerning men and things, far above his age. And though he meddled as little as poſſible with politics, yet his intereſt, his good-nature, and his piety, would not ſuffer him to be an unconcerned ſpectator of thoſe miſeries, under which his country groaned; and in ſome of his remarks on men's preſages concerning the conſequences of the war, and their expectation of a ſettled peace, he paſſed a true judgement, founded on wiſe and ſolid reaſons, and verified by the event. For however many of his friends flattered themſelves with an happy iſſue of the war, he had all along different apprehenſions of it. I ſhall inſert therefore ſome extracts of the letters, according to theſe dates of them. The firſt was written to Mr. *Marcombes*, October 22, 1646, from *London*, where he then was.

“MONSIEUR,

“SINCE diſcountenance of the practice of your language has robbed me of that little readineſs your converſe had taught me in it, I ſhall take the liberty to make uſe of mine, which I know you underſtand equally with your own.

“IN my laſt I promiſed you a more full account of ſundry particulars, I had then the leiſure but to touch at; and for my diſengagement I ſhall tell you, that we are in a very doubtful condition for the preſent, though in all probability a few days will determine either our hopes or our fears. In *England* the great and uninterrupted ſucceſſes have tranſcended as well their own hopes, as their oppoſers fears. In *England* there is not one malignant garriſon untaken, and in *Wales* but two or

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[*] A treatiſe, which he was then compoſing on that ſubject, and ſtill extant in his own hand writing. This was one of his firſt books, and was begun before he was twenty years of age.

“ three rocky places hold out for the King, and theſe too ſo inconſiderable, that they
“ more advantage their enemies forces, by keeping them from idleneſs, than they
“ are able to prejudice them by their oppoſition. The Scots being now to quit the
“ kingdom, the parliament had compounded with them for all their arrears, upon
“ whoſe payment they are to deliver up their garriſons, and retire into their own
“ country. The ſum total, being in all 300,000l. is already agreed upon; but the
“ firſt payment is yet in debate. His majeſty is ſtill at *Newcaſtle*, both diſcontenting
“ and diſcontented; and the Scots will now, upon their departure out of *England*,
“ be forced ſome way or other to diſpoſe of his perſon, which the houſes have here
“ voted to remain at the diſpoſition of both houſes of parliament. The greater part
“ of men in theſe parts are pleaſed to flatter themſelves with the hopes of a ſpeedy
“ ſettlement of things; but for my part, that have always looked upon ſin as the
“ chief incendiary of the war, and yet have by careful experience obſerved the war
“ to multiply and heighten thoſe ſins, to which it owes its being, as water and ice,
“ which by a reciprocal generation beget one another, I cannot without preſumption
“ expect a recovery in that body, where the phyſic that ſhould cure, but augments
“ the diſeaſe. And this opinion of mine is over and above grounded upon ſuch politic
“ conſiderations, (though known to very few beſides myſelf) that in this I muſt
“ even wiſh to have leſs reaſon on my ſide. Thoſe that appear for the King in *Scotland*,
“ are bodied in an army of above 8000 men; a power ſo formidable in that
“ country, that certainly, if by the Scots ill uſage to his Majeſty it be provoked, it
“ will be able, if not to conquer, at leaſt to ruin that beggared kingdom.

“ As for *Ireland*, the news of my lord of *Ormonde*'s peace with the Iriſh, together
“ with the articles upon which it was concluded, have, I am confident, reached
“ *Geneva* long ere this. But our lateſt intelligence out of thoſe parts informs us,
“ that peace is very likely to produce its contrary amongſt them that made it, not
“ only by dividing betwixt the Iriſh and themſelves, for not only the Iriſh attempted,
“ though vainly, to ſurpriſe my lord of *Ormonde*, and cut off his party, whilſt they
“ were in their quarters; but the beſt general, and moſt numerous army, together
“ with divers of their greateſt towns, have poſitively declared againſt the peace; to
“ which the old Engliſh or catholic lords of the Engliſh pale, (ſo we call the counties
“ about *Dublin*) with many others, have ſubmitted; ſo that theſe two flints are
“ ſtriking ſuch ſparks, as are like to kindle a fine bonefire for the Engliſh, if they
“ have the wit but to lay hold on the opportunity, and blow theſe private diſcontents
“ into a civil war. Now this relation, however it ſeem not to carry with it any great
“ probability, I am the more apt to believe, becauſe it is credited by the knowledge
“ of the Iriſh intereſts; for the lords of the pale, though by manners and inclination
“ Iriſh, yet being Engliſh by deſcent, do with reaſon ſuſpect, that if they give
“ way to a total expulſion or extirpation of the Engliſh, their turn will come next
“ to drink out of the ſame cup; and therefore are very willing, by aſſenting to this
“ peace, to ſecure themſelves from that fear, and from the manifeſt danger, that
“ threatened them from their nearneſs to *Dublin*, in the proteſtants hands, and the
“ moſt conſiderable place of the kingdom. On the other ſide, the mere natives
“ promiſed themſelves for the general, by this rebellion, to exchange the throne of
“ *England* for St. *Peter*'s chair; or at leaſt to ſhake off the *Engliſh* yoke for that of
“ ſome foreign prince of their own religion; and in particular the nobility had already
“ devoured in their greedy hopes all the proteſtants eſtates; upon whoſe preſumed
“ acceſſion they built ſtrange and imaginary caſtles in the air. And the
“ clergy, the main firebrand of this rebellion, expected no leſs than to be reinſtated

“ into their ancient poſſeſſions; ſo that both theſe latter parties being thus fruſtrated of their hopes, endeavour to foment among the people (very fit tinder to catch at ſuch a fire) a diſlike of the preſent peace, which the former oppoſe by their authority, and the latter by thundering out excommunications againſt all thoſe that do act; a courſe that has a ſtrange influence upon that kind of people, whoſe ſuperſtition makes them believe a reality of force in thoſe ſcare-crow thunderbolts, that derive all their power only from the people's weakneſs, and are terrible for nothing but becauſe they are pleaſed to fear them.

“ THIS day with kingly ſtate was buried the great earl of *Eſſex*, having 400 officers, not one ſo low as a captain, the houſe of peers, the houſe of commons, the city, and the aſſembly of divines, for his mourners, and all the other parting compliments of honour, that ever ſubject could aſpire unto. His ſickneſs was an apoplexy, which did not long make him linger; and thus he, that had eſcaped ſo many mutinies, at laſt periſhed by a mutiny of the humours. But I have uſually obſerved, that in theſe great funeral ſolemnities, the pageantry of ſorrow has eaten up the reality; and the care of the blaze diverted men from mourning. Beſides theſe coſtly flatteries of the dead (with neglect of the poor, whom that charge might keep alive) ſeem to endeavour to make them guilty of prodigality in their very graves, whilſt it waſtes that upon a ſenſeleſs carcaſs, that is to it as uſeleſs as it is needleſs; whereas it were much better for them to procure the prayers of the living, than their admiration.

“ MY lord of *Inchiquin*'s abſence from *Munſter*, the greater part of this ſummer, by leaving my brother *Broghill* the ſole command of his army, gave him an opportunity to manifeſt to the world his gallantry, which he did with an unwonted ſucceſs; and that no leſs conſtant than it was great. But his own wants at home at laſt reduced him to that ſtarving condition, that when he prayed for his daily bread, his requeſt reached at leaſt as far as his expectation. He is now there ſoliciting for ſupplies for diſtreſſed *Munſter*, which, though very liberally voted, are ſo ſlow in their diſpatch, that many think they have juſt cauſe to apprehend, that the phyſic will not get thither before the patient be dead. His actions have hereby gained him a general eſteem; and he has (if he were not my brother, I durſt ſay deſervedly) acquired the repute of none of the leaſt wits of the time. He and I within theſe ten days intend, God willing, for the Weſt, in order to his journey to *Munſter*, whither he is now to carry over, under his own command, a gallant brigade of 4000 effective foot and 1500 horſe, for the parliament, in that province, till the enſuing ſpring enable them to tranſport thither a more conſiderable power.

“ THE preſbyterian government is at laſt ſettled (though I ſcarce think it will prove long lived) after the great oppoſition of many, and to their no leſs diſlike; though it ſeemed very high time unto others, that ſome eſtabliſhed and ſtrict diſcipline ſhould put a reſtraint upon the ſpreading impoſtures of the ſectaries, which have made this diſtracted city their general rendezvous, which entertains at preſent no leſs than 200 ſeveral opinions in point of religion, ſome digged out of thoſe graves, where the condemning decrees of primitive councils had long ſince buried them; others newly faſhioned in the forge of their own brains; but the moſt being new editions of old errors, vented with ſome honourable title and modern diſguiſements; ſo that certainly if the truth be any where to be found, it is here ſought ſo many ſeveral ways, that one or other muſt needs light upon it. But others, that juſtly pretend to a greater moderation, ſuſpect, that our dotage upon our own opinions makes us miſtake many for impoſtures, that are but glimpſes and manifeſtations of obſcure

“ or formerly concealed truths, or at leaſt our own pride or ſelf-love makes us aggravate very venial errors into dangerous and damnable hereſies. The parliament is now upon an ordinance for the puniſhment of many of theſe ſuppoſed errors; but ſince their belief of their contrary truths is confeſſedly a work of divine revelation, why a man ſhould be hanged, becauſe it has not yet pleaſed God to give him his ſpirit, I confeſs, I am yet to underſtand. Certainly to think by a halter to let new light into the underſtanding, or by the tortures of the body to heal the errors of the mind, ſeems to me like the applying a plaſter to the heel, to cure a wound in the head; which doth not work upon the ſeat of the diſeaſe.

“ My brother *Broghill* continues very much your friend, and, I am confident, will be very ready upon occaſion to realize his profeſſions; and the like I dare boldly affirm of my ſiſter *Ranelagh*. An employment fit for you we cannot yet procure, becauſe all our nobility ſtands at a gaze, to ſee whether the iſſue of the treaties now in debate will be either peace or war; in either of which caſes it is probable, that a good many of them will make viſits to foreign climates.

“ The ſadneſs of your condition I very much reſent, and would offer you my aſſiſtance to ſweeten it, if I did not think the proffer ſuperfluous; but truly I believe it would leſs afflict you, if you were a ſpectator of our miſeries here, where every day preſents us with much more unuſual diſpenſations of providence, where I myſelf have been fain to borrow money of ſervants, to lend it to men of above 10,000l. a year.

“ I was yeſterday in company with our *Iriſh* St. *Auſtin*, the archbiſhop of *Armagh*; and having told him, that you unfolded his myſtery of *the Incarnation* in *French*, he ſeemed very willing, that you ſhould publiſh it, upon the aſſurance I gave him of the fidelity of its tranſlantion. * * * * * *

“ And now it is high time I ſhould give you ſome account both of myſelf and of my condition; which truly hath been chequered with a great deal of variety of fortune, and a great many viciſſitudes of plenty and want, danger and ſafety, ſickneſs and health, trouble and eaſe; wherein I were guilty of an ingratitude great as the favour I have received, if I did not acknowledge a great deal of mercy in God's diſpenſation towards me; which truly hath been ſo kind, as oftentimes to work my good out of thoſe things I moſt feared the conſequences of, and changed thoſe very dangers, which were the object of my apprehenſion, into the motives of my joy. I was once a priſoner here upon ſome groundleſs ſuſpicions, but quickly got off with advantage. The roguery of *Tom. Murray* gave me a great deal of trouble to diſcover and prevent; but I thence reaped the benefit of making further diſcoveries into œconomical knowledge, than ever otherwiſe I ſhould have done. I turned him away laſt year, to let him know, that I could do my buſineſs very well without him; but now, having attained to a knowledge of my own ſmall fortune beyond the poſſibility of being cheated, I am likely to make uſe of him again, to ſhew my father's ſervants, that I wiſh no hurt to the man, but to the knave.

“ I have been forced to obſerve a very great caution, and exact evenneſs in my carriage, ſince I ſaw you laſt, it being abſolutely neceſſary for the preſervation of a perſon, whom the unfortunate ſituation of his fortune made obnoxious to the injuries of both parties, and the protection of neither. Beſides I have been forced to live at a very high rate, (conſidering the inconſiderableneſs of my income) and, to furniſh out theſe expences, part with a good ſhare of my land, partly to live

“ here like a gentleman, and partly to perform all that I thought expedient in order
“ to my Iriſh eſtate, out of which I never yet received the worth of a farthing. * * *

“ As for my ſtudies, I have had the opportunity to proſecute them but by fits and
“ ſnatches, as my leiſure and my occaſions would give me leave. Divers little eſſays,
“ both in verſe and proſe, I have taken the pains to ſcribble upon ſeveral ſubjects;
“ ſome of the leaſt bad of which I ſhall venture to ſend you over, as ſoon as my next
“ vacation ſpares me time to lick them into ſome leſs imperfect ſhape.

“ The *Ethics* hath been a ſtudy, wherein I have of late been very converſant, and
“ deſirous to call them from the brain down into the breaſt, and from the ſchool to
“ the houſe. I have endeavoured to make it not only a lanthorn, but a guide, in a
“ juſt, though a brief treatiſe, that I am writing of it; having already with much
“ trouble in ſome ſixteen chapters travelled through the moſt difficult part of it, and
“ that wherein I ſaw others deficient, I believe I ſhall leave the reſt to be compleated
“ by thoſe, who enjoy more leiſure.

“ The other humane ſtudies I apply myſelf to, are natural philoſophy, the mechanics,
“ and huſbandry, according to the principles of our new philoſophical college,
“ that values no knowledge, but as it hath a tendency to uſe. And therefore
“ I ſhall make it one of my ſuits to you, that you would take the pains to enquire
“ a little more thoroughly into the ways of huſbandry, &c. practiſed in your parts;
“ and when you intend for *England*, to bring along with you what good receipts or
“ choice books of any of theſe ſubjects you can procure; which will make you extremely
“ welcome to our *inviſible college*, which I had now deſigned to give you a
“ deſcription of, but a gentleman, whom I have been forced to keep talk with all
“ the while I was writing this, together with the fear of having too much already
“ treſpaſſed upon your patience, call upon me to end your trouble and this letter
“ together.”

He was likewiſe at *London* in February 1646-7, whence he wrote the following letter, dated the 20th of that month, to Mr. *Francis Tallents*, then fellow of *Magdalen* college in *Cambridge*.

SIR,

“ I SHOULD venture to apologize for my ſilence, if I thought it not leſs guilty
“ than meritorious; ſince to reprieve you from the importunity of my letter, I have
“ hitherto denied myſelf that happineſs, that your civility makes me confident I might
“ have enjoyed by the receiving of yours. I have been every day theſe two months
“ upon viſiting my own ruined cottage in the country; but it is ſuch a labyrinth this
“ *London*, that all my diligence could never yet find the way out on’t, and hath but
“ juſt now put me in a probability of leaving it within theſe two or three days. The
“ beſt on’t is, that the corner-ſtones of the *inviſible*, or (as they term themſelves)
“ the *philoſophical college*, do now and then honour me with their company, which
“ makes me as ſorry for thoſe preſſing occaſions that urge my departure, as I am at
“ other times angry with that ſolicitous idleneſs that I am neceſſitated to during my
“ ſtay; men of ſo capacious and ſearching ſpirits, that ſchool-philoſophy is but the
“ loweſt region of their knowledge; and yet, though ambitious to lead the way to
“ any generous deſign, of ſo humble and teachable a genius, as they diſdain not to
“ be directed to the meaneſt, ſo he can but plead reaſon for his opinion; perſons
“ that endeavour to put narrow-mindedneſs out of countenance, by the practice of ſo

" extenſive a charity, that it reaches unto every thing called man, and nothing leſs " than an univerſal good-will can content it. And indeed they are ſo apprehenſive " of the want of good employment, that they take the whole body of mankind for " their care.

" BUT leſt my ſeeming hyperbolical expreſſions ſhould more prejudice my reputation than it is able any ways to advantage theirs, and I be thought a liar for " telling ſo much truth, I will conclude their praiſes with the recital of their chiefeſt " fault, which is very incident to almoſt all good things; and that is, that there is " not enough of them.

" FOR news, I believe you do not ignore, what a ſtream of ſucceſs the parliament, " ſince I had the honour to ſee you, has had. I will only now take the freedom to " tell you, that I am greatly afraid, moſt men flatter themſelves in their prognoſtications of peace, which are calculated rather to the meridian of their deſires, than to " that of their reaſon. And though I muſt confeſs, the traveller ſeems to be very " near the inn, yet I know not why the horſe may not ſtumble at the threſhold; for " I am ſomewhat unapt to perſuade myſelf, that the judgment will ceaſe, while the " the cauſe continues; but am rather very ready to apprehend, that while adverſity " makes ſome obſtinate, and others wanton, though the war perhaps may die, the " judgment will be kept alive. The pulpits were never more adorned with excellent " divines, than they now are here, but with ſo unſuitable a ſucceſs in many of the " people, that I can ſometimes think it no breach of charity to believe, that the " ſmall-pox has ſtricken inward, and many of them have but baniſhed their vices " from the body into the heart. For my part, the excellency of the miniſtry, ſince " waited on by ſuch an improficiency, increaſes my preſaging fears of the approaching miſery of the people; for I ſhall eaſily be drawn to ſuſpect that horſe to be " very ſick, that thrives not in ſo plentiful a paſture. And truly, methinks, it is but " a very ſad ſymptom, when the phyſic augments the diſeaſe. For matter of ſects, " it ſeems, that moſt of thoſe at *Amſterdam* have been returned us over by bill of exchange, which our Engliſh ſearchers have been ſo induſtrious to improve, that there " are few days paſs here, that may not juſtly be accuſed of the brewing or broaching of ſome new opinion. Nay, ſome are ſo ſtudiouſly changeling in that particular, " they eſteem an opinion as a diurnal, after a day or two ſcarce worth the keeping. " If any man have loſt his religion, let him repair to *London*, and I'll warrant him, " he ſhall find it: I had almoſt ſaid too, and if any man has a religion, let him but " come hither now, and he ſhall go near to loſe it. Pray God, it fare not with religion amongſt thoſe novelties, as it does ſometimes with a great commander, when " he is taken priſoner by a company of common ſoldiers, who every one tugging to " have him for himſelf, at laſt pull him to pieces, and ſo each get a limb, but none " enjoys him whole. For my part, I ſhall always pray to God to give us *the unity* " *of the ſpirit in the bond of peace*, and deſire you to believe, that, amongſt all the " apoſtaſies of the time, I ſhall be the leaſt capable of being ſeduced by that that " may oppoſe my being and continuing ſo,

" Your moſt affectionate friend and faithful ſervant,

" ROBERT BOYLE."

MR. *Tallents*, to whom this letter was written, had been, while he was an undergraduate, ſub-tutor to ſeveral ſons of the earl of *Suffolk* (one of whoſe daughters was

married

married to lord *Broghill*) being removed from *Peter* houſe to *Magdalen* college for that purpoſe; and about the year 1642 travelled abroad with them as their tutor. After his return, he became ſenior fellow and preſident or vice-maſter of his college; and having ſpent near twenty years in the univerſity, was ſettled of St. *Mary*'s in *Shrewſbury*, whence he was ejected for nonconformity in 1662. In the year 1670 he went a ſecond time as tutor to two young gentlemen, Mr. *Boſcawen* and Mr. *Hamden*. In the laſt part of his life he was paſtor to a congregation of proteſtant diſſenters at *Shrewſbury*, where he died April 11th, 1708, in the 89th year of his age. He was author of the Chronological Tables[a].

Mr. *Boyle* returned to *Stalbridge* ſoon after his writing the abovementioned letter to Mr. *Tallents*, as appears from one to his ſiſter, the lady *Ranelagh*, dated there Feb. 27, 1646-7, in which he writes as follows:

“ * * * As to my *Dublin* buſineſs, I muſt expect from your ſpeedy information (I now beg of you) of their names, to whom the parliament has given power to oblige me there, directions both how to frame, and to whom to addreſs my requeſt. I muſt acknowledge to you, amongſt God's mercies of the firſt magnitude, both my lord of *Ormonde*'s unexpected coming in, and the parliament's leſs expected diſpatch; though both very conſonant to the wonted method of that gracious providence, that we find then oftentimes the neareſt for our reſcue, when that is furtheſt from our expectations. For my part, I am bold to believe, had we no other arguments to prove a providence, yet the ſtrange revolutions, of which in ſo ſhort a time our ruined country has been the unhappy ſcene, were more than ſufficient to demonſtrate that truth, to which alone I ſhall allow a greater infallibility, than to that other moſt certain one of my being,

“ Siſter, your moſt truly affectionate

“ brother and humbleſt ſervant,

“ Robert Boyle.”

His next letter, dated from the ſame place, March 6, 1646-7, ſhews, that he was then entering upon his chemical ſtudies.

“ * * * That great earthen furnace, ſays he, whoſe conveying hither has taken up ſo much of my care, and concerning which I made bold very lately to trouble you, ſince I laſt did ſo, has been brought to my hands crumbled into as many pieces, as we into ſects; and all the fine experiments, and caſtles in the air, I had built upon its ſafe arrival, have felt the fate of their foundation. Well, I ſee I am not deſigned to the finding out the philoſophers ſtone, I have been ſo unlucky in my firſt attempts in chemiſtry. My limbecks, recipients, and other glaſſes have eſcaped indeed the miſfortune of their incendiary, but are now, through the miſcarriage of that grand implement of *Vulcan*, as uſeleſs to me, as good parts to ſalvation without the fire of zeal. Seriouſly, Madam, after all the pains I have taken, and the precautions I have uſed, to prevent this furnace the diſaſter of its predeceſſors, to have it tranſported a thouſand miles by land, that I may after all this re-

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[a] See his character by Mr. *Matthew Henry* ſubjoined to his *Funeral Sermon.*

“ ceive it broken, is a defeat, that nothing could recompenſe, but that rare leſſon it “ teaches me, how brittle that happineſs is, that we build upon earth.”

HIS acquaintance with Mr. *Samuel Hartlib* began very early. The father of that gentleman was a very conſiderable merchant in *Poland*, but obliged, on account of his attachment to the proteſtant religion to retire to *Elbing* in *Pruſſia*. His two firſt wives were Poliſh ladies, of noble extraction, and his third, the mother of Mr. *Samuel Hartlib*, had two ſiſters, both of whom were very honourably married, one to Mr. *Clarke*, the ſon of a lord mayor of *London*, afterwards to Sir *Richard Smith*, a privy counſellor, and at laſt to Sir *Edward Savage*; and the other to Mr. *Peake*, a younger brother of a good family[a]. Mr. *Hartlib*, Mr. *Boyle*'s friend, came to *England* about the year 1630, and ſoon diſtinguiſhed himſelf by his great zeal for the improvement of natural knowledge, and making it uſeful to human life. For which purpoſe, beſides what notices he could procure at home, he entertained a conſtant correſpondence with the virtuoſi in moſt other parts of *Europe*; and *Milton*, in his *Tractate of Education*, which he addreſſed to him, ſpeaks of him, as “ a perſon ſent hither by ſome “ good providence from a far country, to be the occaſion and the incitement of great “ good to this iſland.” He obſerves likewiſe himſelf, in his letter to Dr. *Worthington* abovecited, that “ as long as he had lived in *England*, he had ſpent yearly out of his “ own betwixt three and four hundred pounds ſterling. And when, ſays he, I was “ brought to publick allowance, I have had from the parliaments and councils of “ ſtate a penſion of three hundred pounds ſterling a year, which as freely I have “ ſpent for their ſervice and the good of many.” But notwithſtanding the eminent advantages, which the public had reaped from his writings, and the aſſiſtances he had given to thoſe of other men, he was reduced, after the Reſtoration, to very neceſſitous circumſtances, as appears from the letters of Dr. *Worthington* to him, publiſhed at the end of that divine's *Miſcellanies*, and from a petition[b] of Mr. *Hartlib* to the houſe of commons, that “ he might find from their goodneſs and bounty ſome relief “ in his diſtreſſed condition, by being freed from his debts, and put in a capacity to “ continue his ſervice to the public, to advance in his generation the beſt objects for “ the uſe of mankind in all kinds.”

THE firſt letter I find of Mr. *Boyle* to him was dated from *Stalbridge*, March 19, 1646-7.

“ Dear Mr. *Hartlib*,

“ I NEED a great deal of rhetoric to expreſs to you, how great a ſatisfaction I “ received in the favour of your letter, both for the ſake of the theme, and more “ for that of the author. But my contentment was greatly qualified by the miſcarriage of the general writing (which ſhould be ſtiled the Univerſal Truchman, or “ General Interpreter, if I were to be godfather) you were pleaſed to ſend me; for “ the commendations you give it are too great not to make me very ſenſible of its “ loſs. If the deſign of *the Real Character*[c] take effect, it will in good part make “ amends to mankind for what their pride loſt them at the tower of *Babel*. And

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[a] Mr. *Samuel Hartlib*'s letter to Dr. *John Worthington*, Aug. 3, 1660, communicated by Mr. *John Ward*, profeſſor of rhetoric in *Greſham* college.

[b] Communicated by Mr. *Ward*.

[c] This ſeems to relate to an excellent deſign, which was afterwards executed by Dr. *John Wilkins*, and publiſhed at *London*, 1668, in folio, under the title of *An Eſſay towards a Real Character, and a Philoſophical Language*.

“ truly, ſince our arithmetical characters are underſtood by all the nations of *Europe* the ſame way, though every ſeveral people expreſs that comprehenſion with its own particular language, I conceive no impoſſibility, that oppoſes the doing that in words, that we ſee already done in numbers. As for the pneumatical engine, that I uſe to call a wind-gun, which you mention in your letter as preſented to the king, and forbidden by him to have any companions, ſure the artiſt, that received the command, was more ingenious than obedient; for I remember very well to have ſeen one of them not exceeding in bigneſs, nor differing much in ſhape from an ordinary carabine, which being charged by the ſole impreſſion of the air, would, by violence of the contracted Boreas, ſend forth a leaden bullet, juſt the caliber, with force to kill a man at twenty-five or thirty paces diſtance from him. This wind-gun I ſaw both charged and diſcharged; and now it comes into my mind, I read, not long ſince, in a late mechanical treatiſe of the excellent *Merſennus*, both the conſtruction and the uſe of this engine; and amongſt the uſes one, whoſe ſtratagem obliged me to take of it particular notice; and it was, how by the help of this inſtrument to diſcover the weight of the air; which, for all the prattling of our book-philoſophers, we muſt believe to be both heavy and ponderable, if we will not refuſe belief to our ſenſes. Your *Imago Societatis*, and your *Dextera Amoris*, I have great longings to peruſe; and though with a deep ſenſe of my inſufficiency, I ſhall very freely expreſs my obedience in delivering the opinion of

“ Your humble ſervant,

“ ROBERT BOYLE.”

THE next letter extant to that gentleman is of the 8th of April, 1647.

“ Dear Mr. *Hartlib*,

“ I HOPE I ſhall need none other apologies for my laſt week's ſilence, than my abſence, when your letters arrived here. Your *Imago Societatis* with a great deal of delight I have peruſed, but muſt beg ſome leiſure to acquaint you with my opinion of it, which now were almoſt impoſſible for me to do, I having already preſented it to a perſon of quality, with whom, if it take ſuitably to my wiſhes, it may thence have no obſcure influence upon the public good, concerning which I have lately traced a little dialogue in my thoughts, which my unceaſing domeſtic diſtractions will by no means as yet permit me to blot paper with. The epiſtle prefixed to the *Imago* is both pithy and to the purpoſe. And truly I am extremely glad to ſee a perſon of Mr. *Hall*'s years employ, in attempts of this nature, that youth, which the moſt of thoſe, that are as little indebted to time as he, think too good for their Maker, though they think it not too good to be ſquandered away. *Campanella*'s *Civitas Solis*[a], and that ſame *Reſpublica Chriſtianopolitana*, which he mentions, will both of them deſerve to be taught in our language. Of the *Utopia* he is modelling, though I cannot judge, before it ſees the light, yet my expectations will be none of the ſmalleſt, if I proportion them to the ingenuity of the author. And for the Divine Emblems, that he makes us hope for, I muſt reſerve my ſenſe of them for their peruſal, ſince the opinions I embrace, both about the

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[a] This piece contains the idea of a commonwealth, after the manner of Sir *Thomas More*'s *Utopia*, and is printed with *Campanella*'s *Realis Philoſophiæ Epilogiſticæ partes quatuor*, at *Francfort*, 1623, in 4to.

“ nature and the teaching of virtue, will doubtleſs appear as paradoxical to others, as they ſeem probable to me. For thoſe deſigns of Mr. *Hall's* Tutor, which you have ſo appropriatingly chriſtened, I believe the requeſts of the excommunicated petition now on foot in the army will ſcarce receive a ſlower ſatisfaction; for the particulars, in which he requires it, do not only aſk a profound knowledge and ſolid judgment (qualities of themſelves not very epidemical) but likewiſe a leiſured and a great multiplicity of reading, and ſo an intimacy as well with authors as with things; qualities, that in this ſtirring and neceſſitous age (where men are forced to court don *Plutus* and my lady *Fortune* with more aſſiduity than the Muſes) make very unfrequent matches in the ſelf-ſame perſon: beſides that the diſſenting opinions of the *Ptolemeans*, the *Tychonians*, the *Copernicans*, (to which I was once very much inclined) and the other noveliſts, are both ſo irreconcilable among themſelves, and leave a man ſo little latitude of neutrality, that it will be perhaps but one remove from impoſſible, preciſely to declare, what has been hitherto both perfectly demonſtrated and confeſſedly on all ſides; the one taking that (as I have known by experience, when I ſtudied that problem of the earth's peregrination) for an undeniable demonſtration, which the other will either abſolutely reject as a paralogiſm, or at leaſt call in queſtion as no more than a bare probability.

“ YOUR *Common Writing* (for which you have my humble thanks) is at laſt come ſafe to my hands; but my occaſions have not yet allowed the leiſure to fix my thoughts upon it: only if the dictionary (whoſe edition, had my wiſhes the power to ſwiften it, ſhould be very ſudden) do not over-ſwell and diſeaſe it of a tympany, methinks the bulk of the grammar is very reaſonable in reference to what the title promiſes, which I was well pleaſed to ſee ſo, apprehending (nor are my fears yet entirely ſuppreſſed) leſt that this way of ſaving the labour of learning a language ſhould prove like a new device, I have lately ſeen, to perform all the operations of arithmetic by the help of an inſtrument, where I found it much more difficult to learn the uſes of the inſtrument, than the rules of the art.

“ THAT in my laſt I took no notice of Mr. *Dury's* excellent diſcourſe of teaching logic, was not at all my neglect, but the treacherouſneſs of my memory; for which meaning to apologiſe to the author himſelf, I ſhall for this time add no more to your trouble, but an humble requeſt to aſſure him, that I am his, and yourſelf, that I am

“ Your moſt really affectionate friend and humble ſervant,

“ ROBERT BOYLE.”

THE letter, which he wrote on the 3d of *May*, 1647, to Mr. *John Dury*, famous for his attempts to reconcile the Lutherans and Calviniſts, will ſhew Mr. *Boyle's* moderation, at that time, with reſpect to differences in religious opinions.

“ IT has been long, ſays he, as well my wonder, as my grief, to ſee ſuch comparatively petty differences in judgment make ſuch wide breaches and vaſt diviſions in affection. It is ſtrange, that men ſhould rather be quarrelling for a few trifling opinions, wherein they diſſent, than to embrace one another for thoſe many fundamental truths, wherein they agree. For my own part, in ſome two or three and forty months, that I ſpent in the very town of *Geneva*, as I never found that people diſcontented with their own church-government (the gallingneſs of whoſe yoke is the grand ſcare-crow that frights us here) ſo could I never obſerve in it

“ any ſuch traſcendent excellency, as could oblige me either to bolt heaven againſt,
“ or open *Newgate* for all thoſe, that believe they may be ſaved under another.
“ Wherefore I muſt confeſs, it would be extremely my ſatisfaction, if I could ſee,
“ by God's bleſſing, your pious endeavours of twiſting our froward parties into a
“ moderate and ſatisfactory reconcilement, as ſucceſsful, as I am confident they will
“ be prudent and unwearied. As for our upſtart ſectaries (muſhrooms of the laſt
“ night's ſpringing up) the worſt part of them, if not exaſperated by, inſtead of
“ lighting them into the right way with the candle, flinging the candleſtick at their
“ heads, like *Jonah*'s gourd, ſmitten at the root with the worm of their irrationality,
“ will be as ſudden in their decay, as they were haſty in their growth; and indeed
“ perhaps the ſafeſt way to deſtroy them is rather to let them die, than attempt to
“ kill them.”

ON the 8th of the ſame month he wrote the following letter to Mr. *Hartlib*.

“ Dear Mr. *Hartlib*,

“ IT was very needleſs in your laſt, to make apologies for the glad parliamentary
“ news you began your letter with; for beſides that its goodneſs authoriſes its nature,
“ and were able to prefer ſo pleaſing a diſobedience to the moſt exact compliance
“ with my deſires; beſides this, I ſay, you intereſt yourſelf ſo much in the
“ *Inviſible College*, and that whole ſociety is ſo highly concerned in all the accidents of
“ your life, that you can ſend me no intelligence of your own affairs, that does not
“ (at leaſt rationally) aſſume the nature of *Utopian*. And truly, Sir, for my particular,
“ had you been to coin and ſhape news, not ſo much to inform, as to delight
“ me, you could ſcarce have made choice of any, that were more welcome, either to
“ my wiſhes for their own particular ſatisfaction, or for thoſe that I dedicate to the
“ good of the public, which can acknowledge your merits with no advantage, that has
“ not a direct tendency to its own, and which, by the higheſt expreſſions of gratitude
“ for your ſervice to it, does but enable your zeal to multiply and continue them.
“ The phraſes of the ordinances (which theſe alone of yours have brought me into
“ charity with again) were indeed extremely civil in reſpect of thoſe that framed
“ them; and yet but barely juſt in regard of him for whom they were deſigned.
“ Certainly the taking notice of, and countenancing men of rare induſtry and publick
“ ſpirit, is a piece of policy as vaſtly advantageous to all ſtates, as it is ruinouſly neglected
“ by the moſt. And therefore we may evidently obſerve thoſe commonwealths
“ (as the *Hollanders* and the *Venetian*) to be the moſt happy and the moſt
“ flouriſhing, where ingenuity is courted with the greateſt encouragements. Mr.
“ *Hall*'s unmerited elogium of me I muſt in juſtice aſcribe rather to his civility, than
“ to his opinion; to the former of which I am alſo redevable for a very handſome
“ complimental letter, he was lately pleaſed to honour me with, to which I ſhall requeſt
“ a ſpeedy conveyance of the incloſed (though unſuitable) anſwer, and a belief,
“ that I liſt it not amongſt the leaſt of your favours, to procure me the acquaintance
“ of a perſon, that treading antipodes to the ſtrain of his contemporaries, has September
“ in his judgment, whilſt we can ſcarce find April upon his chin.

“ MY ſenſe of his propoſitions concerning the College I muſt neceſſarily ſuſpend,
“ till a more exquiſite information of the particulars of his whole deſign. Only by
“ the by I ſhall take the freedom to tell you, that though I eſteem Mr. *Hall* very
“ moderate in the point of pecuniary duties, you can ſcarce be too tender in taſking
“ young collegiates as to the duties of the brain, ſince they being all of them to be

“ perſons of quality and voluntiers, will hardly ſupport with alacrity any thing that ſavours of conſtraint; beſides that the gallantry and nobleneſs of their own principles will carry them on unimpoſedly to do much more, than your ſtricteſt conſtitutions can reaſonably enjoin them. The expedients you propoſe to Mr. *Hall*'s tutor are not at all unlikely to take; and the applications you counſel him to make to thoſe three famous mathematicians, can promiſe a great deal of probability for their ſucceſs: eſpecially *Gaſſendus*, a great favourite of mine, I take to be a very profound mathematician, as well as an excellent aſtronomer, and one that has collected a very ample treaſury of numerous and accurate obſervations of all that belongs to the abſtruſe ſcience of thoſe ſublimer bodies. I find you very happy, or rather very judicious, in the nominating of the perſons, Mr. *Hall*'s tutor is by you addreſſed to; and am confident, as well as you, that thoſe elevated ſpirits will not prove half ſo coſtive and ſo pedantical, as the great ſcholarians of our colleges, whom yet I am apt enough to pardon, in conſideration of the uſefulneſs (for the moſt part) of the knowledge they conceal, which perhaps being admired but as long as kept in a myſtery, an imparting diſcovery would depreciate.

“ THE riſe you have now to reſume your former correſpondencies with the great *Merſennus*, I hope you will greedily embrace, he being a man truly incomparable in his own way, and the mechanics he tranſcends in as greatly beneficial as little underſtood. The Engliſhing of, and additions to *Oughtred*'s *Clavis Mathematica* does much content me, I having formerly ſpent much ſtudy on the original of that algebra, which I have long ſince eſteemed a much more inſtructive way of logic, than that of *Ariſtotle*. No body has yet been charitable enough to ſend me either the long deſired *Office of Addreſs*, or Mr. *Dury*'s wiſhed-for diſcourſe concerning Accommodation, though my longings for their ſight have been very ſuitable to the contentments I expect from their peruſal. I have written a long letter to Mr. *Dury*, by the ſame poſt, that is to deliver you this; and it ſhall not be the neglect of my improving my rhetoric to the uttermoſt, that ſhall impede my prevailing with him, by exemplifying his rules, to clothe with fleſh and ſkin his excellent ſkeleton of the Art of Reaſoning.

“ FOR your bedfellow's receipt for the ſtone (which certainly wants a parallel, if it be not more eaſy than effectual) I beſeech you to return her (together with the preſent of my humble ſervice) moſt humble thanks, which I mean very ſhortly, God willing, to pay you in an epiſtle I have drawn up to perſuade men to communicate all thoſe ſucceſsful receipts, that relate either to the preſervation or recovery of our health; to which (if you will pardon me a clinch) I ſhall add, as to the diſeaſe laſt named (ſo cruel in its tortures, and ſo fatal in its cataſtrophe) that they muſt have their hearts more hard than a very ſtone, that can refuſe a ſanative remedy for the ſtone.

“ As for me, during my confinement to this melancholy ſolitude, I often divert myſelf at leiſure moments in trying ſuch experiments, as the unfurniſhedneſs of the place, and the preſent diſtractedneſs of my mind, will permit me; which when once my vacant intervals of time will give me leave to blot paper with, and make ſome ſhort diſcourſes and reflections upon, you may (with all the ſervices you ſhall be pleaſed to command their author) confidently expect from,

“ Sir, your moſt affectionate friend and humble ſervant,

“ ROBERT BOYLE.”

THE *Inviſible College*, which Mr. *Boyle* ſpeaks of in this letter, as well as in thoſe to Mr. *Marcomes* and Mr. *Tallents*, probably refers to that aſſembly of learned and curious gentlemen, who, after the breaking out of the civil wars, in order to divert themſelves from thoſe melancholy ſcenes, applied themſelves to experimental inquiries, and the ſtudy of nature, which was then called the new philoſophy, and at length gave birth to the Royal Society. This aſſembly Dr. *Wallis*, who was one of its members, gives the following account of*. "About the year 1645 (ſays he) while I lived in *London*, I had the opportunity to be acquainted with divers worthy perſons, inquiſitive into natural philoſophy and other parts of human learning, and particularly of what hath been called the new or experimental philoſophy. We did by agreement, divers of us, meet weekly on a certain day, to treat and diſcourſe of ſuch affairs. Of which number were Dr. *John Wilkins*, afterwards biſhop of *Cheſter*, Dr. *Jonathan Goddard*, Dr. *George Ent*, Dr. *Gliſſon*, Dr. *Merret*, doctors in phyſic; Mr. *Samuel Foſter*, then profeſſor of aſtronomy at *Greſham* college, Mr. *Theodore Haak*, a German of the Palatinate, and then reſident in *London*, who, I think, gave the firſt occaſion, and firſt ſuggeſted thoſe meetings, and many others. Theſe meetings we held ſometimes at Dr. *Goddard*'s lodging in *Wood-ſtreet* (or ſome convenient place near) on occaſion of his keeping an operator in his houſe, for grinding glaſſes for teleſcopes and microſcopes; and ſometimes at a convenient place in *Cheapſide*; ſometimes at *Greſham* college, or ſome place near adjoining. Our buſineſs was (precluding matters of theology and ſtate-affairs) to diſcourſe and conſider of philoſophical inquiries, and ſuch as related thereunto, as phyſic, anatomy, geometry, aſtronomy, navigation, ſtatics, magnetics, chemics, mechanics, and natural experiments, with the ſtate of theſe ſtudies, as then cultivated at home and abroad. About the year 1648, 1649, ſome of us being removed to *Oxford*, firſt Dr. *Wilkins*, then I, and ſoon after Dr. *Goddard*, our company divided. Thoſe in *London* continued to meet there, as before, and we with them, when we had occaſion to be there. And thoſe of us at *Oxford*, with Dr. *Ward*, ſince biſhop of *Saliſbury*, Dr. *Ralph Bathurſt*, now preſident of *Trinity* college in *Oxford*, Dr. *Petty*, ſince Sir *William Petty*, Dr. *Willis*, then an eminent phyſician in *Oxford*, and divers others, continued ſuch meetings in *Oxford*, and brought thoſe ſtudies into faſhion there, meeting firſt at Dr. *Petty*'s lodgings in an apothecary's houſe, becauſe of the convenience of inſpecting drugs, and the like, as there was occaſion; and after his remove to *Ireland* (though not ſo conſtantly) at the lodgings of Dr. *Wilkins*, then warden of *Wadham* college; and after his removal to *Trinity* college in *Cambridge*, at the lodgings of the honourable Mr. *Robert Boyle*, then reſident for divers years in *Oxford*. Theſe meetings in *London* continued, and after the king's return, in 1660, were increaſed with the acceſſion of divers worthy and and honourable perſons, and were afterwards incorporated by the name of the *Royal Society*, and ſo continue to this day."

But to return to Mr. *Boyle*, his early zeal for religion and virtue, and his friendly faithfulneſs in reproving and counſelling ſuch as tranſgreſſed the laws of both, are evident from two letters ſent to a noble lord, one or both written when he was but little turned of twenty years of age. I ſhall ſubjoin one of them.

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* See his letter to Dr. *Thomas Smith*, dared January 29, 1696-7, publiſhed in Mr. *Thomas Hearne*'s Appendix to his preface to *Peter Langtoft*'s chronicle, Vol. I. p. 161. edit. *London*, 1725.

“ Dear Count,

“ WHEN I ſhould have anſwered yours by the laſt poſt, ſome phyſic I had
“ taken the night before ranging very briſkly through all my veins had ſo
“ ſtirred up and exaſperated my ill humours, that I durſt not handle a pen. But now
“ I am calmed enough to let you know, that I leſs applaud your conſtancy than your
“ gallantry, that makes you ſo unſeaſonably turn Preſbyterian, by devoting yourſelf
“ to the *Kirk*. I hope you will never juſtify the proverb of, *the nearer the Church,*
“ *the farther from God*; but this ſame epithet of *incomparable*, with the appertaining
“ ſtory, if it be not rather your compliment, than your opinion, reliſhes of more
“ paſſion, than is allowed you for any woman but one. If I durſt ſo much injure
“ your judgment, as to diſtruſt your conſtancy, I ſhould tell you, that marriage is
“ not a bare preſent, but a legal exchange of hearts; and the ſame contract, that
“ gives you right to another's, ties you to look upon your own as another's goods,
“ and too ſurely made over, to remain any longer in your gift. I ſhould tell you
“ farther, that no change could be leſs excuſable than yours, not only becauſe you
“ are bleſſed with a lady, whoſe beauty and merit make injuries to your faith and
“ happineſs inſeparable, but becauſe too you cannot father upon your obedience any
“ temptation to a ranging flame; for you aſſumed the entire liberty of chuſing to
“ yourſelf whom you muſt now ſolely blame, if you matched not to your ſatisfaction,
“ when you matched ſo purely for it, that had you as much motive to fickleneſs, as
“ you have to the contrary virtue, you ought to expect as little pity, as, God be
“ praiſed, you need; and could betray no diſlike of your election, that would not
“ either criminate your honour, or diſparage your judgment. * * *

“ BUT I hope, dear count, by this time you will think me as mad for writing all
“ this to you ſeriouſly, as I ſhould think you, if my ſuſpicions were juſt; for I hope
“ your innocence will turn all this into a compliment to Mrs. *Kirke*, by diſtruſting
“ her eyes of the power of kindling a flame, which ſuch obſtacles oppoſe, and ſo
“ great inconveniences would attend.

“ THE diſaſter of your addreſſes to the council of ſtate I am extremely troubled
“ at, both for your ſake and my own; ſince not only that diſappointment defeats me
“ of good company, but, I much fear, miſleads you into ill. And truly, my lord,
“ I am enough your ſervant to tell you freely, that a young man, that ventures himſelf
“ in ill company, tempts as many temptations, and expoſes himſelf to as many
“ inconveniencies, as to reckon up would require a homily, and to inſiſt on a volume,
“ rather than a letter. The ſeveral goods of body, mind, fame, and fortune,
“ are blaſted by ill company. For firſt (to begin with the laſt) it engages us to that
“ riot and expenſiveneſs, which, beſides that it is ever ruinous, is now ſignally unſeaſonable,
“ being not only extremely unſuitable to the ſad condition of our times,
“ but peculiarly obſtructive to thoſe who make applications to the ſtate for relief or
“ compenſation of loſſes; ſince the ſame expences that beget want, paſs for arguments
“ of plenty, in the opinion of thoſe whoſe belief of our want is as great a
“ requiſite to the removal of it, as the juſtneſs of that belief were a misfortune.
“ How much the body ſuffers by thoſe debauches, that are epidemical to looſe company,
“ though the inſtances be numerous and obvious, few youths will believe,
“ till their experience hath ſadly convinced them; not to mention, that thoſe diſtempers
“ that are both the effects and puniſhments of exceſs, are as uneaſy upon the
“ ſcore of ſhame, as that of pain; and health impaired by diſſoluteneſs has commonly
“ the fate of needing pity, and wanting it. As for the mind, ill company is

“an infected air for it; which, though it be preciſely no diſeaſe, diſpoſes to the moſt mortal ones. Bad examples are the general pimps of every vice; and I dare boldly affirm, that fewer people go to hell for pride, or oaths, or incontinency, than for company; which I therefore deſire to draw in blacker colours to you, becauſe I have ever obſerved it worſt for the beſt natures, ſince they being pliable and eaſy do many things that offend their conſciences, merely to pleaſe the company which a man muſt be like to be liked, and which not to imitate is interpreted to reprove. And when once compliance has betrayed one to a vice, cuſtom detains us there, it happening to men in ſins, as in taking of tobacco, which, though at firſt ſucked in with reluctance, barely to pleaſe the company, men afterwards find a guſto in, and are unable to leave off. And therefore I prefer few laws before that of a nation we are pleaſed to call barbarous, amongſt whom it was allowed to ſue a man for keeping evil company, as well as for riot, adultery, or any other crime it leads to; which will appear leſs ſtrange and more equitable to them that conſider, that amongſt us a bawd is as legally puniſhable as a whore or wenching. And laſtly, a good name can by nothing be ſtrained deeplier than by ill company. Few people have either the charity or opportunity to know men intimately; and there is no way either more probable, or more frequented, to judge of thoſe we know not, than by their company, ſince men are not unreaſonably ſuppoſed to delight in thoſe qualities they delight in others for. For beſides that the choice of ill company does not only make men thought to have been vicious, but to be made ſo, as longing for unhealthy and extravagant traſh does not only argue, that a maid has the green-ſickneſs, but ſerves to increaſe the diſeaſe; young men conſider not enough, how deep a root the firſt impreſſions they give of themſelves take, and conſider not, how difficult it is to bring men to think well, when to do ſo they muſt acknowledge to themſelves, that they have thought amiſs; eſpecially ſince moſt human acknowledgments being of ſo changeable-coloured a complexion, that like pigeons necks they give various repreſentations, as they are variouſly looked on. A diſfavouring prepoſſeſſion brings even our good actions under the diſadvantage of ſuſpicion, and procures our indifferent ones the harſheſt conſtruction; where the intention is doubtful, men generally collecting it from their opinion of the perſon, and being diſpoſed to believe rather that he is faulty, than they miſtaken.”

It was about the ſame time, that he wrote his *Free Diſcourſe againſt cuſtomary Swearing*; the manuſcript of which in his own hand-writing is ſtill extant; and by the neatneſs and exactneſs of it appears not to have been the firſt draught, though it is dated *Anno MDCXLVII.* The firſt leaf has this title, *The Swearer ſilenced by* R. B. but in the firſt page it is called, *A Free Diſcourſe againſt cuſtomary Swearing.* It is dedicated to his ſiſter, the counteſs of *Kildare.* There is annexed to it, *A Diſſuaſive from Curſing, from Mr.* W. D. *to Sir* G. L. Theſe two pieces were publiſhed at *London*, 1695, in 8vo, by Dr. *John Williams*, afterwards biſhop of *Chicheſter*, who obſerves in his preface, that the *Diſcourſe* was the third treatiſe, which Mr. *Boyle*, though then but twenty years of age, had prepared for the public; his *Seraphic Love*, afterwards printed, and his *Eſſay on miſtaken Modeſty*, being referred to in this piece.

But the courſe of his ſtudies was interrupted for ſome time in the ſummer of the year 1647, by a ſevere fit of the ſtone, to which diſtemper he was extremely ſubject. However, in September following he went to *Briſtol* and *Saliſbury*; and in February, 1647-8, made a voyage to *Holland*, partly to viſit the country, and partly to accompany his brother *Francis* in conducting his wife from the *Hague*[a]. But he did not ſtay long there, for on the 15th of April, 1648, he was at *London*; and on the 13th of May at *Stalbridge*, whence he wrote to his ſiſter *Ranelagh*, giving her an account, that he ſhould poſſibly ſoon ſend her his thoughts upon the ſubject of *Toleration*, in an eſſay of his, intitled, *Of Divinity*[b]. He returned to *London* the ſame month, where he was likewiſe in July; and in a letter to the counteſs of *Monmouth* at *Moor-Park*, dated the 7th of that month, mentions a piece of his writing, then in the hands of that lady[c]. He was in Auguſt following at *Leez* in *Eſſex*, the ſeat of his ſiſter the counteſs of *Warwick*, where on the 6th of that month he finiſhed his treatiſe, intitled, *Some Motives and Incentives to the Love of God, pathetically diſcourſed of in a Letter to a Friend*: of which I ſhall give ſome further account below, when I have occaſion to mention the publication of it. But he was returned to *Stalbridge* on the 9th of September following; and on the 26th of March 1649, was at *Marſton* in *Somerſetſhire*, the ſeat of his brother, the lord *Broghill*; and on the 2d of Auguſt at *Bath*; in a letter from whence to his ſiſter *Ranelagh* of that date[d], he takes notice of his having been troubled for three or four weeks with a quotidian ague, "though in the intervals of the fit (ſays he) I both began and made ſome progreſs in the promiſed diſcourſe of *Public Spiritedneſs*. But now truly weakneſs, and the doctor's preſcriptions, have caſt my pen into the fire, though in ſpite of their menaces I ſometimes preſume to ſnatch it out a while, and blot ſome paper with it. My preſent employment is reviewing ſome *Conſolatory Thoughts on the Loſs of Friends*, which my poor lady *Suſan*'s death obliged me to entertain myſelf with, and which I am now recruiting." But on the 31ſt of that month he was returned to *Stalbridge*, as appears from a letter of his of that date to the ſame lady[e], wherein he writes thus: "I will not now preſume to entertain you with thoſe moral ſpeculations, with which my chemical practices have entertained me; but if this laſt ſickneſs had not diverted me, I had before this preſented you with a diſcourſe (which my vanity made me hope would not have diſpleaſed you) *of the theological uſe of natural philoſophy*, endeavouring to make the contemplation of the creatures contributory to the inſtruction of the prince, and to the glory of the author of them. But my blood has ſo thickened my ink, that I cannot yet make it run; and my thoughts of improving the creatures have been very much diſplaced by thoſe of leaving them. Nor has my diſeaſe been more guilty of my oblivion, than my employment, ſince it has begun to releaſe me; for *Vulcan* has ſo tranſported and bewitched me, that as the delights I taſte in it, make me fancy my laboratory a kind of Elyſium, ſo, as if the threſhold of it poſſeſſed the quality the poets aſcribed to that *Lethe* their fictions made men taſte of, before their entrance into thoſe ſeats of bliſs, I there forget my ſtandiſh and my books, and almoſt all things, but the unchangeable reſolution I have made, of continuing till death your, &c."

He was at *London* on the 15th of November, 1649; on the 20th of the ſame month at *Marſton*; on the 21ſt of December, and in the month of January following, again at *London*; and on the 1ſt of May, 1650, at *Stalbridge*, whence he wrote the following letter to Mr. *Hartlib*:

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[a] Mr. *Boyle*'s letter to Mr. *Marcombes*, dated from *London*, February 22, 1647-8, in which he mentions his intentions of ſetting out for *Holland* the next day.

[b] See his Works, Vol. VI. p. 45.

[c] Ibid. p. 47.

[d] Ibid. p. 48.

[e] Ibid. p. 49.

“ SIR,

“ I HAVE here ſo little time to diſpatch a great deal of indiſpenſible buſineſs in, “ that my knowledge of your goodneſs, and yours of my hurry, promiſes me “ your pardon for the neceſſitated fault of returning to ſo long and ſo excellent a “ letter ſo ſhort and ſo haſty an anſwer, which ought to be wholly employed in ac- “ knowledgments and thanks for the exact intelligence you are pleaſed to oblige me “ with from *Utopia* and *Breda*; my inclination as much concerning me *in Republicâ* “ *Literariâ*, as my fortune can do *in Republicâ Anglicanâ*. Nor am I here altogether “ idle, though my thoughts only are not at preſent uſeleſs to the advancement of “ learning; for I can ſometimes make a ſhift to ſnatch from the importunity of my “ affairs leiſure to trace ſuch plans, and frame ſuch models, as, if my Iriſh fortune “ will afford me quarries and woods to draw competent materials from, to conſtruct “ after them, will fit me to build a pretty houſe in *Athens*, where I may live to phi- “ loſophy, and Mr. *Hartlib*,

Stalbridge,
May-day, 1650.

“ A cordial friend, and not wholly uſeleſs ſervant,

“ R. B.

“ IN great haſte, which I beſeech you pardon.”

To this I ſhall add here another letter to the ſame gentleman, the date of which is wanting.

“ Dear Mr. *Hartlib*,

“ I AM ſure, that you have too much charity to want juſtice; and therefore on the “ ſcore of your ſerious promiſe I am bold, not only to deſire, but to expect at “ your hands a *Mercurius Philoſophicus*, in an account of the projects and ſucceſſes of “ that *college*, whereof God has made you hitherto the midwife and nurſe. I ſhall “ not beg any information from you of that *diurnal* news, of which a whole ſheet “ may be had for a penny, and yet be over-bought. No, I will ſtint my requeſts to “ your *Utopian* intelligence, as the only way to keep me in charity with men, by “ letting me ſee, that the degenerate world yet harbours ſome, that do not undeſerve “ the name. As for me, knowing, that my letters will be guilty of faults enough “ in rhetoric, without loading themſelves with any needleſs crimes in morality, I “ here openly diſclaim all compliments, and ſolemnly engage myſelf (as to me) to “ give them a perfect baniſhment from our converſe, wherein I am confident I ſhall “ have but a very eaſy office to ſurmount; ſince for my part I ever eſteemed them at “ beſt but the froth (not to ſay the ſcum) of civility; and for yours, I know the “ gallantry of your principles leads you to the nobleſt way of putting them out of “ countenance, by carrying you to doing men really more good than they dare pro- “ miſe. And ſince you do not diſdain the meaneſt workman, that is but willing to “ lay ſome few ſtones towards the building of your *college*, I ſhall in my following “ epiſtles (if this procure them a paſs) take the liberty to acquaint you with what “ thoughts and obſervations of mine I ſhall judge uſeful in reference to ſo glorious “ a deſign; to which I ſhall think it very much my happineſs, if any endeavours of “ mine can have the honour in the leaſt meaſure to contribute, not only as they owe

" a duty to the public (though, I muſt confeſs, that of itſelf a very prevalent motive)
" but becauſe I know you ſo vaſtly affectionate to that public, that my invention
" will furniſh me with no fitter way, than that of my ſervices to it, to give you real
" and accepted teſtimonies of my being, &c."

He had now gained ſo eminent diſtinction in the republic of learning, that in 1651 Dr. *Nathaniel Highmore*, a phyſician, who had been a member of *Trinity* college in *Oxford*, addreſſed to him his Treatiſe, printed at *London* that year, under the title of *The Hiſtory of Generation*; *examining the ſeveral opinions of divers authors, eſpecially that of Sir* Kenelm Digby *in his* Diſcourſe of Bodies. *With a general relation of the manner of Generation as well in plants as animals*; *with ſome figures delineating the firſt originals of ſome creatures, evidently demonſtrating the reſt. To which is joined a Diſcourſe of the Cure of Wounds by Sympathy, or without any real application of medicines to the part affected, but eſpecially by that power, known chiefly by the name of Sir* Gilbert Talbot'*s powder*. The dedication is dated from *Sherborne* May 15, 1651, with this inſcription, *To the honourable Mr.* Robert Boyle, *ſon to the right honourable the earl of* Corke, *my much honoured friend*; and begins thus:

" Noble Sir,
" WHERE virtue ſhall be found in conjunction with nobility in ſuch black,
" the laſt and worſt times, it no leſs invites and amazes the eyes and hearers
" of beholders, than ſome new ſtar or blazing comet; but with this difference, the
" one is cauſe of their fear, the other gives life to their hopes and joy. You have,
" Sir, ſo enriched your tender years with ſuch choice principles of the beſt ſort, and
" even to admiration managed them to the greateſt advantage, that you ſtand both a
" pattern and wonder to our nobility and gentry, who in theſe paſt times, many of
" them, have ſo ſpent their precious minutes, that they are ſcarce able to account for
" one, or ſpend an hour but in vice; that cannot brook virtue, becauſe it is not
" born with them; that hate all things, that muſt be obtained by induſtry; who
" moſt degenerately intruſting their wits as well as fortunes with their inferiors, have
" made them maſters of both; a ſad forerunner (I will not ſay author) of theſe ſadder
" times. But you have made a better and far nobler choice; you have not thought
" your blood and deſcent debaſed, becauſe married to the arts. You ſtick not to
" trace nature in her moſt intricate paths; to torture her to a confeſſion, though with
" your own ſweat and treaſure obtained."

It was about the year 1652, that Mr. *Boyle* began an *Eſſay on the Scripture*, ſtill extant in manuſcript, from which his *Conſiderations on the Style of the Scriptures* were chiefly taken. This is evident from a letter of his, dated June 19, 1652, to his brother the lord *Broghill*, at whoſe requeſt he compoſed that *Eſſay*; which letter, with a few alterations, was afterwards made the Epiſtle Dedicatory to the *Conſiderations on the Style*, &c. Under what diſadvantages the *Eſſay* was written, the reader is informed in the preface to the *Conſiderations*: " The Eſſay, *ſays he*, having not been
" all written in country, but partly in *England*, partly in another kingdom, and
" partly too on ſhip-board, it were ſtrange, if in what I writ, there did not appear
" much of unevenneſs, and if it did not betray the unleiſuredneſs, and reliſh of the
" unſettledneſs of the wandering author, who by thus rambling was reduced, for
" want of a library, to comply with the requeſt of his friend, who was more deſirous
" to receive from the author apples and pears growing in his own orchard, than
" oranges and lemons fetched from foreign parts." A diſcourſe written in this

manner, without any foreign aſſiſtance, and under ſo many diſadvantages in the exerciſe of his own talents, ſhews his abilities in a much more conſpicuous light, than if he had compiled ſome finiſhed piece with the uſual helps, and at a time of leiſure and freedom from diſtractions. The public will undoubtedly be glad to ſee his thoughts concerning the laſt Engliſh tranſlation of the Bible, and of the advantage of underſtanding the original languages of the ſcriptures, with ſome account of his own application to the ſtudy of them, extracted from ſome looſe ſheets, intended as a part of his *Eſſay on the Scripture*.

“ THOUGH I think, ſays he, the authors of our laſt tranſlation have made it much
“ more correct than our former was, and preferable to moſt I have met with in other
“ languages; and have therefore been ſincerely and induſtriouſly inſtrumental to the
“ Britiſh churches edification, according to their light; yet that it were very poſſible,
“ as well as to be wiſhed, that an Engliſh tranſlation, yet in many places more cor-
“ rect, might be framed, will ſcarce ſeem improbable to an intelligent perſon, that
“ duly conſiders, how much the knowledge of tongues, and (what would be highly
“ uſeful, not to ſay neceſſary, in ſuch a work) of Jewiſh and other Eaſtern learning,
“ is, ſince our laſt verſion was made, encreaſed and refined, and conſequently, how
“ many texts by that greater light, that now ſhines among critics and antiquaries,
“ may be rendered more fully, or more warily, or more clearly, or more cohe-
“ rently to the context, or more congruouſly to the analogy of faith, or that of
“ reaſon.

“ I CANNOT but reckon the want of (not ſuperficial rudiments, but) ſolid know-
“ ledge and practical readineſs in the originals of the Bible amongſt the chief impe-
“ diments of our diſcovery of the נמלאות *niphlaſt*, or unobvious rarities of that
“ divine book, from which he much derogates that ſtints them to thoſe diſcernible
“ in a tranſlation. But I pretend not, by what I have now ſaid, or reſerve to ſay
“ elſewhere, either to impoſe a neceſſity of learning Greek and Hebrew on thoſe,
“ whoſe incapacities or employments deny them the requiſites or conveniences of ſuch
“ a ſtudy; or to diſcourage thoſe that cannot underſtand the Scriptures in its ori-
“ ginals, for reading it in tranſlations; for (at leaſt among his own children) he, that
“ requires leſs, where leſs hath been intruſted, will not exact a meaſure of knowledge
“ diſproportionate to the means he hath vouchſafed of it; and tranſlations of the
“ Bible may be very effectual to God's purpoſes in it, though not ſo much ſo as their
“ originals to all, eſpecially as to the practical maxims of religion. Experience makes
“ it probable, that tranſlations hold them forth clearly and unanimouſly enough
“ to make their faithful practiſers acceptable unto God through Jeſus Chriſt, as the
“ Egyptians were wondrouſly enriched by the river *Nilus*'s ſtreams flowing from an
“ unknown fountain. To which purpoſe I remember, that that excellent Chriſtian
“ ſcholar and divine Dr. *Uſher* archbiſhop and primate of *Armagh* (whoſe encourage-
“ ments I gratefully acknowledge to have much engaged me to the ſtudy of the holy
“ tongues) talking to me one day in his ſtudy of his intended edition of the Septua-
“ gint, earneſtly implored of him by the tranſmarine critics, obſerved to me, not
“ without ſome juſt wonder, that for divers ages the church (as to the greater part
“ of it) and even the apoſtles, and even thoſe that moſt flouriſhed in piety, ſcarce
“ uſed any other ſcripture than the Septuagint's tranſlation, which is one of the
“ faultieſt verſions of the Bible, and wants not groſs miſtakes. And indeed, ſo
“ there be a devout, induſtrious, and modeſt ſearch after the truths of ſcripture
“ proportionable to the abilities and opportunities that God vouchſafes, and a con-
“ ſcionable conforming of our lives to our diſcoveries, it is much to be hoped, that

“ we may miſs a great many theological truths without miſſing ſalvation. But as I
“ ſhall not exact the ſtudy of the original from thoſe whoſe want of parts or leiſure
“ diſpenſeth them from it; ſo cannot I but diſcommend thoſe, who wanting neither
“ abilities, time, nor conveniences to range thorough I know not how many other
“ ſtudies, can yet decline this; and who ſparing no toil nor watches to put it out of
“ the power of the moſt celebrated philoſophers to deceive them in another doctrine,
“ leave themſelves obnoxious to the ignorance, fraud, or partiality of an interpreter
“ in that of ſalvation; and thereby ſeem more ſhy of taking any opinions upon truſt,
“ than thoſe, in whoſe truth or falſeneſs no leſs than God's glory, and peradventure
“ their own eternal condition, is concerned. Methinks thoſe, that learn other languages,
“ ſhould not grudge thoſe that God hath honoured with ſpeaking to us, and
“ employed to bleſs us with that heavenly doctrine, that comes from him, and leads
“ to him. When I have come into the Jewiſh ſchools, and ſeen thoſe children that
“ were never bred up for more than tradeſmen, bred up to ſpeak (what hath been
“ peculiarly called) God's tongue, as ſoon as their mother's, I have bluſhed to think,
“ how many gown-men, that boaſt themſelves to be the true *Iſraelites*, are perfect
“ ſtrangers to the language of *Canaan*; which I would learn, were it but to be able
“ to pay God the reſpect uſual from civil inferiors to princes, with whom they are
“ wont to converſe in their own languages. For my part, I * * * that have a memory
“ ſo unhappy and ſo unfit to [ſupply] my intellectual deficiencies, and the reſt
“ of my diſabilities, that it often ſtrongly tempts me to give over my ſtudies, and
“ abandon an employment, wherein my ſlow acquiſts are (by the treacherouſneſs of
“ my memory) ſo eaſily loſt; beſides this diſadvantage, I ſay, thoſe excellent ſciences,
“ the mathematics, having been the firſt I addicted myſelf to, and was fond of, and
“ experimental philoſophy with its key, chemiſtry, ſucceeding them in my eſteem
“ and applications; my propenſity and value for real learning gave me ſo much
“ averſion and contempt for the empty ſtudy of words, that not only I have viſited
“ divers countries, whoſe languages I could never vouchſafe to ſtudy, but I could
“ never yet be induced to learn the native tongue of the kingdom I was born and
“ for ſome years bred in. But in ſpite of the greatneſs of theſe indiſpoſitions to the
“ ſtudy of tongues, my veneration for the ſcripture made one of the greateſt deſpiſers
“ of verbal learning leave *Ariſtotle* and *Paracelſus* to turn grammarian, and
“ where he could not have the help of any living teacher, engaged him to learn as
“ much Greek and Hebrew, as ſufficed to read the old and new teſtament, merely
“ that he may do ſo in the Hebrew and Greek, and thereby free himſelf from the
“ neceſſity of relying on a tranſlation. And after I had almoſt learned by rote an
“ Hebrew grammar, to improve myſelf in ſcripture-criticiſms, in the Jewiſh way of
“ reading the oracles committed to them, I, not over-cheaply, purchaſed divers
“ private conferences with one of their ſkilfulleſt doctors (as St. *Hierom* had thoſe
“ nocturnal meetings, which ſo much helped to make him the ſolideſt expoſitor of
“ all the fathers, with *Barraban* or * * * the Jew) I received of him few leſſons, that
“ coſt me not twenty miles riding, at a time, when I was in phyſic, and my health
“ very unſettled. A Chaldee grammar I likewiſe took the pains of learning, to be
“ able to underſtand that part of *Daniel*, and thoſe few other portions of ſcripture,
“ that were written in that tongue; and I have added a Syriac grammar purely to
“ be able one day to read the divine diſcourſes of our Saviour in his own language:
“ in which I can truly profeſs, with the famous publiſher of the Syriac teſtament,
“ *Guido Fabricius* (in his dedication of that book, and his verſion of it, to the then
“ *French* King) that I had no inſtructor to teach me ſo much as to know the letters,

“ but have been, to uſe the words he borrows of the learned *Budæus*, αὐτοδίδακτος καὶ
“ θεοδίδακτος, have had no other living teacher but God and myſelf in the little
“ grammatical learning I have acquired in thoſe four tongues, in which the better
“ underſtanding and reliſhing of the ſcripture limit my pretenſions. Nor do I at all
“ repent my labour, though, to ſecure my progreſs and acquiſts in theſe languages,
“ my bad memory ſtill reduces me to a conſtant and frequent recollection of ſome
“ choice inſtitutions of them all. For certainly the ſatisfaction of underſtanding
“ God, and thoſe excellent perſons celebrated even in his book, expreſs themſelves
“ in their own very terms and proper languages, doth richly recompenſe the pains
“ of learning them; for according to the known ſaying,

“ *Quamvis allatâ gratus ſit ſaper in undâ,*
“ *Dulcius ex ipſo fonte bibuntur aquæ.*

“ Though we ſtream-waters not unpleaſant think,
“ Yet with more guſto of the ſpring we drink.

“ IT is true, that a ſolid knowledge of that myſterious language, God and his
“ prophets ſpake (whatever is given out to the contrary by ſuperficialiſts, amongſt
“ whom I remember a Jewiſh profeſſor of my acquaintance uſed to reckon many
“ that are thought and think themſelves Hebricians, becauſe they could without
“ heſitation and the help of a tranſlation or a dictionary read and render in their own
“ tongue an Hebrew chapter) is, I ſay, ſomewhat difficult, but not ſo difficult, but
“ that ſo ſlow a proficient as I could in leſs than a year, of which not the leaſt part
“ was uſurped by frequent ſickneſſes and journies, by furnaces, and by (which is
“ none of the modeſteſt thieves of time) the converſation of young ladies, make a
“ not inconſiderable progreſs towards the underſtanding of both teſtaments in both
“ their originals. * * *

“ FOR my part, that reflect often on *David*'s generoſity, who would not offer as
“ a ſacrifice to the Lord his God that which coſt him nothing, I eſteem no labour
“ laviſhed, that illuſtrates or endears to me that divine book; my addictedneſs to
“ which I gratulate to myſelf, as thinking it no treacherous ſign, that God loves a
“ man, that he inclines his heart to love the ſcriptures, where the truths are ſo pre-
“ cious and important, that the purchaſe muſt at leaſt deſerve the price. And I con-
“ feſs myſelf to be none of thoſe lazy perſons, that ſeem to expect to obtain from
“ God the knowledge of the wonders of his book upon as eaſy terms as *Adam* did
“ a wife, by ſleeping profoundly, and having her preſented to him at his awaking.”

IN the ſame year 1652 he went to *Ireland*, whence he wrote the following letter in January 1652-3 to *his highly honoured friend John Mallet, Eſq; at Poynington, near Sherburne in Dorſetſhire:*

“ SIR,

“ IT is ſo long ſince that I have been made happy in an unanſwered letter of
“ yours, that it would now almoſt as juſtly produce my bluſhes that it hath lain
“ ſo long unacknowledged, as it did then my ſatisfaction to find it ſo obliging; if it
“ had not ſo abruptly come to my hands, and with ſo many other papers, that till I
“ was this afternoon informed by Mr. *Lillyes*, that he ſent it me, I neither knew by
“ whom you had written it me, nor by whom I might ſecurely return you for it my
“ highly merited thanks. But though this ignorance leſſened, it deſtroyed not the

“ happineſs I received in a paper, whoſe writer kept it from needing endearing cir-
“ cumſtances. And certainly, without a huge deal of inſenſibility, I could not but
“ be highly ſenſible of a civility, that came to ſeek me out in a country, which thoſe,
“ that have the moſt relation to it, ſeldom think on any longer than they are in it.
“ Nor was I only mightily affected to find the welcome continuation of your friend-
“ ſhip for me, but very much delighted to find too, that you began to have a friend-
“ ſhip for the Eaſtern tongues. For though to a perſon ſo uſed to the ſtudy, and
“ repleniſhed with the knowledge of things, as Mr. *Mallet*, the learning of words
“ cannot but at firſt be very tedious; yet ſince to be a good grammarian is neceſſary
“ to be a good divine; and he that hath no ſkill in the original ſcripture himſelf,
“ may be deluded by thoſe that tranſlate it for him; you will find a rich compenſa-
“ tion for the trouble of learning the holy tongue in the advantages of having learned
“ it; and by the help of that primitive language, wherein they were written, you
“ may gain a free and ſafe acceſs to thoſe theological myſteries, which he that is no
“ linguiſt, muſt either totally ignore, or take upon truſt. And certainly a know-
“ ledge ſo directly conducive to that knowledge that is called *life eternal*, cannot but
“ deſerve the being laboured for. And to ſatisfy you, that the difficulties of attain-
“ ing ſo precious a knowledge are not inſuperable, I ſhall tell you, that I have a
“ young kinſwoman newly marriageable, who to the French and Latin hath added
“ ſkill enough in the Greek and Hebrew tongues to read the two teſtaments in them.
“ I am glad to hear by Mr. *Lillyes*, that you have got ſo good an inſtructor as Mr.
“ *Bythner*, whoſe ſhort grammar of the laſt edition is that for which I left off many
“ others of the moſt approved, after having ſurveyed them all. But his inſtitutions
“ being almoſt confined to the etymological part of grammar, are chiefly proper for
“ a beginner; and therefore, when you are proficient enough to decline moſt nouns,
“ and conjugate (though not all verbs) all ſorts of verbs; it would be requiſite for
“ the ſyntactical part of grammar to have recourſe to ſome that have handled it
“ expreſly; wherein next *Glaſſius* (whoſe works are voluminous enough, and of
“ various ſubjects, but, in my green judgment, theological and excellent) the elder
“ *Buxtorfius* hath been the moſt full and accurate of thoſe I have yet met with. There
“ is likewiſe an old book of *Flacius Illyricus*, which he calls *Clavis Scripturæ*, which,
“ though little taken notice of, is more than a little uſeful, and very well worth
“ peruſing; ſince giving the reader a particular account of the Hebraiſms, as they
“ lie in ſcripture, he oftentimes teaches him at once both grammar and divinity. But
“ I muſt beg your pardon for troubling you with theſe perhaps needleſs advertiſe-
“ ments: I have cauſe to believe Mr. *Bythner*’s will be much riper; only the pre-
“ judice I ſuſtained, by not having ſeaſonably an addreſs to the authors I have
“ named, makes me willing, rather to hazard the exerciſe of your patience by ſuper-
“ fluous directions, than venture to let you miſpend any of your ſtudious hours, for
“ want of ſuch addreſſes, as are not bad, though they prove not the very beſt. I
“ forget to tell you, that there is one *Gerhard* (ſon of the famous Lutheran divine)
“ who hath publiſhed an Harmonical Synopſis of the Hebrew, Chaldee, Syriac,
“ Arabic, and Ethiopic tongues; which would be no bad iſagoge to the Eaſtern
“ languages, if it were not ſo wretchedly falſe printed, that it expoſes the learned in
“ almoſt every page to perplexities or miſtakes. I wiſh I could give you ſome
“ account of my ſtudies here; but I muſt ſadly confeſs, that the perpetual hurry
“ I live in, my frequent journeys, and the neceſſary trouble of endeavouring to
“ ſettle a very long neglected and disjointed fortune, have left me very little time to
“ converſe with any book, ſave the Bible, and ſcarce allowed me leiſure to ſew toge-

" ther ſome looſe ſheets, that contained my thoughts about the ſcripture, of which
" I wiſh I had a copy to ſubmit to your cenſure. But I hope by God's bleſſing to be
" able to bring over one myſelf this ſpring into your parts; to which it is not my
" leaſt of invitations, that I ſhall there enjoy the happineſs of a converſation that
" hath made me upon ſo many ſcores,

" SIR,

" your moſt affectionate friend,

" and obliged humble ſervant,

" ROBERT BOYLE."

" MY moſt humble ſervice, Sir, I beſeech you, to Sir *Thomas Mallet* and my lady,
" and the fair young lady you are happy in. And I ſhall beg you to do me the fa-
" vour to convey my high reſpects to the *Sherbone* family, when you go thither. The
" news here is not conſiderable enough to deſerve your notice. My friends here are,
" God be praiſed, well, and thoſe that have the happineſs to know you, much your
" ſervants."

HE came back from *Ireland* to *England*, probably about Auguſt 1653, and was at *London*, where he met with Dr. *John Wilkins*, then warden of *Wadham* college in *Oxford*, and afterwards biſhop of *Cheſter*, to whom he intimated his reſolutions of ſettling at *Oxford*[f]. In September he was a few days at *Stalbridge*; but being obliged by his affairs to return to *Ireland*, in his way thither he wrote the following letter to his friend Mr. *Mallet*:

" SIR,

" IF the ſuddenneſs of that unwelcome neceſſity, which hurried me from *Stal-*
" *bridge*, would have allowed me ſo much as two days ſtay in thoſe parts, I ſhould
" not be now reduced to make you an apology for my leaving *Stalbridge*, before I
" kiſſed your hands at *Poynington*. And though the urgency of my occaſions allowed
" me but one day to pack up bag and baggage, and ſettle my affairs there, I ſhould
" have ſpent part of that day in paying you the duty, and giving myſelf the ſatiſ-
" faction of a viſit, if the greateſt obſtacle my haſte put to my deſires, had not
" been the tiredneſs of my horſes by my preceding journies. The apprehenſion of
" loſing my paſſage, unleſs my arrival here prevented the feared change of the wind,
" poſted me ſo abruptly hither, that it allowed me to comply neither with my
" civility, nor with my inclinations, the latter of which are never more gratified
" than in your charming converſation, which the wind's croſſneſs ſince my arrival here
" makes me very much regret I did not venture to give myſelf the happineſs of.
" The ſad condition of *Ireland* making me ſomewhat irreſolute of my going thither
" this winter, I do not wholly deny myſelf the pleaſing thoughts of being happy
" in your company at *Poynington*. But in caſe a fair wind blow away thoſe thoughts,
" I muſt implore, in the want of your immediate converſation, that which is practiſed

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[f] See Dr. *Wilkins*'s Letter of Sept. 3, 1653, Vol. VI. p. 633, 634.

“by letters, as the next contentment to the former by nearlieſt approaching it. In the neceſſity of repairing into *Ireland* to ſettle my affairs there, now things ſeem tending to a ſettlement in that unhappy country, I ſhall leave behind me at *Stalbridge* the bearer, *Roger Ball*, and with him a leaſe of that manor for ſome yet unexpired years; in which leaſe you were pleaſed to allow me to have your name put in as a truſtee for me. The reaſon of my mentioning this to you is, that by the miſtaking confidence of honeſt old Mr. *Mawdeſley*, then ſteward of my courts at *Stalbridge*, I made ſeveral grants there, as preſuming I had power to do ſo; which, upon more knowledge of the law, and a more heedful peruſal of my own more than ordinarily ſtrict entail, I found, though hardly overthrowable in equity, yet to be queſtionable in ſtrictneſs of law. My juſt tenderneſs in caſes of this nature made me extremely troubled, that my ignorance had made me do what knowingly nothing ſhould; and therefore by Sir *Thomas Mallet*'s excellent advice, finding a leaſe of *Stalbridge* for about thirty years unexpired to remain in the hands of an old ſervant of my father's, I made it one of my motives to go into *Ireland*, and one of my concerningeſt buſineſſes there, to get this leaſe aſſigned over in truſt to yourſelf and *Roger Ball*, with whom I have left order, that in caſe God ſhould call me to himſelf before my return to *Stalbridge*, he ſhall beg you to join with him in making every tenant, whoſe grant is queſtionable, a leaſe of as many years, as your authority extends to grant, that ſo theſe poor men, in caſe their titles (contrary to my expectation and to probability) ſhould be overthrown as to their firſt grants, may have a good title to all the unexpired years of your leaſe; and that number, according to common eſtimation, will be as advantageous, if not better, than their firſt grants. I find, Sir, no ſmall trouble in the neceſſity of giving you this great one; but I am confident you are ſuch a friend to juſtice, that you will pardon a fault that proceeds only from ſome tenderneſs of that virtue in a ſervant of yours, who very well knowing gratitude to be a virtue as well as juſtice, if not part of it, is not made more deſirous by the laws of the former virtue to ſecure his tenants, than the duties of the latter will make him of the opportunity of expreſſing and evincing, how highly he is Mr. *Mallet*'s

“affectionate, faithful, and obliged

Briſtol, Sept. 23, 1653.

“humble ſervant,

“ROBERT BOYLE.

“I SHALL beg the favour of having my moſt humble ſervice preſented to Sir *Thomas Mallet* and my lady, and to your own fair lady, with the reſt of your family; to all which you have engaged me to be a ſervant. If the bearer ſhall, in my abſence, apply himſelf to you for directions, your vouchſafing them to him will very much oblige me. If you pleaſe to make me at any time happy in your letters, it will as well inſtruct as ſatisfy me, to be informed, how you proceed in relation to your Eaſtern ſtudies.”

HE purſued his intended journey to *Ireland*; for we find him there in January 1653-4[g], where he propoſed to continue till Midſummer 1654[h], though his reſidence in that kingdom was by no means agreeable to him, ſince in a letter[i], which he wrote in the beginning of that year to Mr. *Clodius*, an eminent chemiſt, who married the daughter of Mr. *Hartlib*, he ſtiles it " a barbarous country, where chemical ſpirits " were ſo miſunderſtood, and chemical inſtruments ſo unprocurable, that it was hard " to have any Hermetic thoughts in it." For this reaſon he exerciſed himſelf in making anatomical diſſections, being aſſiſted therein by his friend Dr. (afterwards Sir) *William Petty*, then phyſician to the army and to the deputy of *Ireland*, and one of the moſt extraordinary men of his time, who in 1648, at the age of twenty-five, inſcribed to Mr. *Hartlib*, *Advice for the Advancement of ſome particular parts of Learning*, printed at *London* in 4to; in which he gives plans of a mechanical and medical college, wherein youths might be educated, and the hiſtory of all mechanical arts and trades tranſmitted to poſterity; and this piece ſeems to have been one of the firſt draughts of a philoſophical college that was ever made in *England*. Mr. *Boyle* was brought acquainted with this gentleman by means of Mr. *Hartlib*, as early as the year 1647[k], and received great advantage, while he continued in *Ireland*, from his converſation and their joint ſtudies in anatomy; in the courſe of which " I ſatisfied " myſelf (ſays he[l]) of the circulation of the blood, and the freſhly diſcovered and " hardly diſcoverable *receptaculum chyli* made by the confluence of the *venæ lacteæ*, " and have ſeen (eſpecially in the diſſections of fiſhes) more of the variety and con- " trivances of nature, and the majeſty and wiſdom of her author, than all the books " I ever read in my life could give me convincing notions of."

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[g] See Mr. *Hartlib*'s letter to him of Feb. 28, 1653-4, Vol. VI. p. 78.

[h] Ibid. p. 55, and 83.

HE made alſo ſtrict enquiries after the minerals, which *Ireland* afforded; and though he could meet with few, who had either ſkill or curioſity in that way, yet ſilver ore was brought to him, which was found upon one of his brother's eſtates, that upon trial was eſtimated to be worth between thirty and forty pounds a tun. A ſilver mine was then farmed of the ſtate; and he was aſſured by experienced men, that *no country in Europe was ſo rich in mines as Ireland, had but the inhabitants the induſtry to ſeek them, and the ſkill to know them*[m].

AFTER his return to *England*, which was probably, according to his intentions, in the latter end of June, 1654, he went to reſide at *Oxford*, in order to proſecute his ſtudies with the greater advantage, and continued there for the moſt part till April 1668, when he ſettled at *London* in the houſe of his ſiſter *Ranelagh* in *Pall-Mall*. At *Oxford* he choſe to live in a private houſe, rather than in a college, both for his health, and becauſe he had more room and conveniency to make experiments, than he could have had in a ſociety. The perſon, with whom he lodged was Mr. *Croſſe*, an apothecary, whoſe great acquaintance with Dr. *John Fell*, afterwards biſhop of *Oxford*, (who made him one of the executors of his pious and charitable will) and his own noble foundation of an hoſpital near *Ampthill* in *Bedfordſhire*, for the maintenance of thirteen decayed citizens of *Oxford*, give him a right to be mentioned with honour in this hiſtory[n]. *Oxford* indeed was the only place in *England*, where at that time Mr. *Boyle* could have lived with much ſatisfaction to himſelf. Dr. *Wilkins*, who had married a ſiſter of *Oliver Cromwell* the Protector, was a man of excellent temper, and admirable abilities, and by the influence of his example and authority, ſupported a ſpirit of rational piety, and a right taſte of learning in that univerſity. Dr. *John Wallis* and Dr. *Seth Ward*, the two Savilian profeſſors of geometry and aſtronomy, Dr. *Thomas Wills*, the phyſician, then ſtudent of *Chriſt-Church*, Mr. *Chriſtopher Wren*, then fellow of *All-Souls* college, Dr. *Goddard*, warden of *Merton* college, Dr. *Ralph Bathurſt*, fellow of *Trinity*, and afterwards preſident of the ſame, and dean of *Wells*, were eminent likewiſe for their genius and application to the moſt uſeful parts of literature, which theſe great men united their endeavours to cultivate and promote. For this purpoſe they held frequent meetings, in which they conferred chiefly on philoſophical ſubjects; and being ſatisfied, that there was no certain way of arriving at any competent knowledge, unleſs they made a variety of experiments upon natural bodies, in order to diſcover what phænomena they would produce, they purſued that method by themſelves with great induſtry, and then communicated their diſcoveries to each other.

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[i] Vol. VI. p. 54. [k] Ibid. p. 76. [l] Ibid. p. 55. [m] Ibid.
[n] Dr. *Wotton*'s papers, c. iii. § 1.

THIS was an employment, and this a ſociety, which exactly ſuited Mr. *Boyle*'s inclinations. He had before laid in a great ſtock of mathematical and chemical knowledge: and as for the *Ariſtotelian* way of explaining the phænomena of nature, he rejected it, as a mere ſyſtem of words, that would never make any man more intelligent than he was before. The *Carteſian* philoſophy began to make a noiſe in the world; but he reſolved to acquieſce in no ſingle man's hypotheſis, and to draw no concluſions from premiſes in natural things, which he could not actually verify himſelf; and ſo for many years he would not read over *Des Cartes*'s *Principles*, leſt he might be biaſſed by the ingenuity or authority of that philoſopher. With theſe diſpoſitions he ſet himſelf to philoſophiſe, and to perſuade the nobility and gentry of the nation, who had the means and leiſure to purſue ſuch ſorts of ſtudies, to follow his example. He was convinced, that it would be of ineſtimable uſe to mankind to engage them in theſe enquiries; it would divert them from thoſe impertinent and criminal amuſements, with which moſt of them buſied themſelves, and would make them not only better Chriſtians, but likewiſe more uſeful members of ſociety.

IT was during Mr. *Boyle*'s reſidence at *Oxford*, that he invented that admirable engine, the *Air-Pump*, which was perfected for him in 1658 or 1659, by the very ingenious Mr. *Robert Hooke*, after he had ſeen a contrivance for that purpoſe by Mr. *Garterix*, which was too groſs to perform any great matters[o]. Mr. *Hooke*, who was afterwards profeſſor of geometry in *Greſham* college, and doctor of phyſic, then lived with Mr. *Boyle*, whom he aſſiſted in chemiſtry, having been recommended to him by Dr. *Willis* the phyſician, whom he had before ſerved in the ſame capacity[p]. By this engine Mr. *Boyle* made ſuch experiments, as have gone very far to enable him, and thoſe who have ſucceeded him, to form a juſt theory of the air. By this he demonſtrated its elaſticity; and that property alone was a means to find out abundance more. He begun alſo to compoſe hiſtories of its particular qualities, all founded upon experiments or obſervations, of which he kept very exact regiſters, hoping by this means to leave ſuch materials, as future ages might build complete theories upon. There were at that time no philoſophical ſocieties erected in other parts of *Europe*: what men had hitherto performed, they had moſtly done upon their ſingle ſtock, and that rather by the way of abſtracted reaſonings, than by making experiments. In *Italy* indeed the great *Galileo*, and his ſcholar *Evangeliſta Torricelli*, had made ſome experiments, which had excited the curioſity of mankind; as did eſpecially that famous one of the former, in which by a tube filled with water, inverted and plunged into a veſſel of the ſame, he kept a cylinder of it ſuſpended at the height of thirty-four feet; by which the weight of the air was firſt demonſtrated. The philoſophy of *Des Cartes* had for ſome years been publickly taught in *Holland*; and though built upon principles very oppoſite to thoſe either of *Ariſtotle*, or the chemiſts, yet had not the neceſſary vouchers of repeated experiments, purpoſely tried, to make it good. However that philoſopher had done eminent ſervice, in leſſening the devotion which had been implicitly paid to *Ariſtotle*'s philoſophy, and infuſing an inquiſitive ſpirit into mankind. A diſpoſition to think freely concerning the cauſes of natural things, had now ſpread itſelf in *England*, *France*, and *Italy*; and that diſpoſition occaſioned all thoſe alterations in men's ways of ſtudy, and produced all thoſe extraordinary diſcoveries of nature, which diſtinguiſhed the laſt age.

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[o] Life of Dr. *R. Hooke*, by *R. Waller*, Eſq; p. 3.

[p] Mr. *Ward*'s lives of the profeſſors of *Greſham* college, p. 170.

BUT philoſophy and enquiries into nature were not the only things which engaged Mr. *Boyle*'s attention. His eyes were then ſtrong enough to follow thoſe critical ſtudies which he had begun before, and which he thought neceſſary to underſtand the ſcriptures thoroughly: and for this he had peculiar advantages at *Oxford*. Dr. *Edward Pococke*, Mr. *Thomas Hyde* of *Queen*'s college, and Mr. *Samuel Clarke*, who were men of great eminence for their ſkill in the Eaſtern languages, reſided there; and Mr. *Hyde* particularly was frequently conſulted by him during the reſt of his life upon any difficulties which he met with in the courſe of his reading upon thoſe ſubjects. But one of his moſt intimate friends, with whom he converſed upon theological points, was Dr. *Thomas Barlow*, then chief library-keeper of the *Bodleian* library, and afterwards biſhop of *Lincoln*. The doctor was a man of prodigious reading, and a proportionable memory; he knew what the fathers, ſchoolmen, or canoniſts had ſaid upon any queſtion in divinity, or caſe of conſcience; and being with all theſe accompliſhments very communicative of his knowledge, he gained the higheſt degree of Mr. *Boyle*'s eſteem and friendſhip, who uſed, as long as he lived, to conſult him upon caſes of conſcience, wherein he was ſure at the ſame time of learning all that had been hitherto urged upon the queſtion propoſed to Dr. *Barlow*[q], many of whoſe anſwers are ſtill extant. But however important theſe advantages were, which he enjoyed at *Oxford*, for the proſecution of his ſtudies, he extended them by a correſpondence with perſons eminent for their knowledge in other parts of *England*, and in foreign countries. Among theſe correſpondents, one of the principal was *Henry Oldenburg*, Eſq; a native of *Bremen* in *Lower Saxony*, who was for ſeveral years agent for that city in *England*, and afterwards tutor to the lord *Dungarvan*, nephew to Mr. *Boyle*, with whom he travelled abroad in the year 1657. He was at laſt ſecretary to the Royal Society, and died ſuddenly in September 1677, and his wife, the daughter of Mr. *John Dury*, dying about the ſame time, left two orphans, whom Mr. *Boyle* took care of for the preſent, no will of Mr. *Oldenburg* being found, nor had he or his wife any relation in *England*. The firſt letter extant of this gentleman to Mr. *Boyle* is dated April 15, 1657; and in another written from *Saumur* in *France* in the latter end of that year, he mentions his[r] ſatisfaction, that the council had granted Mr. *Boyle*'s deſires for the promotion of knowledge; “ which (ſays he) I ſuppoſe to be thoſe, that
“ were couched in a certain petition you were pleaſed to impart unto me at *Oxford*;
“ wherein, if I remember well, a matter of twelve thouſand pounds ſterling was of-
“ fered to purchaſe confiſcated lands and houſes with in *Ireland*, and to commit the
“ profit thereof into the hands of certain truſtees, for to employ it in the entertain-
“ ment of an agent, ſecretary, tranſlators, for keeping intelligence, diſtributing re-
“ wards, *&c.* in order to the end aforeſaid.”

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[q] Dr. *Wotton*'s papers.

[r] Vol. VI. p. 142.

DR. *John Beale*'s letters to him are very numerous, and upon ſubjects of great importance. This divine was born of a good family in *Herefordſhire*, and educated at *Eton* ſchool, whence he was tranſplanted to *King*'s college in the univerſity of *Cambridge*, where he ſoon diſtinguiſhed himſelf by his uncommon genius and learning. He was one of the firſt members of the Royal Society, and in 1665 was promoted to be chaplain to king *Charles* II. He was likewiſe doctor of divinity, and reſided chiefly at his living of *Yeovil* in *Somerſetſhire*, where he died about the year 1683. There are ſeveral of his papers in the *Philoſophical Tranſactions*.

ANOTHER of Mr. *Boyle*'s correſpondents, and moſt intimate friends, was *John Evelyn*, Eſq; of *Wotton* in *Surrey*, born there on the 31ſt of October, 1620, and educated at *Baliol* college in *Oxford*; who having travelled for ſeven years into the politer countries of *Europe*, upon his return to *England*, was one of the firſt of thoſe gentlemen, who met for the eſtabliſhment of the Royal Society. He paſſed through ſeveral conſiderable poſts in the reigns of king *Charles* II. *James* II. and *William* III. and died at *London*, February the 27th, 1705-6, in the 86th year of his age[s]. His works are of various kinds and very valuable; and Dr. *Burnet*[t] ſtiles him *a moſt ingenious and virtuous gentleman*; and informs us, that he was *not ſatisfied to have advanced the knowledge of this age by his moſt uſeful and ſucceſsful labours about planting and divers other ways, but was ready to contribute every thing in his power to perfect other mens endeavours.* His acquaintance with Mr. *Boyle* began, as he obſerves himſelf, in his letter to Dr. *Wotton*[u], “from courteous viſits he made me at my houſe at *Say's-Court*[w], which I as conſtantly repayed; ſo it grew reciprocal and familiar, divers letters paſſing between us at firſt in civility, and the ſtile peculiar to him upon the leaſt ſenſe of obligation. But thoſe compliments laſted no longer than till we became perfectly intimate, and had diſcovered our inclinations of cultivating the ſame ſtudies and deſigns, eſpecially in ſearch of natural and uſeful things, myſelf then intent on collections of notes in order to an *Hiſtory of Trade*, and other mechanical furniture, which he earneſtly incited me to proceed in. So that our intercourſes of letters were now moſtly upon that ſubject, and were rather ſo many receipts and proceſſes, than matter of letters; what I gathered of this nature, (eſpecially for the improvement of *Gardening* and *Sylva*, *Kalendarium*, *Acetaria*, &c. being but part of that work, a plan whereof is publiſhed) would aſtoniſh you to ſee... Theſe deſigns and apparatuſes growing beyond my forces, were left imperfect, upon the reſtoration of the baniſhed King, when every body expected a new world, and had other proſpects than what the melancholy and almoſt deſpair ſuggeſted to paſs away anxious thoughts by thoſe innocent employments I have mentioned; the eſtabliſhment of the Royal Society taking in all theſe ſubjects; which made our perſonal meeting (unleſs at *Greſham* college, where we aſſiduouſly met and converſed) leſs neceſſary.” Among Mr. *Evelyn*'s letters is a very long and curious one[x], dated at *Say's-Court*, Sept. 3, 1659, and containing a ſcheme of a *Philoſophical College*, in which he wiſhed to have Mr. *Boyle* for a fellow-member, *who is alone*, ſays he, *a ſociety of all that were deſirable to a conſummate felicity*. And on the 29th of the ſame month he wrote to him another letter[y], upon reading the manuſcript of Mr. *Boyle*'s *Seraphic Love*, which he had done with ſo high a ſatisfaction, that he obſerves at the concluſion of his letter, that “he extremely loved Mr. *Boyle* before, but that his heart was infinitely knit to him now. For what (ſays he) are

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[s] General Dictionary, Vol. V. p. 126—129.

[t] Hiſtory of the Reformation, Part III. B. 3. p. 417.

[u] Dated at *Wotton* in *Surrey*, Sept. 10, 1703.

[w] In *Deptford*.

[x] Vol. VI. p. 288, 289, 290, 291.

[y] Ibid. p. 291.

" we now to expect from ſo timely a conſecration of your excellent abilities. The " *primitiæ* ſanctified the whole harveſt; and you have at once by this incomparable " piece taken off the reproach, which lay upon piety, and the enquiries into nature, " that the one was too early for young perſons, and the other the ready way to athe-" iſm; than which as nothing has been more impiouſly ſpoken, ſo nor has any thing " been more confuted."

DR. *John Pell* was another of Mr. *Boyle*'s friends. He was deſcended of an ancient family in *Lincolnſhire*, and born at *Southwyke* in *Suſſex*, of which his father was miniſter, on the 1ſt of March, 16$\frac{10}{11}$. At thirteen years of age he was ſent to *Trinity* college in *Cambridge*, being at that time as good a ſcholar as moſt maſters of art in the univerſity, which degree he took in 1630, and ſoon became eminent for his ſkill in the mathematics, and in the Latin, Greek, Hebrew, Arabic, Italian, French, Spaniſh, and High and Low Dutch languages. He was ſeveral years profeſſor of mathematics at *Amſterdam*, and afterwards at *Breda*, and in 1654 was appointed envoy from the protector *Oliver* to the proteſtant cantons in *Swiſſerland*, where he continued till 1658. After the reſtoration he entered into holy orders, and became doctor of divinity, and chaplain to Dr. *Gilbert Sheldon*, archbiſhop of *Canterbury*, though he never enjoyed preferment equal to his merit, and died in mean circumſtances in the pariſh of St. *Giles*'s *in the Fields*, on the 12th of December, 1685, and was interred in that church[z]. Among other pieces publiſhed by him, he tranſlated from the Low Dutch *A Tract touching the ſkill of a better way of Anatomy of Man's Body, by* Yonker de Bills, *Lord of Koppenſdam*, printed at *London*, in 1659, in 12mo; to which Mr. *Boyle* wrote a preface, prefixed to that edition, which was procured by himſelf. Mr. *Hartlib*, in a letter to Mr. *Boyle* from *London*, dated November the 1ſt that year[a], mentions his intention to ſend a copy of this tranſlation and preface to monſieur *Bills* himſelf, *to let him know, what honour hath been done unto him by a young nobleman in theſe parts, and many more to friends in the Low Countries. Really*, continues he, *you have done an excellent work for ſpreading this anatomical magnale upon the honeſt learned world.* He ſent likewiſe the following extract of a letter, which he had juſt received from abroad, with relation to the author himſelf. " Monſieur *Bills* lives at preſent at *Rotterdam* " in the *Engliſh Court* (ſo is the name of the houſe, where he lives, in *Rotterdam.*) " He is a Frenchman, ſpeaks French, Low Dutch, and Latin. He hath four bodies, " two men and two women, ſo anatomiſed, that he can ſhew the inward ſtate of a " man's body; for all the parts of the body are in the body, except the guts and " brains, which lie by. He aſketh above 100,000 Dutch guilders, and then he will " teach the anatomy out of them to ſcholars, ſurgeons, &c. He ſaith this coſt him " not only infinite pains and head-breakings, but that of his own patrimony he " ſpent to this work 60,000 guilders. He affirms, that *Paris* hath preſented him " about 20,000 francs, and *Amſterdam* about 3000 francs for two *corpora*; but he " will not ſell them apart, but in that manner as I mentioned. It ſeems it is very " rare, that he anatomiſed in ſuch a manner thoſe bodies, and prepared alſo, that " they can be clearly ſeen, every thing in his proper place. The like is never yet " done in this world."

DR. *John Wallis* was likewiſe a very intimate friend and correſpondent with our great philoſopher. This excellent divine and mathematician was ſon of a clergyman, and born at *Aſhford* in *Kent* on the 23d of November 1616, and about Chriſtmas 1632 ſent to *Emanuel* college in *Cambridge*, where he took the degree of maſter of arts in 1640, and about the ſame time was choſen fellow of *Queen*'s college in that univerſity, and received holy orders from Dr. *Walter Curle* biſhop of *Wincheſter*. In 1644 he was appointed one of the ſcribes or ſecretaries to the aſſembly of divines at *Weſtminſter*, being at that time miniſter of St. *Gabriel Fen-church* in *London*, whence he removed to the church of St. *Martin Ironmonger Lane*; and in 1649 was made Savilian profeſſor of geometry in the univerſity of *Oxford*; and on the 31ſt of May 1654 took the degree of doctor of divinity. February the 17th, 1657-8, he was choſen *Cuſtos Archivorum* of that univerſity; in which place, as well as in his profeſſorſhip of geometry, he was confirmed at the reſtoration, when he became chaplain to his Majeſty[b]. He died October the 8th, 1703, being eighty-ſeven years, three months, and five days old, and was interred in the choir of St. *Mary*'s church in *Oxford*, where a monument is erected to his memory, though the moſt valuable one is the collection of his own writings, publiſhed together in Latin at *Oxford* in 1699, in three volumes folio. His early regard for Mr. *Boyle* is evident from his dedication to him of his book *de Cycloide & Corporibus inde genitis*, with this inſcription; *Honorabili doctiſſimoque Viro D. Roberto Boyle armigero, tum illuſtri familiâ, tum magnis virtutibus nobili*; and in his dedication he takes notice of Mr. *Boyle*'s eminent ſkill in divinity, in the ſacred and ancient, as well as modern languages, and in political and publick affairs both at home and abroad; and his inceſſant and ſucceſsful cultivation of true philoſophy by experiments of all kinds, *&c.* *Qui præter tuam in theologicis tum cognitionem tum praxin, atque in linguis tum ſacris antiquiſque, tum modernis etiam peritiam, in politicis item & negotiis publicis, & rerum & perſonarum, domi peregréque, intimam notitiam,*

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[z] General Dictionary, Vol. VIII. p. 250—253.

[a] See Mr. *Boyle*'s works, Vol. VI. p. 130.

Qui mores hominum multorum noris & urbes;

in phyſicis etiam & veræ philoſophiæ venatione ita perpetuis & ſubtiliſſimis experimentis, medicis, chemicis, anatomicis aliiſque omne genus, naturam quaſi ferro & igne proſequaris, (nedum per omnes ſubtiliorum in quacunque arte opificum officinis) in ſecretos abditiſſimoſque ſequeris receſſus, & quaſi in viſcera penetras, ut mirum ni tibi tandem ſe in prædam dedat: quique naturam, tanquam equuleo impoſitam, ſeverâ ſaltem, ne crudeli dicam, quæſtione, torquendo vexas & retorquendo, quo verum tandem faſſa ſecreta pandat omnia: idem, in matheſi, æquationes analyticas (equuleum mathematicum) verſas, quo vix ſubtilius aliud inſtrumentum abdita perquirendi aut extricandi involuta.

IN the ſame year 1659, Mr. *Boyle* being acquainted with the circumſtances of the learned Dr. *Robert Sanderſon*, afterwards biſhop of *Lincoln*, who had loſt all his preferments on account of his attachment to the royal party, he conferred upon him an honorary ſtipend of fifty pounds a year. This ſtipend was given as an encouragement to that excellent maſter of reaſoning, to apply himſelf to the writing of *Caſes of Conſcience*, 1. "Explaining the nature of conſcience, the obligation of it, how far that extends, and the reaſons of it, with ſome univerſal principles and general rules concerning it, *in theſi*; and then, 2. *in hypotheſi*, to fall to the reſolving particular caſes[c]." Dr. *Sanderſon* therefore publiſhed his treatiſe, intitled, *De Obligatione Conſcientiæ Prælectiones decem, Oxonii in Scholâ Theologicâ habitæ Anno Domini* 1647, and addreſſed it to Mr. *Robert Boyle* in an elaborate dedication, dated at *Boothby Pannel* in *Lincolnſhire*, November 22, 1659; wherein he ſpeaks of his patron

[b] General Dictionary, Vol. X. p. 89—101.

[c] Dr. *Barlow*'s letter to Mr. *Boyle*, Sept. 13, 1659. Vol VI. p 301. See likewiſe *Iſaac Walton*'s life of biſhop *Sanderſon*, and biſhop *Barlow*'s letter to *Walton*, dated at *London* May 10, 1678, and ſubjoined to *Walton*'s *life of biſhop* Sanderſon.

as much more diſtinguiſhed by his excellent diſpoſitions, love of learning, humanity, piety, and all kinds of virtue, than by his birth and quality: *Cum natalium ſplendore illuſtrem, tum generoſæ mentis indole, amore literarum, humanitate, pietate, & omni virtutum genere multo etiam illuſtriorem.*

UPON the reſtoration in 1660, Mr. *Boyle* was treated with great civility and reſpect by the King as well as by the earl of *Southampton*, lord high treaſurer, and the earl of *Clarendon*, lord chancellor of *England*. And he was ſolicited by the latter to enter into holy orders, not only out of regard to him and his family, but chiefly with a view to the ſervice of the church itſelf, which could not but receive a great ſupport, as well as a powerful example, from one, who would ſoon be raiſed to one of the higheſt ſtations in it. This, as he owned to biſhop *Burnet*, made ſome impreſſion upon him. His mind was even then at three and thirty ſo entirely diſengaged from all the projects and concerns of this world, that as the proſpect of dignity in the church could not move him much, ſo the probability of his doing good in it was much the ſtronger motive. But two things determined him againſt it. One was, that his having no other intereſts with relation to religion, beſides thoſe of ſaving his own ſoul, gave him, as he thought, a more unſuſpected authority in writing or acting on that ſide. He knew, that the irreligious fortified themſelves againſt all that was ſaid by the clergy, with this, that *it was their trade*, and that *they were paid for it*. He hoped therefore, that he might have the more influence, the leſs he ſhared in the patrimony of the church. But his main reaſon was, that he had ſo high a ſenſe of the obligations, importance, and difficulty of the paſtoral care, that he durſt not undertake it; "eſpecially, ſays biſhop *Burnet*[d], not having felt within himſelf *an inward* " *motion to it by the holy Ghoſt*; and the firſt queſtion, that is put to thoſe who come " to be initiated into the ſervice of the church, relating to that *motion*, he, who had " not felt it, thought he durſt not make the ſtep, leſt otherwiſe he ſhould have lied " to the holy Ghoſt. So ſolemnly and ſeriouſly did he judge of ſacred matters." He choſe therefore to purſue his philoſophical ſtudies in ſuch a manner, as might be moſt effectual for the ſupport of religion; and began to communicate to the world the fruits of thoſe ſtudies. The firſt of theſe was printed in 1660 at *Oxford*, in 8vo, under the title of *New Experiments phyſico-mechanical touching the Spring of the Air and its Effects, made for the moſt part in a new pneumatical Engine: written by way of letter to the right honourable* Charles *lord viſcount* Dungarvan, *eldeſt ſon to the earl of* Corke: dated at *Beconsfield* December the 20th, 1659. In this diſcourſe he diſcovered and demonſtrated the elaſtic power or ſpring of the air, and by this means exploded the notion of a *Fuga Vacui*, and ſhewed, that the ſtrange effects, which were before aſcriped to that imaginary cauſe, ariſe merely from the native ſelf-expanſion of the air. The extent of which elaſtic expanſion he found divers ways to meaſure by his engine, which likewiſe diſcovers the influence which the air hath upon flame, ſmoke, and fire; that it hath none in magnetical operations; that it is probably greatly interſperſed in the pores of water, and compreſſed by the incumbent atmoſphere even in theſe cloſe receſſes: what operation the exſuction of the air hath on other liquors, as oil, wine, ſpirit of vinegar, milk, eggs, ſpirit of urine, ſolution of tartar, and ſpirit of wine: the gravity and expanſion of the air under water: the effect of the air in the vibrations of pendulums, and in the propagation of ſounds: that fumes and vapours aſcend by reaſon of the gravity of the ambient air, and not from their own poſitive levity: the nature of ſuction, the cauſe of filtration, and the riſing of the water in ſiphons: the nature of reſpiration, illuſtrated by trials made on ſeveral kinds of animals, and the effects of the air in the operations of corroſive liquors. However this work, which was likewiſe tranſlated into Latin, was attacked by *Franciſcus Linus* and Mr. *Thomas Hobbes* of *Malmeſbury*; which occaſioned Mr. *Boyle* to ſubjoin to a ſecond edition of it, printed at *London* 1662, in 4to. *A Defence of the Doctrine touching the Spring of the Air againſt the Objections of* Linus, *wherewith the Objector's* Funicular Hypotheſis *is examined*; *and an Examen of Mr.* Tho. Hobbes's Dialogus Phyſicus de Naturâ Aeris; *with an Appendix (Chap.* VII.) *touching Mr.* Hobbes's *Doctrine of Fluidity and Firmneſs.* The third edition of the *New Experiments Phyſico-mechanical* was printed at *London* in 1682, in 4to.

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[d] Funeral Sermon on Mr. *Boyle*, p. 29. edit. in 4to. and memorandums dictated by Mr. *Boyle* to biſhop *Burnet* concerning his own life.

He produced likewiſe to the world in 1660, a piece on another ſubject, which I have obſerved above to have been finiſhed as early as the year 1648, and was now printed at *London* in 8vo, and entitled, *Some Motives and Incentives to the Love of God pathetically diſcourſed of in a Letter to a Friend*, or, as it is called in the running title, *Seraphic Love.* In the *Advertiſements* prefixed to it we are informed, that this letter was not written ſingle, being but the laſt of divers, wherein love in general was confeſſed, juſtified, and celebrated; the received way of making love was explicated, defended, and oppoſed; conſtancy and inconſtancy in love were argued for and againſt; Platonic love was explicated and celebrated; the cure of love was profeſſed and proſecuted; and the controverted points concerning love were diſcourſed of in a way ſuited to the ſeveral themes, and to the humours and principles of the ſuppoſed writers. But all the former papers, drawn up in a compliment to a lady, though very free from the guilt of either licentiouſneſs or profaneneſs, were judged by Mr. *Boyle* too little ſerious, either to appear alone, or to accompany the diſcourſe publiſhed by him, wherein he had expreſſed his own opinion, as in the former he but deduced thoſe of imaginary perſons. The 9th edition of this book was publiſhed at *London* in 1708, in 8vo. It was alſo tranſlated into Latin.

Among the letters to him written in the year 1660, there is one dated Oct. 2. that year from Mr. *Robert Codrington*, a gentleman known by his Engliſh verſions of *Quintus Curtius*, and *Juſtin*, the Roman hiſtorian, and his other tranſlations from the Latin and other languages. In this letter he mentions his having dedicated a book to Mr. *Boyle* about ten years before, for which he had received a gratuity; and now addreſſes a Latin poem, which he intended to publiſh among the reſt of his poetical works [e].

On the 10th of that month likewiſe Mr. *Robert Southwell*, afterwards a knight, and envoy from King *Charles* II. to the King of *Portugal*, and preſident of the Royal Society, wrote to him from *Florence*, to inform him, that the Grand Duke of *Tuſcany* was extremely deſirous of a correſpondence with him, that Prince being not only a patron of learning, but alſo a great maſter of it himſelf [f].

The year following Mr. *Boyle* publiſhed at *London* in 4to, *Certain phyſiological Eſſays, and other Tracts*, viz. I. *Some Conſiderations touching experimental Eſſays in general.* II. *Two Eſſays concerning the Unſucceſsfulneſs of Experiments, containing divers Admonitions and Obſervations (chiefly chemical) touching that Subject.* III. *Some Specimens of an attempt to make chemical Experiments uſeful to illuſtrate the Notions of the corpuſcular Philoſophy.* IV. *A phyſico-chemical Eſſay containing an Experiment, with ſome Conſiderations touching the differing Parts and Redintegration of Salt-Petre.* V. *The*

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[e] Vol. VI p. 636, 637.

[f] Ibid. p. 296, 297.

Hiſtory of Fluidity and Firmneſs. In the *Advertiſement to the Reader* it is obſerved, that theſe tracts were not written near about the ſame time, (ſome of them being divers years older than others) nor were they now publiſhed in the ſame order in which they were written. For the firſt of them, though drawn up about four years before, *viz.* in the year 1657, was not only written after the ſecond, third, and fourth, but after divers other Eſſays, which Mr. *Boyle* had then lying by him among his papers, it being intended for a kind of introduction to all thoſe treatiſes, which under ſeveral names, but chiefly that of *phyſiological Eſſays*, he had then compoſed. But having, during the confuſions before the reſtoration, ſo diſpoſed of his papers to ſecure them, that he could not himſelf ſeaſonably recover them; and being engaged by promiſe to ſome friends to let about half a dozen of his ſmall tracts come abroad into the world by ſuch a time, he was obliged to ſend them to the preſs, as they came, ſome at one time, ſome at another, to his hands. The greateſt part of them are addreſſed to his nephew Mr. *Richard Jones*, only ſon to the lord viſcount *Ranelagh* by Mr. *Boyle*'s ſiſter. In theſe *Eſſays* and *Tracts* he encourages and perſuades the making *Experiments*, and collecting *Obſervations*, and gives the neceſſary cautions which are to be uſed in ſuch deſigns. He imparts a very conſiderable luciferous experiment concerning the different parts and redintegration of *Salt-petre*; from which he ſhews, that *motion*, *figure*, and *diſpoſition* of parts are ſufficient to produce all the *ſecundary Affections* of bodies, and conſequently that there is no need of the ſubſtantial forms and qualities of the ſchools. In the *Hiſtory of Fluidity* he ſhews, that a body ſeems to be fluid by conſiſting of corpuſcles touching one another only in ſome parts of their ſurfaces; whence, by reaſon of the numerous ſpaces between them, they eaſily glide along each other, till they meet with ſome reſiſting body, to whoſe internal ſurface they exquiſitely accommodate themſelves; and that the requiſites to fluidity are chiefly theſe; ſmallneſs of the component particles, a determinate figure of them, vacant ſpaces between them, and agitation of them variouſly and apart by their own innate motion, or by ſome thinner ſubſtance, which ſhall toſs them about in its paſſage through them. He ſhews likewiſe, how a conſiſtent body may be rendered fluid; and having illuſtrated the doctrine of fluidity by experiments, obſerves, that the reaſon why ſome fluids will not mix with others, is their particular textures, and the peculiar motion of their minute parts. In the *Hiſtory of Firmneſs* he informs us, that firmneſs conſiſts principally in this, that the particles which compoſe it, being ſomewhat groſs, are alſo at reſt, and have a mutual coheſion, whereby they are rendered unapt to diffuſe themſelves every way; ſo that its three principal cauſes appear to be groſſneſs, quiet contact, and implication of the component parts. He obſerves, that a juxta-poſition of parts is not the only cauſe of coheſion, but that the ſpring and weight of the air is one great cauſe; though a juxta-poſition of the parts of glaſs ſeems requiſite and ſufficient to make ſuch a compact ſubſtance, the parts of matter which compoſe it, being firſt minutely divided by the fire before their union. He further tells us, that both the figures and textures of the parts of a body may not only contribute to their ſolidity, but that the interpoſition of the minute parts of another body may render ſome liquors ſolid, and that the addition of a powder only may make a liquor ſolid: that fluids conſiſt not of parts diviſible into fluids, as quantity into quantity: that there is a plaſtic power inherent in ſeveral bodies; and that mixture itſelf is ſufficient to produce putrefaction. Theſe Eſſays were tranſlated into Latin. The ſecond edition of them was printed at *London* 1669, in 4to, and in it ſome of the Tracts were enlarged by experiments, and an addition made to it of *A Diſcourſe about the abſolute Reſt of bodies.*

THESE *Eſſays*, together with his *New Experiments*, being ſent by Mr. *Hartlib* to Mr. *John Beale*, the latter wrote the following letter concerning them[g].

“ THIS day I received the books, ſo that I cannot ſo ſuddenly give you my judgement
“ on them: only this I can ſay, that if an oracle had offered me my choice of
“ what queſtions I moſt deſired to be informed, I am well aſſured, that thoſe that
“ are handled by Mr. *Boyle*, had been the very firſt matter of my election. He wants
“ a ſpirit, that takes not pneumaticals to be the ſpring of life, and weightieſt of naturals,
“ and now moſt conſiderable, ſince *Jordanus Brunus* and others have inclined
“ our modern philoſophers to approximate air to the chief of the ambient heavens.
“ And for my own part, I have long conceived this ambient air to conſiſt of as many
“ and different ingredients, as our earth or ſeas, but I never expected in this life to
“ ſee the diverſities ſo handled and traced out, and the properties ſo fully diſcovered;
“ yet this I am inforced to confeſs before I have ſeen half of the book. In the other
“ treatiſes I am informed of the cauſe of ſuch frequent miſconduct in the beſt kind
“ of medicines, and the ſtrange Protean force of ſalt-petre, and that nitre, which is
“ ſo highly ſtiled by ſome, as if it were the ſpirit of the world. For the Corpuſcularian
“ philoſophy, I had long ago complained to you, that Sir *Kenelm Digby* and
“ ſome others had ſaid enough of it to make me giddy in their *pro & contra*; and my
“ refuge was lord *Bacon*'s *Circa ultimates rerum fruſtranea eſt inquiſitio.* But now I
“ ſee a ſtay for the light of reaſon and experience. Neither had I any thing more in
“ chace than the cauſe of *firmneſs*, which in lord *Bacon*'s language, (who firſt
“ awakened my attention to it) is frequently called *conſiſtency*. Being now in my
“ devotions before the oracle, you can expect but few lines, and in theſe my advice,
“ that you procure them for yourſelf before they are all gone out of the ſhops; for
“ I ſupplicate, that theſe may rather return to you in caſh than in kind. It is my
“ great joy, that Mr. *B.* is ſo far engaged to give us the reſt of his Notes and following
“ Experiments. In theſe he hath obliged all the intelligent inhabitants of this
“ world, and hath given us hope, that we ſhall ſhortly complete human ſciences.
“ Some families amongſt us have anſwered to all lord *Bacon*'s votes for *Advancement*
“ *of Learning*; and this honourable family deſerves to be reputed the firſt college in
“ this univerſity or œcumenical academy.”

And in another letter he wrote as follows:

“ MY laſt was very abrupt and conciſe, becauſe at that time, as I then told you,
“ I was attentive in my devotions before the noble oracle, which I may now, upon
“ the reviewed ſolemnities of holy oath, ſtile the moſt ſatisfying oracle in my apprehenſions,
“ that ever appeared in the converſe of mortals on this habitable globe, for
“ the diſcovery of all the works of nature. To thoſe that have been ſo tired and
“ wearied as I have been, in the ſeveral ways of *Toleſus*, *Flood*, *Gaſſendus*, the Carteſian,
“ and Atomical or Corpuſcularian philoſophers, and all others that I could
“ hear of ever ſince I was concerned in the victory of ſchool-ſophiſters: to thoſe, I
“ ſay, that have condeſcended to take deep notice of the inſufficiency of language,
“ and conjectures, and ungrounded ratiocinations, and have ſubmitted their patience
“ to the ſeverity of lord *Bacon*'s inquiſitions, and, amongſt theſe, to myſelf, here are

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[g] Mr. *Hartlib* ſent a copy of that letter to Dr. *Worthington* incloſed in one from himſelf, dated Aug. 26, 1661, communicated to me by Mr. profeſſor *Ward*.

“ offered ſuch pleaſing refreſhments, as gain us the reliſh of that *Virgilian* ſimplicity, which was ſo highly admired by *Scaliger* in theſe verſes;

“ *Tale tuum carmen nobis, divine poeta,*
“ *Quale ſopor feſſis in gramine, quale per æſtum*
“ *Dulcis aquæ ſaliente ſitim reſtinguere rivo.*

“ To indulge my own freedom, and a little to practiſe my pen and judgment in the impartial collections of other men's deepeſt or moſt inticing reaſonings, I had in philoſophy and theology reduced *Interlocutores* in the manner of *Tully's Oratory* and his *Academical Queſtions*, to offer their ſeveral ſtrengths, (as you have ſeen a ſpecimen pretending to the vindication of a rural life) for which academical ſcene I had long ago provided thoſe arguments in theology, which have ſwayed no leſs men than *Grotius* and the earl of *Briſtol*, and do make the loftieſt cedars ſtagger and revolt ſeveral ways, at as much uncertainty, as the variation of the compaſs. And from all theſe I could procure a concurrence as to the unchangeable oracle for a holy life. For philoſophical ſatisfactions I did chiefly addreſs to philoſophical experiments, in which I ſeemed to have the beſt overtures of aid from lord *Bacon*; but of this I complained, that in the progreſs of late years we had not brought his experiments, or added our own, to any degree of ripeneſs. And this was indeed my diſcouragement. Now I confeſs I am ſurpriſed with wonder at the preſent advancement; and I dare promiſe our poſterity, that knowledge ſhall in this following age abound in very great perfection, and to the beſt of noble operations. I can now no longer forbear to enquire for the return of Mr. *Oldenburg*, which was promiſed ſome weeks ago. From him I hope to be informed, when Mr. *Boyle* will be pleaſed to oblige the world with the publication of his other works:

“ 1. THAT of *promiſcuous Experiments* mentioned in the *philoſophical Eſſays*, p. 14, 15, 16.

“ 2. THE *Dialogues concerning Heat, Flame, and Fire*, mentioned in theſe *new Experiments*, p. 104, 119, and 222.

“ 3. THE *magnetical Steams of the Earth*, mentioned *ibid.* p. 120.

“ 4. THE *Continuation of Experiments made by the pneumatical Engine.*

“ AND this we may well diſcern, that nothing can drop from his pen, that doth not oblige the future ages univerſally; neither can preſent envy appear againſt ſuch bright luſtre. I pray you be mindful to lay this load upon Mr. *Oldenburg*, for it enables him to oblige us in that ſweet entertainment of hope and expectation, which eaſeth all kinds of griefs. The note of animals in vinegar, *Phil. Eſſays*, p. 40. may ſhew what conſiderable uſe may yet be made of our beſt magnifying glaſſes, if the overtures were well proſecuted.”

THE next work, which Mr. *Boyle* publiſhed in 1661, was his *Sceptical Chemiſt: or Chemico-phyſical Doubts and Paradoxes touching the Experiments, whereby vulgar Spagiriſts are wont to endeavour to evince their Salt, Sulphur, and Mercury, to be the true Principles of Things:* Oxford, in 8vo. To the ſecond edition, printed at *Oxford* 1679[h], in 8vo, are ſubjoined *divers Experiments and Notes about the Producibleneſs of chemical Principles.*

IN this treatiſe he obſerves, that the number of the *Peripatetic Elements* and *chemical Principles* is doubtful; and that the words *Element* and *Principle* are here uſed as equivalent terms, and ſignify thoſe primitive and ſimple bodies, of which the mixed ones are ſaid to be compoſed, and into which they are ultimately reſolved: that the matter of all bodies was originally divided into ſmall particles of different ſhapes and ſizes; and that theſe particles might unite into ſmall parcels not eaſily ſeparable again: that a great variety of compounds may ariſe from a few ingredients: that various ſubſtances are obtainable from bodies by fire: that fire is not the true and genuine analyſer of bodies, ſince it does not ſeparate the principles of a body, but variouſly compounds and alters the texture of a body, which it acts upon: that ſome things obtained from a body by fire were not its proper ingredients: that all compound bodies differ only in ſome mechanical properties: that it does not appear, that three is preciſely and univerſally the number of the diſtinct ſubſtances or elements, into which all mixed bodies are reſolvable by fire: that ſome bodies afford more than three principles: that earth and water are to be reckoned among the chemical principles, and at leaſt one certain alkaline, if not alſo an acid ſpirit. He cenſures the ambiguity of the chemical writers, and ſhews, that the chemical principles are diſſimilar in their nature. He enquires, whether the five chemical principles ſhould be rejected for not being homogeneous; whence the notion of theſe principles aroſe; whether fire be neceſſary in the compoſition of bodies; and whether there be any elements at all. He ſhews, that bodies are not compoſed from all the mere elements, by the growth of bodies, vegetables, animals, minerals and metals, and the analyſis of bodies: that the *tria prima* ſeem to be the productions of fire: that neither phlegm nor earth are univerſal and pre-exiſtent ingredients of mixed bodies: that gold itſelf is deſtructible, and new qualities producible in bodies by a mere change of texture: that chemical principles are tranſmutable, as appears from the phænomena afforded by an eſſential oil in repeated diſtillations: that thoſe principles are not ingenerable and incorruptible: that ſalts may be produced and deſtroyed; and that alkalies are tranſmutable into other ſubſtances, and common ſalt, different kinds of ſpirits, ſulphurs, mercuries, phlegm, and earth are producible. He informs us, that the chemical theory of qualities is narrow, defective, and uncertain, ſuppoſing things not proved, often ſuperfluous, and frequently contradicting the phænomena of nature. That the principles found in bodies are not the cauſe of their qualities, but that contrary qualities are aſcribed to the ſame body: that, upon the whole, the chemiſt's ſalt, ſulphur, and mercury, themſelves are not the firſt and moſt ſimple principles of bodies, but rather primary concretions of corpuſcles or particles more ſimple than they, as being endued only with the firſt moſt radical and moſt univerſal properties of ſimple bodies, bulk, ſhape, motion, or reſt, by the different conventions or coalitions, of which minuteſt portions of matter are made thoſe differing concretions, which chymiſts call their principles. He then makes ſome reflections upon the hypotheſis of alkali and acid; and ſhews, that the ſuppoſition of them is precarious, their offices arbitrary, and the notion of them unſettled; that the taſte is no judge of them, and the hypotheſis of them often needleſs and inſufficient, affording but unſatisfactory ſolutions of phænomena. In a marginal note on the *Preface introductory* in the ſecond edition, he mentions ſome other *Dialogues* concerning *heat*, *fire*, *flame*, &c. which had been ſeen by two ſecretaries of the Royal Society, but were miſſing, with ſome other pieces of his, after the haſty removal of his goods by night in the great fire of *London*. This work was tranſlated into Latin.

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[h] The title-page erroneouſly ſays 1680. See the *Advertiſement*.

IN 1662 a grant of the forfeited impropriations in the kingdom of *Ireland* was obtained from the king in Mr. *Boyle*'s name, though without his knowledge; for the application of which to the ſupport of religion and promotion of learning he was greatly ſolicitous, as appears from a letter of his to Dr. *Michael Boyle*, biſhop of *Corke*, dated May the 27th that year[i]; in which he mentions, that he expected from an honeſt gentleman in public employments, and very well verſed in the affairs of that country, an account, " how thoſe impropriations, which are, *ſays he*, within " the grant made me by his Majeſty, are, or are like to be, provided of miniſters " and maintenance. And though the procuring me this account would, I know, " require ſome time; yet the requiſiteneſs of it to aſſiſt me, how to order myſelf on " this unhappy occaſion, made me unwilling to write to your lordſhip, till I had received it; and the adjournment of the parliament made me hope I might receive " it ſeaſonably enough. But being ſurpriſed ſince the laſt poſt with the news of the " parliament's being met again, and of the quick progreſs that is making in the " act for the ſettlement of *Ireland*, the promiſe I made your lordſhip at our " ſeparation forbids me to delay to ſay ſomething to you about the buſineſs we " then diſcourſed of; but it is now too late at night, that I muſt not till a fitter opportunity enlarge upon particulars, but muſt make haſte to inform your lordſhip " that having, as I told you I intended, applied myſelf to that excellent prelate the " biſhop of *Lincoln*[k], for his advice on ſo nice an occaſion, I came upon the whole " matter to this reſult, that the king's grant made to me having preceded both the " grant made in the act to the church, and even the church's application to his " Majeſty for that grant, I might juſtly challenge the impropriations mentioned in " my proviſo. . . . . . And therefore I would not have what I intend to do, reflect " upon thoſe perſons of honour, that (though unknown to me) made uſe of my name " to obtain the grant from the king; ſince in ſuch diſputable caſes, perſons that act " very differently from one another, may yet all ſatisfy their own judgments and " conſciences in their proceedings. But in regard my intention in general was to " apply an addition of my revenue, if my friends procured any for me, to good " uſes, though, I confeſs, I deſigned it rather for the advancement of real learning, " than to any other purpoſe; yet ſince it ſo falls out, that unknown to me it is caſt " upon impropriations, it is very likely, that by the account I expect of the ſtate of " them, I may ſee cauſe to make the more immediate ſervice of religion (by relieving the poor in thoſe places, and contributing, if need be, towards the maintenance of miniſters there or elſewhere, or promoting other good works, as from " time to time occaſion may require) the principal of theſe good uſes, to which I " have thoughts to apply about two-thirds of what will, *de claro*, come to me out of " my ſhare of this grant, hoping alſo, that the other perſons of honour, that are " concerned in it, will have a care, that the impropriations, whoſe revenues are " given them, be provided with miniſters. And I ſhould poſſibly employ the other " third part alſo the ſame way, but that his majeſty has been pleaſed, without my " ſeeking, or ſo much as knowledge, to appoint me governor to a corporation for " the propagating of the goſpel among the heathen natives in *New England* and " other parts of *America*. And this corporation being at great charges for neceſſary " works, and eſpecially for the tranſlating and printing of the Bible in the Indian " tongue, above one-half of their revenue is injuriouſly detained from them by a " perſon, who had ſold it to a corporation erected for the ſame purpoſes with ours " under the late uſurping powers, and now has repoſſeſſed himſelf of his land, becauſe thoſe that he ſold it to, were not legally qualified to have a ſale made to " them. By which means ſo pious a deſign, as is purſued by this corporation, is now in danger to miſcarry for want of maintenance; ſo that the work being ſo charitable, and I having a peculiar call to promote it, I think, after having adviſed with the biſhop of *Lincoln* in the caſe, that it becomes me, on ſuch a juncture of circumſtances, to apply the other third part, or thereabouts, of what the king's grant will yield me for ſix or ſeven years at leaſt, to the carrying on of ſo unqueſtionably good a work; ſo that the main benefit I intend to derive from the king's bounty is the opportunity of doing ſome good with what, if my friends had not obtained it, might have been begged by others, who would have otherwiſe employed it. And by this, I hope, your lordſhip will be confirmed in the belief of what I formerly aſſured you of, namely, that had I known, that any thing was aſked for me, whoſe grant would have been prejudicial or unwelcome to the church, I ſhould not have conſented to have my name made uſe of for a much greater matter, than the proviſo is like to yield me. And the ſame day your lordſhip did me the honour to inform me of the ſtate of the caſe, I offered ſome of the parties concerned to relinquiſh the grant, though it had already paſſed the king and council, ſo I nor my name might have nothing to do with it; but one of the intereſted parties being too far off to be conſulted, the proffer was declined. From all which, I hope, your lordſhip will do me the right to conclude, that I was far from deſigning the prejudice of the church; and that though it cannot in reaſon be expected, that I ſhould do any thing to hinder the other perſons concerned in my proviſo to purſue the having it paſſed, and to enjoy the benefit of it; yet I do not at all deſire, that either your lordſhip, or any other perſon, that thinks the proviſo may be juſtly excepted againſt upon the church's behalf, ſhould, for my ſake, at all abſtain from acting and ſpeaking, as his judgment and conſcience ſhall direct him. For as my partners are (and I think not cauſeleſsly) confident of being able to carry on in *Ireland* that, which has already paſſed the ſcrutiny here; ſo, for my part, all I pretend to by this grant is, to have the power in my own hands, to ſee, that the proceed of it be carefully employed to the good uſes whereto I deſign it." The biſhop of *Corke* in his anſwer to this letter of Mr. *Boyle*, dated at *Dublin* Aug. 13, 1662, obſerves[1], that with regard to the buſineſs abovementioned, he was perfectly ſatisfied, that Mr. *Boyle*'s deſign, "(if, *ſays he*, you had any, for I cannot yet imagine you to be any otherwiſe than paſſive in the whole matter) in acquiring thoſe impropriations was charitable and religious; for you cannot do any thing that is otherwiſe. I muſt likewiſe very much applaud your pious intendments for the advancement of real learning, and eſpecially for the extenſiveneſs of your charity towards the poor heathen natives of *New England*. But yet I muſt humbly take leave to acknowledge myſelf unſatisfied, why that additional revenue, which his majeſty deſigned and promiſed for the better ſupport of the clergy here, that they may with greater comfort attend the cures of their ſeveral churches, ſhould be diverted to any other uſe, though in itſelf it be generous and handſome, eſpecially when I conſider, how many congregations depend upon the ſervice of thoſe pariſhes; how deſtitute they will be of a church to reſort unto; how the poor people will be compelled to wander through the country, to find out an opportune place for the performance of their public duties and devotions unto God. For although at preſent your piety provides all that poſſibly you may, to prevent theſe ſad conſequences; yet you know not how they ſhall be minded, who ſucceed you; nor are you certain, that thoſe other perſons

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[i] Vol. VI. p. 56.

[k] Dr. *Robert Sanderſon*.

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[1] Vol. VI. p. 638.

" (whoever they be) that have obtained this grant under your patronage, will be as " inclinable upon that account as you are. If they be not (as it is great odds on " your ſide againſt moſt men living) it will not then be unworthy your conſideration, " whether ſomething of their fault will not be chargeable upon you, by whoſe only " means they were enabled to do the prejudice. As to that advice and reſolve, " which was given you by that moſt excellent prelate, whoſe piety and learning " makes him deſervedly honoured by all that have either known or read him, I " muſt neceſſarily believe it was founded on a very great miſtake. For in caſe the " difference be otherwiſe than it is ſtated in your letter, if that bleſſed Martyr *Charles* " I. did in his life-time ſolemnly devote all impropriations unto the church, that " ſhould be any way inveſted in him; if his moſt gracious majeſty, that now is, " ſhould, as an early evidence of his gratitude to God for his miraculous reſtoration, " immediately after his acceſs into his kingdom, engage himſelf in the like bounty " to this church of *Ireland*; if his majeſty had not only promiſed, but actually " granted them under his privy ſignet, before any conceſſion of what your grant " contains was made unto you (whereof each particular may, as I ſuppoſe, be made " ſomewhat conſpicuous) then certainly you will think it worthy ſome further conſi- " deration, whether you will inſiſt upon that grant or no."

MR. *Boyle* in his letter of May 27, 1662, above cited, mentions his having been appointed by the king governor of the *Corporation for propagating the Goſpel in* New England, *and the parts adjacent in* America, and that the revenue of this corporation was injuriouſly detained from them. The caſe was this: in July 1649, there was an ordinance paſſed in the parliament, *for the promoting and propagating of the Goſpel of Jeſus Chriſt in* New England, by the erecting a corporation in perpetual ſucceſſion, to be called by the name of *the preſident and ſociety for the propagation of the Goſpel in* New England, " to receive and diſpoſe of monies in ſuch manner, as ſhall beſt and " principally conduce to the preaching and propagating the goſpel amongſt the " natives, and for the maintenance of ſchools and nurſeries of learning for the edu- " cation of the children of the natives; for which purpoſe a general collection was " appointed to be made in and through all the counties, cities, towns, and pariſhes " of *England* and *Wales*, for a charitable contribution, to be as the foundation of ſo " pious and great an undertaking [m]." With the monies thus collected lands were purchaſed to the value of between five and ſix hundred pounds a year [n], and ſettled in a corporation of citizens of *London* in truſt, *Henry Aſhhurſt*, Eſq; being their treaſurer. Upon the reſtoration of king *Charles* II. the corporation being dead in law, colonel *Bedingfield*, a papiſt, who had ſold an eſtate of 322 *l. per ann.* [o], which had been ſettled for the uſes of it, repoſſeſſed himſelf of it, and at the ſame time refuſed to pay back the money which he had received for it. Mr. *Boyle* therefore uſed his intereſt with the lord chancellor *Clarendon*, to prevent that act of injuſtice; and the corporation being revived in 1661, by an expreſs charter [p], he was made governor of it, and the eſtate, which had been detained by *Bedingfield*, was reſtored to the corporation by the chancellor's decree [q].

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[m] Account of the Society for propagating the goſpel in foreign parts, eſtabliſhed by the royal charter of King *William* III. p. 4. edit. *London*, 1706, in 4to.

[n] Mr. *Daniel Neale*'s hiſtory of *New England*, Vol. I. Chap. VI. p. 263. *who obſerves, that* Mr. Baxter *in his* Life, fol. 290. *is miſtaken in computing that purchaſe at ſeven or eight hundred pounds* per ann.

[o] *Neale, ubi ſupra.*

[p] The copy of the charter is extant among Mr. *Boyle*'s papers, and not being, as far as I can learn, hitherto printed, is inſerted in the *Appendix* to this Life.

In 1663 the Royal Society being incorporated by King *Charles* II. by letters patents, dated the 22d of April, Mr. *Boyle* was appointed by the charter one of the council of the learned body [r]; and as he had been one of the principal perſons, to whom that ſociety owed its firſt riſe and progreſs, he continued during the reſt of his life one of its moſt uſeful members.

In June the ſame year he publiſhed at *Oxford*, in 4to, *Some Conſiderations touching the Uſefulneſs of experimental natural Philoſophy, propoſed in a familiar Diſcourſe to a Friend by way of Invitation to the ſtudy of it.* The *firſt Part* contains five Eſſays: I. Of the Uſefulneſs of experimental Philoſophy, principally as it relates to the mind of man. II. Of the ſame. III. A continuation of the ſame. IV. A requiſite Digreſſion concerning thoſe that would exclude the Deity from intermeddling with Matter. V. Wherein the Diſcourſe interrupted by the Digreſſion is reſumed and concluded. The *ſecond Part* conſiſts of two ſections; *viz.* Section I. of the Uſefulneſs to Phyſic, containing five Eſſays, *viz.* 1. Some particulars tending to ſhew its Uſefulneſs to the phyſiological Part: 2. To the pathological part of Phyſic: 3. To the ſemeiotical part of Phyſic: 4. To the hygieinal part of Phyſic: 5. To the therapeutical part of Phyſic. This book was reprinted at *Oxford*, in 4to, the year following. In the advertiſement to the reader Mr. *Boyle* obſerves, that divers parts of this work were ſent to the preſs in 1660 and 1661, as well as in 1663, and that the very laſt Eſſay of it was written ſeveral years before; ſince which time thoſe papers were left, ſometimes in the hands of friends, and ſometimes in diſtant places, where he could not come at them; a circumſtance which he mentions, that the reader might neither wonder nor blame him, if he ſhould meet with ſome things in them, that had already been publiſhed by others, or were more vulgarly known, than his mentioning them implied; and that particularly a great portion of the *firſt Part* was written about ten or twelve years before, when he was ſcarce above twenty-one or twenty-two years of age.

His next book was *Experiments and Conſiderations touching Colours: firſt occaſionally written, among ſome other Eſſays, to a Friend, and now ſuffered to come abroad as the beginning of an experimental Hiſtory of Colours.* To this were added, *A ſhort Account of ſome Obſervations made by Mr.* Boyle *about a Diamond, that ſhines in the dark; firſt incloſed in a Letter written to a Friend, and now, together with it, annexed to the foregoing Treatiſe, upon the ſcore of the affinity between* Light *and* Colours: *and Obſervations made October* 27, 1663, *about Mr.* Clayton's *Diamond, and read before the Royal Society the day following.* This book was printed at *London* 1663, in 8vo, and reprinted there in 1670. It was likewiſe tranſlated into Latin. In this diſcourſe concerning *Colours*, he obſerves, that they may be conſidered either as a quality reſiding in the body, that is ſaid to be coloured, or to modify the light after ſuch or ſuch a manner; or elſe as the light itſelf, which ſo modified ſtrikes upon the organ of ſight, and ſo cauſes that ſenſation which we call *Colour*; but that this latter is the more proper acceptation of the word *Colour*: that it is not without reaſon, that he aſcribes colour chiefly to the ſuperficial parts of bodies: and that a blind man at *Maeſtricht* in the *Low Countries* could at certain times diſtinguiſh colours by the touch of his fingers.

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[q] Biſhop *Burnet*'s memorandums concerning Mr. *Boyle*'s Life, and Mr. *R. Baxter*'s Funeral Sermon for *Henry Aſhhurſt*, Eſq; p. 46.

[r] *Sprat*'s hiſtory of the Royal Society.

As to particular colours, he tells us, that *whiteneſs* depends upon the roughneſs of the ſurface of the body called white, which gives it innumerable ſmall ſuperficies, which acting like ſo many *ſpecula* in various poſitions, reflect the rays of light, that fall on them, not towards one another, but externally towards the ſpectator: that blackneſs reſults from a peculiar kind of texture of the ſuperficial particles of a body, by which it damps the light, which falls on it, ſo that very little is reflected to the eye. He gives us likewiſe ſeveral experiments, which ſhew, that both black and white may be mechanically produced and deſtroyed.

HE gave the public alſo the ſame year *Some Conſiderations touching the ſtyle of the Holy Scriptures, extracted from ſeveral Parts of a Diſcourſe concerning divers particulars belonging to the Bible, written divers years ſince to a Friend:* London, in 8vo, and reprinted there in 1675 in the ſame form. A Latin tranſlation of it was printed at *Oxford* in 1665. The *Diſcourſe*, from which theſe *Conſiderations* were extracted, was the *Eſſay on Scripture*, of which I have given ſome account above; and it was publiſhed by Mr. (afterwards Sir) *Peter Pett*, attorney-general for *Ireland*, to whom the letter of Mr. *Boyle* prefixed to it is addreſſed, and who was meant by the letters *P. P. A. G. F. J.*[*]. It was upon occaſion of theſe *Conſiderations*, that Mr. *Boyle* received from an anonymous hand the following letter:

October 26, 1665.

“ Heavenly *Aretaphilus*,

“ A LITTLE book, under the title of *Conſiderations on the Style of the Holy Scriptures*, coming to my hands, the value your writings merit from all men, engaged me in its peruſal; the ſympathy of which contents with my genius makes me challenge ſo near a relation to the author, as to ſtyle myſelf his admirer. I have obſerved not only the inſignificant tautology, the unpleaſant circumſtances, the inconſiderableneſs of the deſign, and the emptineſs of the end of moſt books, which crowd daily to the preſs, wreſting and cenſuring the ſcriptures; but alſo the danger of ſeveral tenets, which the authors would make ſeem agreeable to the ſcriptures, merely to proſelyte men to their opinion, who are apt to be in love with their rhetoric, and are thereby ſeduced to their own damnation, that I almoſt fear to read any book with the title of holy. I preſume, if men did conſider the weight of that text, *Whoſo adds, or diminiſheth*, &c. there would not appear ſo many comments and large volumes in public, to confound and miſlead the multitude, who are apt enough to ſchiſmatize, where they have a patron, *viz. Paul*, or *Apollos*, or *Cephas*, &c. and to turn to the crooked way of deſtruction, when that little book, which affords matter for thoſe great volumes, and points to the ſtreight path of ſalvation, is neglected and ſlighted. But *nitimur in vetitum*, we are too prone to forbidden cuſtoms; yet I hope this *Enchiridion* may prove a remedy or antidote to ſome againſt the public infection. In the mean time I rejoice, that there is one who has that eſteem for ſo worthy and profitable a book as the Bible, without conſorting with men's pretended helps and additions, concluding with St. *Paul*, that it is *alone able to make the man of God perfect*. And now pardon this trouble, and accept it, till God grant me an opportunity of a nearer conference,

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[*] See Sir *Peter Pett*'s dedication to lord *Altham* before the Memoirs of the right honourable *Arthur* earl of *Angleſey*, edit. *London* 1693, in 8vo.

"that I may improve my talent of knowledge, and preſs towards the mark of all "true Chriſtians,

"Yours.

"*To the honourable Mr.* Robert Boyle, *at lady*
"Ranelagh'*s in* Pall-Mall, London.

IN 1664, Mr. *Boyle* was elected into the company of the Royal Mines[t]; and in September that year received the following letter from *John Winthrop*, Eſq; governor of *Connecticut* and *New Haven* in *New England*:

"Honourable Sir,

"A BARK of this place meeting with one of the frigates at ſea near the port of "their arrival, I had thereby the favour of your honour's letter, and after-"wards met that gentleman Dr. *Sackvill*, recommended therein, at *Graveſant*, a ſmall "town upon the weſt end of *Long Iſland*, and had there ſome acquaintance with him. "But that being a time and place of much action, and my ſtay no longer than that "the fort and town at *Manhatos* was reduced to his majeſty's obedience, I could not "then have much conference with him. He abideth yet at that place (now called "*New York*) which is above a hundred miles from my preſent habitation; but I "hope there may be better opportunities, and I ſhall be ready to do him any ſer-"vice. After the reducing of that place, and that I ſaw it in the peaceable poſſeſ-"ſion of his majeſty's honourable commiſſioners, I came back towards *Hartford* to "have met the commiſſioners of the colonies there, it being their uſual appointed "time, and that the place for this year; but being taken with a fever at *New Haven* "in the way homeward, I was diſappointed of being with them at that time; but "underſtanding after from thoſe that ſupplied in my abſence there, that there was a "propoſition in your honour's letter to them for their advice, how a great ſtock "might be employed for the furthering that good work among the Indians, although "that queſtion I ſuppoſe hath been anſwered by the commiſſioners then met; yet I "am bold to add this motion, that your honour would pleaſe to cauſe that paper, "which I left with the honourable corporation in *England*, to be reviewed. I ſup-"poſe it doth give an hint of a foundation of an uſeful employ of a large ſtock, "even to thoſe good ends the corporation principally aim at. I am very deeply ob-"liged to your honour for multiplied favours, and in particular this of late received, "of the good advice, and intimations, and informations, in reference to thoſe "matters in your honour's letter. I do endeavour greatly to attend your commands, "and to diſpoſe all people to that duty and obſervance towards thoſe honourable com-"miſſioners ſent by his majeſty, as may teſtify their true loyalty and affection to his "majeſty, from whom they come, and I hope for the good of theſe poor plantations. "I ſhall not give your honour further trouble at preſent, than that you will pleaſe to "read, that I am in ſincerity,

"Honoured Sir,

Hartford in *New England*,
Sept. 25, 1664.

"your moſt faithful humble ſervant,

"J. WINTHROP."

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[t] Mr. *Oldenburgh*'s letter to Mr. *Boyle*, dated at *London* December 10, 1664. Vol. VI. p. 185.

THE year following Mr. *Boyle* gave the public his *Occaſional Reflections upon ſeveral Subjects: whereto is premiſed a Diſcourſe about ſuch kind of Thoughts:* London, 1665, in 8vo. reprinted in 1669, in 8vo. and tranſlated into Latin, but never publiſhed in that language. This piece was drawn up when he was very young, as appears from the following paſſage in a letter of his to his ſiſter the lady *Ranelagh*; *Nor ſhall I diſobey your threatened order for looking you out a miſlaid ſcribble I drew up* in my infancy *concerning occaſional Reflections.* And this conſideration, added to that of the cuſtom of the age, in which the imagination was more indulged upon important ſubjects, than the ſeverity of a true taſte of writing will admit, may ſerve to apologiſe for this treatiſe, againſt the inſult upon it in Dr. *Swift's pious Meditation on a Broom Staff*, who has certainly not ſhewn in that piece a juſt regard to the intereſts of religion, any more than to the character of Mr. *Boyle*, by allowing himſelf to treat ſuch ſubjects, and ſo excellent a perſon, with the moſt licentious buffoonery. The ſame year he publiſhed ſome ſmall pieces in the *Philoſophical Tranſactions*, viz. *An Account of a very monſtrous Calf*, N° I. p. 10. together with *an Obſervation* imparted to him by Mr. *David Thomas* touching ſome particulars further conſiderable in that monſter, N° II. p. 11. *Obſervations upon a monſtrous Head of a Colt*, N° V. p. 85. *Some anatomical Obſervations of Milk found in Veins inſtead of Blood, and of Graſs found in the Wind-pipes of ſome Animals*, N° VI. p. 100. *Of a place in* England, *where, without petrifying Water, Wood is turned into Stone*, Ibid. p. 101. *A further Account of an Obſervation abovementioned about white Blood*, Ibid. p. 117. But a more important work was that printed at *London* 1665, in 8vo. under the title of *New Experiments and Obſervations touching Cold; or an experimental Hiſtory of Cold begun. To which are added, an Examen of Antiperiſtaſis, and an Examen of Mr.* Hobbes's *Doctrine about Cold. Whereunto is annexed an Account of Freezing* [a] *brought into the Royal Society by the learned Dr.* C. Merret, *a fellow of it. Together with an Appendix containing ſome promiſcuous Experiments and Obſervations relating to the precedent Hiſtory of Cold.* The ſecond edition was printed at *London* 1683, in 4to. In this work he gives an account of the defectiveneſs of common weather glaſſes, the advantages of the new hermetical thermometers, and an enquiry concerning the cauſe of the condenſation of the air, and aſcent of water by cold in the ordinary weather-wiſers. But theſe are only preliminaries; for the main diſcourſe preſents us with an account, what bodies are capable of freezing others, and what of being frozen: the ways to eſtimate the degrees of coldneſs: how to meaſure the intenſeneſs of cold produced by art beyond that employed in ordinary freezing: in what proportion water will be made to ſhrink by ſnow and ſalt: how to meaſure the change produced in water between the greateſt heat of ſummer, the firſt degree of winter cold, and the higheſt of art: how to diſcover the different degrees of coldneſs in different regions: a way of freezing without danger to the veſſel: what may be the effects of cold, as to the preſerving or deſtroying the texture of bodies. whether ſpecific virtues of plants are loſt through congelation, and then thawing: whether electrical and magnetic virtues are altered by cold: the expanſion and contraction of bodies by freezing: how they are cauſed, and how their quantity is to be meaſured: the ſtrength of water freezing, and an enquiry into the cauſe of that prodigious force: the ſphere of activity of cold: how far the froſt deſcends in earth and water: an experiment ſhewing, whether cold can act through an hot medium: a way of accounting the ſolidity of ice, and the ſtrength of the adheſion of its parts: what liquors are its quickeſt diſſolvents; and an experiment of heating a cold liquor with ice. He afterwards confutes the vulgar notion of *Anteperiſtaſis*, and Mr. *Hobbes*'s Doctrine of Cold; and obſerves, that though he will not undertake to prove the nature of cold to be privative, yet he thinks it eaſy to ſhew, that the arguments produced for its being poſitive are not concluſive.

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[a] Made in December and January 1662.

HIS excellent character in all reſpects had gained him ſo high an intereſt with the King, that about Auguſt the ſame year 1665, he was nominated to the provoſtſhip of *Eton* college, then vacant by the death of Dr. *John Meredith*, a poſt of great honour as well as profit. However he declined accepting it, as well becauſe he was probably apprehenſive, that the courſe of his ſtudies, to which he had ſo many years devoted himſelf, might receive ſome interruption from the duties of that poſt, as on account of his inſuperable diſinclination to entering into holy orders, which is thought neceſſary to that office[v]. But whatever other reaſons might concur with this laſt, to determine his refuſal of the provoſtſhip, the fact of his nomination to it is evident from a letter of Mr. *Oldenburg* to him, dated at *London* Auguſt the 29th, 1665[w], in which he ſays: " If he [Dr. *John Beale*] be not miſinformed by one of his majeſty's " chaplains, who now waits at *Saliſbury*[x], and laſt week gave him a viſit, you, Sir, " are nominated provoſt of *Eton*. Which if ſo, our ſaid friend hopes, and ſo do I, " you will not refuſe it, conſidering, that that place hath many opportunities of in" fluence on the univerſities, and is able to oblige many; which ſtrengthens our " hope, that you will accept of it, eſpecially ſince it is, as I underſtand, no burthen " and much accommodation, it being alſo but half a day's journey from thence to " *London*, and not much more to *Oxford*." And this is confirmed by a letter of Dr. *Beale* from *Yeovill*, Sept. 7, 1665[y], which begins thus: *I wiſh this may find you in* Eton *college, and well ſettled there*. On the 8th of that month we find him created doctor of phyſic in that univerſity[z].

IN February following Mr. *Henry Stubbe*, a phyſician at *Stratford* upon *Avon* in *Warwickſhire*, addreſſed a letter to Mr. *Boyle* upon the ſubject of the famous Mr. *Valentine Greatraks*, the Iriſh Stroker. This extraordinary perſon was ſon of *William Greatraks* of *Affane* in the county of *Waterford*, Eſq; by a daughter of Sir *Edward Harris*, Knt. one of the juſtices of the king's bench in *Ireland*, in the reign of king *Charles*[a]. He was born at *Affane* on the 14th of February 1628, and educated at the free-ſchool at *Liſmore*, where he continued till he was thirteen years of age, and was deſigned the year following for the college of *Dublin*; but the rebellion breaking out in that nation, was obliged, with his mother and ſeveral other ſmall children, to fly for refuge into *England*, where they were relieved by the favour of his uncle Mr. *Edward Harris*, after whoſe death, his mother, for his further progreſs in literature, committed him to the care of Mr. *John Daniel Getſeus*, a German, who was miniſter of *Stock Gabriel* in the county of *Devon*, with whom he ſpent ſome years in ſtudying humanity and divinity. After five or ſix years abſence from his native country he returned thither, but found it in a moſt miſerable ſtate; which made him retire to the caſtle of *Caperquin*, *where I ſpent*, ſays he[b], *a year's time in contemplation, and ſaw ſo*

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[v] See Dr. *John Fell*'s Life of Dr. *Richard Alleſtry*, prefixed to the Sermons of the latter, printed at *Oxford* 1684, in folio.

[w] Vol. VI. p. 192.

[x] Where the king then was, on account of the plague's raging at *London*.

[y] Vol. VI. p. 390.

[z] *Wood*, *Faſti Oxon*, Vol. II.

[a] A brief account of Mr. *Valentine Greatraks* and divers of the ſtrange cures by him lately performed, p. 15. edit. *London* 1666, in 4to.

[b] Ibid. p. 17.

much of the madneſs and wickedneſs of the world, that my life became a burthen to me, and my ſoul was as weary of this habitation of clay, as ever the gally-ſlave was of the oar, which brought my life even to the threſhold of death, ſo that my legs had hardly ſtrength to carry my enfeebled body above. In the year 1649 he became lieutenant in the regiment of *Roger* lord *Broghill*, afterwards earl of *Orrery*, then acting in *Munſter* againſt the Iriſh and Papiſts; and in 1656 a great part of the army there being diſbanded, and he among the reſt, he retired to his eſtate at *Affane*, and was ſoon after appointed clerk of the peace for the county of *Corke*, and regiſter for tranſplantation, and juſtice of the peace[c]. About the year 1662, *I had an impulſe*, ſays he[d], *or a ſtrange perſuaſion in my own mind (of which I am not able to give any rational account to another) which did very frequently ſuggeſt to me, that there was beſtowed on me the gift of curing the king's exil; which, for the extraordinarineſs of it, I thought fit to conceal for ſome time; but at length I communicated this to my wife, and told her, that I did verily believe, that God had given me the bleſſing of curing the king's evil; for whether I were in private or public, ſleeping or waking, ſtill I had the ſame impulſe. But her reply to me was, that ſhe conceived this was a ſtrange imagination; but to prove the contrary, a few days after there was one* William Maher *of* Salterbridge *in the pariſh of* Liſmore, *that brought his ſon* William Maher *to my houſe, deſiring my wife to cure him, who was a perſon ready to afford her charity to her neighbours, according to her ſmall ſkill in chirurgery. On which my wife told me, there was one, that had the king's evil very grievouſly in the eyes, cheek, and throat. Whereupon I told her, that ſhe ſhould now ſee, whether this were a bare fancy or imagination, as ſhe thought it, or the dictates of God's ſpirit on my heart; and thereupon I laid my hands on the places affected, and prayed to God for Jeſus ſake to heal him; and then I bid the parent two or three days afterwards to bring the child to me again, which accordingly he did, and then I ſaw the eye was almoſt quite whole, and the node, which was almoſt as big as a pullet's egg, was ſuppurated, and the throat ſtrangely amended, and, to be brief (to God's glory I ſpeak it) within a month diſcharged itſelf quite, and was perfectly healed, and ſo continues, God be praiſed.* His next cure was of *Margaret Mac-ſhane* of *Ballinecly* of the pariſh of *Liſmore*, who had been afflicted with the evil above ſeven years in a much more violent degree; and ſoon after his fame encreaſing, he cured the ſame diſeaſe in many other perſons for three years, not meddling with any other diſtempers, till about the end of thoſe three years, the ague growing epidemical, he found as formerly, that there was beſtowed on him the gift of curing that diſeaſe[e]. “ Within ſome ſmall time after “ this, *continues he*[f], God was pleaſed by the ſame or the like impulſe to diſcover “ unto me, that he had given me the gift of healing; which the morning following “ I told my brother and wife, but neither of them could be prevailed with to believe “ it, though for my own part I had a full aſſurance thereof within me. This impulſe “ I had the ſunday after Eaſter-day, the 2d of April 1665 early in the morning; “ and the Wedneſday enſuing I went to cornet *Dean*'s (about ſome occaſions I had “ with him) to *Liſmore*, where there came into his houſe to me a poor man, that with “ a violent pain in his loins and flank went almoſt double, and had alſo a moſt grievous ulcerous leg very black, wherein were five ulcers; who deſired me for God's “ ſake, that I would lay my hands on him, and do him what good I could. [But by “ the way take notice, that as God gave me the ſeveral gifts from time to time, he “ always ſent patients, that applied themſelves to me, for I never ſought after any “ from the firſt moment to this very inſtant.] Whereupon I put my hand on his

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[c] Ibid. p. 18, 19.
[d] Ibid. p. 22.
[e] Ibid. p. 22—25.
[f] Ibid. p. 25.

"loins and flank, and immediately run the pains out of him, ſo that he was re-"leaſed, and could ſtand upright without the leaſt trouble. Then I put my hand "on his ulcerous leg (which the chirurgeons, after they had ſhewed all their ſkill on "him, told him it was periſhed at the bone, and ſo muſt be cut off, but that he "wanted 3 *l.* to give one of them for his pains, as he informed me) which forth-"with changed colour, and became red, and three of the five ulcers cloſed up, and "the reſt within a few hours afterwards; ſo that he went out well, that could hardly "by the help of his ſtaff crawl in, and within two days afterwards he fell to his "labour, being a maſon by trade, and ſo continued ſeveral months afterwards to "my knowledge, and to this inſtant, for ought I know." The Thurſday following he cured colonel *Phaire* of *Cahirmony* in the county of *Corke* of an ague, and afterwards many other perſons of different diſtempers by ſtroking [g]; ſo that there was a reſort to him from all parts. But being cited into the biſhop's court at *Liſmore*, and not producing a licence for practiſing, he was prohibited from laying his hands on any perſon for the future, but ſtill continued to do ſo till January 1665-6, when he came to *England* at the requeſt of the earl of *Orrery*, in order to cure the lady of the lord viſcount *Conway* of *Ragley* in *Warwickſhire*, who had for many years laboured under a moſt violent head-ach. He ſtaid at *Ragley* three weeks or a month; and though he failed in his endeavours to relieve that lady, he cured vaſt numbers of people in thoſe parts, and at *Worceſter*. He was then by his Majeſty's order ſent for to *Whitehall*, and continued ſome time in *London*, performing many remarkable cures there in the preſence of perſons eminent for their ſkill and integrity [h].

Mr. *Stubbe*, who was witneſs to ſeveral of his cures in *Warwickſhire*, publiſhed therefore at *Oxford* in 4to, a piece, entitled, *The miraculous Conformiſt: or, An Account of ſeveral marvellous cures performed by the ſtroking of the hands of Mr.* Valentine Greataricks; *with a phyſical Diſcourſe thereupon, in a Letter to the honourable* Robert Boyle, *Eſq*; *with a Letter relating to ſome other of his miraculous Cures, atteſted by* E. Foxcroft, *M. A. and Fellow of* King's *college in* Cambridge. Mr. *Stubbe*'s letter to Mr. *Boyle* is dated at *Stratford* upon *Avon*, February 18, 1665-6, and begins thus: "Since the beſt and moſt agreeable retribution I can make you for the honour "you do me in your remembrances, and all your other ſignal favours, is but to "gratify your curioſity with any remarkable intelligence, that may advance either "phyſic or philoſophy, I ſhall endeavour to be as generous in my acknowledgments "to you, as you have always been in obliging me. Since my laſt unto you, my lord "*Conway* did me the honour particularly to invite me to his houſe and acquaintance, "giving me withal a fair opportunity of converſing with Mr. *Greataricks*, and be-"holding ſeveral of thoſe performances, the report whereof, as it gives juſt cauſes "of aſtoniſhment to you that are more remote, ſo the effects fill with admiration the "moſt learned or ſuſpicious beholders. In truth they are ſuch, that he is not at all "obliged to the ignorant for the eſteem he hath acquired, nor is it poſſible for the "moſt tender or ſuperſtitious and cenſorious zealots to deſtroy his repute. He is a "man of a graceful perſonage and preſence, and, if my phantaſy betrayed not my "judgment, I obſerved in his eyes and mien a vivacity and ſprightlineſs, that is no-"thing common. As far as I could inform myſelf by a long and private diſcourſe, "he is a man of a very good life, of tender and charitable principles, as extenſive "as the effects of his goodneſs are. He profeſſeth conforming unto the doctrine "and diſcipline of the church of *England*, yet without that cenſoriouſneſs, whereby

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[g] Ibid. p. 25—28.

[h] Ibid. p. 28—40.

“ ſome ſignalize themſelves. His thoughts concerning himſelf are modeſt and humble; and he preſumes ſo well of others, that even in ſome colourable circumſtances he regulates his apprehenſions by the revealed mercies of God, and not the ſeverity of men. In fine, without any prejudice to this age be it ſaid, he ſeemed to me, by his faith and by his charitableneſs, to include in his ſoul ſome grains of the golden age, and to be a relick of thoſe times, when piety and miracles were ſincere.” The poſition which Mr. *Stubbe* maintains in this letter, is[i], that *God had beſtowed upon Mr.* Greatraks *a peculiar temperament, or compoſed his body of ſome particular ferments, the effluvia whereof being introduced ſometimes by a light, ſometimes by a violent friction, ſhould reſtore the temperament of the debilitated parts, reinvigorate the blood, and diſſipate all the heterogeneous ferments out of the bodies of the diſeaſed, by the eyes, noſe, mouth, hands, and feet.* He then endeavours to explain the nature and manner of Mr. *Greatraks*'s working upon his patients for their cure.

BUT ſome paſſages in this letter of Mr. *Stubbe* giving offence to Mr. *Boyle*, the latter wrote, immediately upon the receipt of it, animadverſions upon it, in a letter to him, which having never been yet publiſhed, deſerves a place here from the original in his own hand-writing.

“ SIR, March 9, 1665-6.

“ IT was ſo late yeſternight before I received your account of Mr. *Greatraks*'s ſtupendous performances, that I had much ado to run it over before I went to bed; and this morning being to take care of ſome little affairs in order to a remove, that I am to make in the afternoon for ſome days, I am obliged to anſwer your letter in as much haſte as you tell me you writ it in; which intimation will, I hope, excuſe me to you for my not taking a ſolemn notice of thoſe ſuperfluous acknowledgments you are pleaſed to begin with, for ſervices that are not conſiderable enough to deſerve or expect a public retribution, having been but ſuch, as a leſs intereſt in the Muſes than yours would have entitled you to from one, that is ſo much their ſervant as I.

“ To begin then, I muſt confeſs to you, that I was ſomewhat ſurprized to find this epiſtle of yours brought me from the preſs, before I had ſeen it any other way; and it is no ſmall trouble to me, both upon your ſcore and my own, that I did not ſee the manuſcript before it came abroad. For if I had ſeaſonably ſeen what you wrote about miracles, I ſhould freely have diſſuaded you from publickly addreſſing to me, what I cannot but much diſſent from; and perhaps I ſhould have been able to prevail with you to omit all that part of your epiſtle. For beſides that ſince Page 25. you take notice yourſelf of the prejudice your former meddling with theological matters has done you, you can ſcarce doubt, but that it has made many perſons indiſpoſed to put the beſt conſtructions upon what you write; beſides this, I ſay, I confeſs I think you might have ſpared ſo much pains as you take in the former part of your letter, to ſhew, that Mr. *Greatraks*'s gift may be miraculous, ſince the latter part of it is employed to make out what he performs by natural means. For my part, though I be very backward to believe any ſtrange thing in particular, though but purely natural, unleſs the teſtimonies that recommend it be proportionable to the extraordinarineſs of the thing propoſed; yet I remember not, that I have hitherto met with (no more than you have done) any, at leaſt any cogent proof, that miracles were to ceaſe with the age of the apoſtles; and not only

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[i] P. 10, 11.

“ the excellent *Grotius*, but *Tertullian*, *Juſtin Martyr*, *Cyprian*, and other ancients tell
“ us, that the power of ejecting devils out of poſſeſſed perſons laſted long after that,
“ and was not unfrequent in the Chriſtian church. And therefore if thoſe relations
“ of Mr. *Greatraks*'s cures, that I have not yet ſeen, ſhall convince me, I ſhall not
“ ſcruple, ſince his belief and life give me no juſt ſuſpicions to acknowledge my
“ conviction, and to rejoice in the appearing of a proteſtant, that is enabled and forward
“ to do good in ſuch a way, eſpecially in an age where ſo many do take upon
“ them to deride all that is ſupernatural; and, whilſt they loudly cry up reaſon, make
“ no better uſe of it than to employ it, firſt to depoſe faith, and then to ſerve their
“ paſſions and intereſts. But by what hitherto appears to me of Mr. *Greatraks*'s
“ cures, I muſt take leave to think, that either they are not real miracles; or, if they
“ have any thing in them of a ſupernatural gift, it is ſo far ſhort of the gifts of our
“ Saviour Chriſt and his apoſtles, that I preſume your friends will think, that if it
“ were not the effect of your haſte, it was rather to ſhew your wit than declare your
“ opinion, that you ſeem to make a parity between them. And for my part I ſhould
“ in that caſe, reflecting upon the paſſage you cite, that *there are differences of adminiſtrations,*
“ *but the ſame Lord*, think it more fit to look upon this gift of Mr. *Greatraks*,
“ as a diſtinct and inferior kind, than degrade the unqueſtionable miraculous
“ gifts of the apoſtles, to depreſs them to the ſame level with his. For whether or
“ no it may queſtion the truth of his gifts being miraculous, yet certainly it will
“ leſſen the degree of it, that there are many diſeaſes which he will not ſo much as
“ meddle with; that there are others, which he attempts to cure, but cannot; that
“ there are others, wherein the good he does is not laſting, ſo that his patients are
“ rather relieved than recovered; and that alſo there are others, wherein ſo durable
“ a contact and friction is requiſite, as makes a great reſemblance betwixt the operations
“ of his hand, and the actions of phyſical agents. As to what you ſay about
“ *Trophimus*, whom St. *Paul* left ſick at *Miletus*, and your ſuppoſition, that he may
“ hence have left many ſo elſewhere; the latter part of it, if it be taken in a ſenſe pertinent
“ to the preſent caſe, is precarious; and as to the former, it concludes not, that
“ St. *Paul* could not cure him, unleſs you can make it appear, that he endeavoured
“ it, as you confeſs Mr. *Greatraks* did to cure the excellent lady *Conway* and others.
“ What you ſay, that you believe, that there wanted not at *Corinth* thoſe that had
“ the gift of healing, nor an effectual anointing with oil; yet did the miſdemeanors
“ of ſome draw upon ſeveral of them *irrecoverable* (a main circumſtance, which I
“ read not in the text) ſickneſs and death, is only affirmed. But if it were granted,
“ that perſons ſo qualified were in that church, yet it will not follow, that they were
“ unable to cure thoſe that died, unleſs you make it out that they endeavoured it.
“ For, to note this once for all ſuch caſes, there is no juſt cauſe to ſuppoſe, that God
“ did ſo vouchſafe the gift of miracles to all, that in thoſe times had it, as that it
“ was to be exerted on all occaſions; but that he who gave it, did likewiſe by ſome
“ way or other ſignify to them, when it was fit for his glory (for whoſe ſervice,
“ and not their own or their friends outward advantages they were intruſted with
“ it) that their miraculous endowments ſhould be exerciſed. And to argue from
“ their not performing what it appears not that they did attempt, to their diſability
“ to perform what they attempted, will be inconſequent. St. *Peter* was able, when
“ it was requiſite, to ſtrike *Ananias* and *Sapphira* with ſudden death, and St. *Paul* to
“ ſtrike *Elymas* with blindneſs; yet theſe apoſtles never made uſe of this fatal power
“ for their own or their friends defence, when they ſuffered thoſe rude violences,
“ from which the exerciſe of it would in all probability have protected them. And

In his annotations on the 16th of St. *Mark*.

“ our Saviour himſelf, though he could turn water into wine (as at the wedding in
“ *Cana* of *Galilee)* yet we read not, that ever he thought fit to do it but once; nor
“ can it be well argued from his ſending his diſciples to buy loaves, that he was not
“ able miraculouſly to multiply bread, or feed a greater number than his ordinary
“ retinue by extraordinary means. As to what you ſay, that there were ſome diſeaſes
“ that the diſciples could not cure; I remember not any ſuch thing in the goſpel,
“ unleſs you mean, as I ſuppoſe you may, the youth mentioned in the 17th of
“ *Matthew:* but he, the ſtory informs us, was *poſſeſſed with the devil*; of which poſſeſſion
“ the bodily diſtemper (whatever that were) was but an effect, and therefore
“ in the beloved phyſician's goſpel, both the paroxyſm is deſcribed by *a ſpirit taketh* Luke ix. 39. v. 42.
“ *him*, &c. and *the devil threw him down and tare him*; and in the narrative of the cure Chap. xvii.
“ it is ſaid, that *Jeſus rebuked the unclean ſpirit*, which in St. *Matthew* is called *the* v. 18.
“ *devil*; and is ſaid *to depart out of him*. And as to that part of this ſtory in the goſpels,
“ that relates to the *caſting out of the devil*, I ſhall not ſpend time to diſcourſe
“ upon it, nor to enquire what was the extent and conditions of the power Chriſt
“ gave his diſciples, whilſt they were, as his uſual train, in their attendance upon
“ himſelf; yet it plainly appears by their divine maſter's rebuking them in the forecited
“ chapter of St. *Matthew* for their unbelief, as that which alone had hindered
“ the ejection of the evil ſpirit, that it was not want of power, but their culpable
“ neglecting to uſe the proper means that left the devil unejected. But when they
“ were ſent forth and commiſſioned by Chriſt, not only we never read that they attempted
“ a cure which they could not effect, but their performances were divers of
“ them of a much higher nature than Mr. *Greatraks*'s. For not only *Peter* and *John*
“ cured inſtantly a man that had been lame from his mother's womb; *Peter* cured
“ in a trice at *Lydda*, one whom the palſy had kept bed-rid eight years together;
“ *Paul* in the ſame manner at *Lyſtra* made one leap and walk, that had been a cripple
“ from his mother's womb; but the ſame apoſtle had ſo exuberant and diffuſive a
“ power of chaſing, not only diſeaſes, but devils, that he did it at a diſtance by the Acts xix.
“ intervention of handkerchiefs and aprons that were brought from his body to the 12.
“ ſick or poſſeſſed. To which I ſhould add the cures intimated to be wrought by
“ St. *Peter*'s ſhadow, if I did not conſider, that it may be ſaid, that to come ſo near
“ as to have that paſs over them, was to come within the atmoſphere (if I may ſo
“ call it) which the effluvia of his body made miraculouſly ſanative. To enumerate
“ all the other effects of the diſciple's ſupernatural gifts, would be to tranſcribe
“ a good part of the Acts of the Apoſtles. And if I could imagine, that any diſcerning
“ men were in danger of thinking, that becauſe you have made it plauſible,
“ that ſome of your *Thaumaturgus*'s cures are performed by an ἰδιοσυγκρασία, or ſome
“ kind of complexional efficacy, I ſhould add, that I think ſuch a ſuſpicion exceeding
“ ill-grounded, becauſe there are divers phænomena in the miracles of our Saviour
“ and his apoſtles, that do not at all agree with ſo injurious an hypotheſis as that
“ would be. For, to repeat nothing of what I have ſaid already, our Saviour could
“ communicate the power of working miracles to others at his pleaſure, (which I
“ think you do not believe Mr. *Greatraks* can do to you) as in the caſe of the ſeventy
“ diſciples; and that many years after his death, as in the caſe of St. *Paul*; divers
“ of his miracles were done on abſent perſons, as that on the Centurion's dying
“ ſervant and others. The mute fiſhes obeyed him, and that ſo ſtrangely, that
“ being at a diſtance one of them brought St. *Peter*, not only himſelf, but a determinate
“ piece of money. His power reached not only to living creatures, but inanimate
“ ones, as the ſea and the wind; the former of which ſupported him when

“ he walked on it, and both the former and the latter obeyed him. His power, and
“ that of his apoſtles, (which will not, I ſuppoſe, be aſcribed to a peculiar temperament,
“ reached not only to the curing of the ſick, but the raiſing of the dead,
“ (which if the devil could do, he would have far more followers than now he has,
“ though they be but too many) and divers of the apoſtles cures were done without
“ any contact at all but barely by their word; by which alſo they could do harm
“ as well as good, and bring diſeaſes and death as well as chaſe them, which will not,
“ I preſume, be aſcribed to an exuberance of health (or *pluſquam perfect* tincture, as
“ the chemiſts ſpeak). Beſides that, we may well be induced to believe, that even
“ thoſe cures, wherein they did touch the ſick, were miraculous, both by other
“ reaſons that may be drawn from what has been already ſaid, and by theſe two.
“ The one, that there is no ground to believe, that ſo many differing and unallied
“ perſons in one place, and at one time, ſhould have that ſanative *ἰδιοσυνκρασία*,
“ of which many ages have produced us ſo few unqueſtionable inſtances, if any other
“ than what your *Thaumaturgus*, ſuppoſing his ſuch, affords; for the teſtimonies you
“ cite of *Rodericus a Caſtro* are not cogent, eſpecially conſidering his religion and
“ that of his country; and his *creduntur* is an expreſſion that rather argues his diffidence
“ than belief. And the ſecond, that the apoſtles and their contemporary
“ workers of miracles, who profeſſed themſelves ſent by God to promulgate a new
“ doctrine, had other indowments that were confeſſedly miraculous, (and therefore
“ needed not a complexional phyſical power of healing) as is evident in the gift of
“ tongues ſo illuſtriouſly poured out on the apoſtles at the feaſt of Pentecoſt. And
“ to this ſo much more to the ſame purpoſe might be added, that though I have
“ given ſome proof of my not being afraid to propoſe paradoxes if I think them
“ truths, yet I ſhall clearly profeſs to you, that as much as I aſcribe to the Corpuſcularian
“ philoſophy, (both in my other tracts, and profeſſedly in a new one, that
“ this week comes out in favour of it) I am far from believing, that any mechanical
“ or phyſical hypotheſis will make out thoſe ſupernatural phænomena, without having
“ a recourſe to the miraculous interpoſition of God. And when I had the curioſity to
“ conſider the grand opinions that are entertained among men about religion in
“ general, I have long looked upon thoſe enemies to Chriſtianity as none of the wariest
“ and formidableſt, that granting the truth of the hiſtorical part of the New Teſtament,
“ (which relates to miracles) have gone about to give an account of it by
“ cœleſtial influences, or natural (though peculiar) complexions, or ſuch conceits,
“ which have quite loſt them, in my thoughts, the title of knowing naturaliſts. But
“ I muſt not forget, that the opinion I have been oppoſing may poſſibly be diſclaimed
“ as well by you as me, though I wiſh ſome readers do you not the diſcourteſy
“ to take a riſe from your epiſtle to maintain it. And by this time I preſume
“ you expect that I ſhould ſay ſomething to the hiſtorical and phyſical parts of your
“ letter, after having inſiſted ſo long on the theological. But though to comply with
“ that expectation and my own haſte, I ſkip other points relating to divinity, and
“ what were not proper for me to meddle with; yet there are two things that
“ I muſt not leave untaken notice of. And firſt, as for the apology you conclude
“ with for recommending ſome phyſical directions after Mr. *Greatraks* ſtroking, I
“ think moſt of your readers, eſpecially divines, will much more eaſily forgive a phyſician
“ the preſcribing of medicines, which is a main part of his profeſſion, than
“ the irreverent mention you are pleaſed to make of the language of one of the penmen
“ of the Old Teſtament, and all but one of the New; that mention neither being
“ required of you as a phyſician, nor appearing at all neceſſary to be made in

“ that place; and, but that you wrote in haſte, neither would you have publiſhed
“ that, whereby you will needleſsly diſpleaſe many, (and I am confident divers of
“ your own profeſſion) nor would you of all men have addreſſed it to me, who have
“ publickly given cauſe to think it muſt be peculiarly unwelcome to me. The other
“ theological paſſage not to be overlooked, is that which contains an opinion, which
“ I think Chriſtianity itſelf may be ſo much concerned in, that I cannot forbear, as
“ I paſs along, to take ſome little notice of it, as by telling you, that in the opinion
“ you hold as an *undoubted truth, that God had permitted all religions to have their real*
“ *miracles*, I do very much diſſent from you. I have not now time to examine how
“ far it may be ſaid, that among a people or church, where God has already eſtabliſhed
“ a doctrine by divine and acknowledged miracles, and has expreſsly foretold,
“ (or at leaſt ſufficiently intimated) that it is poſſible he may permit ſome ſeducers
“ to work ſtrange things to try the people, to whom a divine doctrine atteſted by
“ miracles has been delivered; I muſt not, I ſay, now ſtay to examine, how far one
“ may grant, that in ſuch a caſe, and after ſuch a warning, falſe prophets may be
“ permitted to work miracles; though for my part I ſee no cogent reaſon, why the
“ τέρατα ψεύδους of the man of ſin may not by a conſtruction, which we find exceeding
“ common in Hebrew writings, be rendered (as our Engliſh tranſlation ſeems to interpret)
“ *falſe wonders*, i. e. miracles, that are as well falſe themſelves, as are the
“ doctrine they are brought to confirm. But though, as I was ſaying, I muſt not
“ now inſiſt on a debate of what may or may not be granted in the like caſes, that I
“ have propoſed; yet ſpeaking as you do indefinitely of all religions, and taking real
“ miracles for ſuch exertions of God's power, as are above the power of creatures, I
“ am ſo very unapt to believe the ſtories of the Turkiſh, Heatheniſh, and other
“ miracles pretended to by divers enemies of the Chriſtian religion in confirmation of
“ theirs, and ſome enquiries I have made have ſo confirmed me in my diffidence, that
“ when I conſider the nature and uſe of true miracles, (for I ſpeak not of natural
“ prodigies, ſorceries, or impoſtures) I confeſs I am ſo little convinced, that I ought
“ to believe the ſuſpicious and unlikely reports that go of the miracles of the Turks
“ and Heathens, divers of which are ſaid to have been done in times and places
“ when there were no miraculous gifts in the church to controul them, that perhaps
“ I ſhould not believe thoſe that I find recorded in the ſcripture itſelf, if the relations
“ of them were not recommended by ſuch concurrent characters of credibility,
“ as would make my rejecting them an obſtinacy, and as are of another gueſs weight
“ than thoſe that countenance thoſe relations whereto I do not give credit. And I
“ little doubt, but if the pretended miracles of *Pyrrhus* and *Veſpaſian* had been
“ watched and conſidered by Mr. *Stubbes* as narrowly as thoſe of Mr. *Greatraks*'s
“ had been, you would have found at leaſt as much reaſon to aſcribe their cures, as
“ his, to phyſical cauſes, if not to ſome miſtake, colluſion, or flattery, which the
“ perſons on whom thoſe wonders are fathered, render the more ſuſpicious. And
“ though perhaps no body will more willingly grant than I, that the ſoundneſs of the
“ doctrine ought to go along with thoſe miracles that bear witneſs to religion, yet I
“ doubt, whether the holineſs of doctrine, which may be allowed to ſhow that a religion
“ may be inſtituted by God, will, without the aſſiſtance of miracles, ſufficiently
“ aſſure men that it really is ſo.

“ I COME now to the phyſical part of your letter, wherein many that will diſſent
“ from you about the theological, will, I preſume, confeſs, that divers things are plauſibly
“ propoſed. And as you will believe, that I am of their mind in the general, ſo
“ as to particulars I have not time to enter into a debate of it, eſpecially ſince you

“ ſeem willing to refer that taſk to the Members of the Royal Society, who, if they
“ think fit to meddle with it, are the likelieſt perſons to give a fair account of it.
“ But in the mean while to keep ſcribbling as long as the time will permit, I ſhall
“ briefly take notice of ſome particulars in the caſual order, wherein they offer themſelves
“ to my thoughts, premiſing only in general, that you write to one that is
“ almoſt as backward to acquieſce in the explications of ſtrange things, as to believe
“ the narratives.

“ I AM not yet fully convinced, that there is in what either you or the ingenious
“ Mr. *Foxcroft* relate upon your own obſervations, (of which in this whole letter I
“ do upon your teſtimony ſuppoſe the truth) any thing that is purely ſupernatural,
“ (unleſs in the way, wherein he was made to take notice of his gift, and exerciſe it,
“ there may be ſomething of that kind) and therefore till the contrary doth appear,
“ I hold it not unlawful to endeavour to give a phyſical account of his cures, and
“ to enquire whether his touch be any more than a more noble ſpecific, that reaches
“ not to all diſeaſes, or to moſt, but to more than the generality of ſpecificks, whoſe
“ operations are uſually more confined.

“ THOSE phyſicians, and other learned men, that think every diſeaſe muſt be overcome
“ by diſtinct and appropriated remedies, that are contrary to the particular diſeaſe
“ upon the account of ſome known quality, as that a fever being a diſeaſe hot
“ and dry, muſt be cured by remedies cold and moiſt, will perhaps think it incredible,
“ that the application of one thing ſhould work ſo many differing effects, and
“ cure ſo many differing diſtempers. But though I think it is ſtrange and extraordinary,
“ yet I dare not ſay it is impoſſible to mere nature. This would be ſufficiently
“ manifeſted, if the relations, that *Helmont* makes of the various and ſudden
“ cures he ſaw performed by *Butler*'s drif either touched with the tip of the tongue,
“ or by outward application, were proper to be urged on this occaſion. But without
“ having recourſe to them, the ſtrangeneſs may be leſſened, if we conſider theſe two
“ things; the one, that diſeaſes are not always ſo differing in their nature and eſſence
“ as they are commonly thought, but the ſame morbifick matter for eſſence may
“ produce very differing ſymptoms, which may be taken for ſeveral diſeaſes, according
“ to the condition of the parts that it reſides in, or works upon, (in all or moſt
“ of which it may be ſubdued by the ſame remedies, which may deſtroy its texture,
“ giving it a more innocent one, or proſcribe it) as we ſee in ſome metaſtaſes of the
“ morbific matter, which according as it ſucceſſively affect differing parts has the
“ appearance of ſeveral diſeaſes, as in ſome of the inſtances you give, and in hyſterical
“ fits, which ſometimes counterfeit almoſt all diſeaſes, and in other examples,
“ which I have elſewhere taken notice of. The other, that ſome remedies are of that
“ noble nature, that their efficacy reaches to diſeaſes that ſeem of contrary kinds, as
“ may be ſeen in the operations of the *Spa* waters, and thoſe which I have in ſome
“ lucky ſeaſons obſerved at our own wells near *Tunbridge*; and we ſee how many differing
“ diſeaſes are cured by an excellent air, which keeps me from wondering at that
“ ſentence of *Hippocrates*, γῆν μεταμείβειν ξύμφορον ἐπὶ τοῖσι μακροῖσι νουσήμασι.

Hippocratis Lib. tertii, Sect. 5ta Popular.

“ As to what ſeems your main hypotheſis, that Mr. *Greatraks* performs his cures
“ by the ſtrengthening and reinvigorating of nature, which being relieved and fortified
“ by the ſanative effluvia that paſs from his body into the body of the patient,
“ doth afterwards vanquiſh the diſeaſe herſelf, I doubt not, that you will have many
“ of your readers of that opinion with you; and particularly thoſe Helmontians and
“ other chemiſts, that hope or plead for univerſal medicines, operating by way of
“ reſtoratives, will be glad to find you to countenance their tenet. For my part,

“ unleſs I could ſend you what I once drew up by way of diſquiſition about the received notion of nature (wherein perhaps I do not acquieſce) I cannot think it proper to mention to you my particular thoughts of the power of ſtrengthened nature. But ſome ſcrupulous perſon will not only deny, that nature alone, though fortified with any thing, that is but a cordial, can cure all diſeaſes (as for inſtance, the ſtone in the bladder, or kidneys, when it is confirmed and grown too big to be voided by the urinary paſſages) but will perhaps enquire, whether it does appear that Mr. *Greatraks*'s touch is poſitively a cordial, and not barely or chiefly ſo, as it relieves nature by freeing her from ſome diſtempers that oppreſſed her, as when burned feathers remove the faintings of hyſterical women, and others are freed from lypothymias by being pinched, or having cold water thrown in their faces. But theſe ſcrupulous perſons (if you meet with any ſuch) will perhaps be more eaſily ſatisfied in other caſes than about the cure of wens. For whereas Mr. *Greatraks*'s touch muſt, according to the forementioned hypotheſis, free the patient from them, by invigorating nature, and enabling her to diſcuſs or preſcribe thoſe preternatural collections of matter; I muſt inform you, that to omit *Helmont*'s ſtory, I was the other day viſited by an eminent phyſician, who not long ſince upon occaſion of a fine pendulum clock, that I was taking notice of in his chamber, told me, that it was preſented him by ſuch a one (a well known perſon, that has the honour to be one of his Majeſty's domeſtics) for having cured his daughter of an almoſt monſtrous wen by the laſting application of a dead man's hand. And ſuch another cure of a wen, or ſome ſuch tumour, I remember our famous *Harvey* related to me as performed (if I very much miſtake not) by himſelf, by ſtroking the wen, as *Helmont* preſcribes, with a dead man's hand. But this advantage you have, that however it will perhaps be very difficult for thoſe, that upon theſe, and the like grounds, do reject the ſtrengthening of nature, to pitch, among the hypotheſes that are yet noted, upon a more plauſible one, eſpecially ſince your great *Hippocrates* is ſuppoſed to be of the like opinion when he pronounced that ſentence, Νȣσῶν φύσεεις ἰητροὶ. But I hope you will allow me to add (what if I forget not you yourſelf ſomewhere ſeem to intimate) that Mr. *Greatraks*'s touch may work not only as a cordial or ſtrengthener of nature, but as a proper remedy of qualities oppoſite to thoſe of the cauſes of the diſeaſes he cures. For I do not ſee, why it may not be poſſible for the ſanative, and perhaps anodyne ſteams of his body to be of ſuch a texture, that they may both reinvigorate the ſpirits, and by appropriated qualities oppoſe and ſubdue the morbific matter or ferment, as we ſee, that lemons (to name no other things) have, beſides their cordial virtues, the power to cool, incide, and reſiſt putrefaction, and ſtrengthen the ſtomach, and promote urine, *&c.* And when I conſider the ſtrange effects that longing has produced in teeming women, and the ſudden and manifeſt operations I have ſometimes known paſſions to have upon the body, I am not ſure, but that ſomething elſe may be fit to be taken into conſideration about Mr. *Greatraks*'s cures, and that in ſome of his patients the mind by exalted imagination, and by ſtrong paſſions (which in ſo extraordinary and ſuppoſedly ſupernatural as well as public way of healing as his, there are ſeveral circumſtances that may heighten) may have an intereſt in the recovery, by occaſioning uſually a great and therefore probably ſometimes a lucky commotion in the blood and ſpirits, upon whoſe motion (and thereby texture) thus altered, that the amendment, if not recovery, may ſometimes enſue, the obvious, though unheeded inſtances of thoſe that are freed from the hiccough, by being told of ſome feigned ill news, or even of ſome other thing, that but excites a great at-

" tention of mind, and much more the examples, that have been divers times ſeen " of diſeaſes frighted, or by fits of paſſion driven away, make me think it not abſurd " to ſuſpect.

" WHAT to mention of morbific ferments, I know divers ingenious readers will " approve, and they ſeem to be of good uſe in the explication of diſeaſes. But " whether all diſeaſes require ferments; and whether your doctrine about them " be as well applicable to the reſt, as to ſome, is a diſquiſition, that I ſhall willingly " leave to thoſe learned men of your faculty, that our age and country abounds " with.

" WHAT you mention of Mr. *Greatraks*'s exciſion of wens and immediately ſtanching " the blood, as it much takes off from the appearance of a miracle, ſo as to the " phyſical cauſe of it, it may or may not have a great deal of difficulty, according to " ſome circumſtances, which I would gladly be informed of. For if the wen be " ſuch, or ſo placed, that it being cut out by a chirurgeon, no ſuch veſſels would be " cut as that, according to the common courſe of nature, the effuſion of blood would " be great, it is no great wonder it ſhould ſtop after the application of his finger, " that perhaps compreſſes the orifice of the veſſels; and the marvel is confined to the " ſudden ceſſation of the pain, which may be attributed to ſome anodyne effluvia " iſſuing from his hand. But if in the extirpating of the wen any great veſſels be " cut aſunder, the ſtanching of the blood ſeems to be more difficult to be accounted " for by ſuppoſing with you, that his cures are performed by reſtoring the tempera- " ment of the debilitated parts, and reinvigorating the blood. For though this be " granted, it will not be ſo clear, that the blood ſhould by his touch be ſo quickly " ſtanched, ſince in a perſon, that were perfectly healthy, and where the blood " needed no invigoration, upon ſuch a ſolution of continuity there would be a large " effuſion of the continually circulating blood, unleſs appropriated means be uſed to " ſtay it, as is daily ſeen in the wounds received by healthy men. The obſervation " of ſtanching of blood out of *Platerus* I well remember, as having had occaſion " to make uſe of it, together with an experiment of his own, that he annexes to it, " of a perſon, that he himſelf cured by the ſame means. But I doubt, whether " theſe will reach your caſe, till you have made out the cauſe of this operation. For, " that there may be very differing applications, that will ſtanch blood, may be learned " from him in the ſame place, where he relates how he made a chirurgeon ſtanch a " dangerous hæmorrhage, not by the application of a bloody part of a hen, but of " aqua fortis, which is corroſive inſtead of being balſamical. If indeed there be any " truth in the ſtrange relations I have met with in ſome navigators of an Indian " ſimple, that worn by a great commander, kept his wounds from bleeding (which " what I elſewhere mention of the effect of the moſs of a dead man's ſkull upon a " noble friend of mine, does, though not approach, make the leſs incredible) it may " be more plauſibly argued, that Mr. *Greatraks*'s finger may in this caſe operate as a " natural agent; but then there will remain ſome little ſcruple, whether the effect be " wrought in the particular way you ſeem to pitch upon, till it appears, that theſe " things work either as reſtorers of nature, or as balſams, and not rather as medicines " (by ſome hidden virtue) appropriated to ſuch particular effects, which ſeems to be " the caſe of the moſs of dead men's ſkulls, and the holding of ſpiders to the noſe of " bleeding perſons. But perhaps a fuller information of circumſtances will eaſe me " of this ſcruple, and therefore I ſhall not inſiſt upon it.

" WHAT you ſay of the ſubtlety of the effluvia, and of the great efficacy they are " capable of, will not be much ſtruck at by a Corpuſcularian. And if I could think

" it proper here to add ſome of the inſtances of that kind, which I have lying by
" me in my notes about Occult Qualities, (as they are commonly called) perhaps they
" would afford no deſpicable confirmation both to what you here ſay, and to what I
" have elſewhere written about the power of inviſible corpuſcles. And I am the
" more perſuaded to think great matters performable by them, both by ſome odd
" obſervations, that I have ſince met with of the efficacy of the even ſolid parts of
" dead animals, and particularly of a ſea-horſe tooth outwardly applied to the
" body, and by conſidering, that a ſanative temperament may reaſonably be ſuppoſed
" capable of diffuſing its virtue by contacts more plentifully and more powerfully
" in a living body of a ſanguine complexion, where the natural heat of the blood
" and ſpirits, being vegete and active, inceſſantly emit ſo great a plenty of inſinuating
" ſteams, as living bodies, that tranſpire freely, appear to do by the notable
" obſervations of *Sanctorius*'s ingenious *Medicina Statica* (not to mention my own
" trials to the ſame purpoſe). I will only add in favour of the efficacy of ſteams,
" that I have ſeveral times made a ſlight preparation of ſal armoniac (that I have imparted
" to divers ingenious men) by the odour of which more than once or twice,
" ſome, that have ſuddenly fallen down and were taken for epileptic, have been in a
" few minutes, after I had held a ſmall vial under their noſes, brought to themſelves
" again; hyſterical vapours, and ſome painful dulneſſes of the head, have
" been often, as the patients aſſured me, diſſipated for the time in a trice; and there
" are at leaſt two or three able phyſicians (whereof but one is unknown to you) who
" will aſſure you, they have (though not conſtantly) preſently cured the tooth-ach by
" the ſteams of that ſame preparation of ſal armoniac, which they were pleaſed to
" ſend to me for; and of which, if I had time, I could tell you ſome other feats
" wrought without the contact of the viſible body.

" As to what you ſay about the poſſibility of freeing patients from the morbific
" matter (whoſe bulk oftentimes is not near ſo great as the miſchief it does) without
" purging, vomiting, and the like evacuations; I am glad to be able to confirm by
" your teſtimony what I ventured to publiſh about three years ſince to the ſame
" purpoſe, where I endeavoured to bring ſpecifics out of the diſeſteem of a ſort of
" learned phyſicians of a neighbouring country, that were very ſevere to them; and
" where I remember I attempted to confirm the opinion you now defend, by the
" inſtances, among others, of thoſe, that are cured of agues and other diſeaſes
" by ſudden frights, where there uſually intervenes no ſenſible evacuation of peccant
" matter.

" WHAT you teach, that as to the pains and diſtempers, that are thought to fly
" from him from one part of the body to another, they avoid not his hand; but his
" touch and ſtroke ſo invigorateth the parts, that they reject the heterogeneous ferment,
" till it be expelled the body at ſome of thoſe parts he is thought to ſtroke it
" out at, is a handſome conjecture, and agrees very well with your hypotheſis. But
" I perceive by your way of mentioning ſimilar attraction, you are (and that very
" juſtly) diffident of its being granted you by me, who am, I confeſs, very ſhy of
" admitting any thing as a principle in matters purely phyſical, that I cannot well
" underſtand, and do not find well proved. But you are freed from further effects
" of my ſcrupulouſneſs, by my being called upon by the late time of the day to take
" horſe. I ſhall not make you any apology for taking the ſame liberty to diſſent from
" you in ſome points, that you took in propoſing thoſe opinions to me, which the
" knowledge of my principles might eaſily make you think I would diſſent from.
" And ſince both your letter and this anſwer are written in haſte, as I am willing that

" my

"haſte ſhould be my excuſe for any thing, that the leiſure of reading over your letter
"a ſecond time, and of reviewing what I have confuſedly ſcribbled might have made
"me avoid or mend; ſo I would not be ſo unkind to you as to impute to your deſign, whatever ſome critical readers may plauſibly enough infer to the prejudice of
"religion from ſome unſtudied expreſſions of your *raptim ſcripta*; nor ſhall I take
"them for your deliberate tenets, unleſs upon a review you ſhould ſurpriſe me with
"declaring them to be ſo; but am very willing to leave you the liberty of explaining
"yourſelf in any thing, wherein the impetus you tell me you writ with, may have
"had an influence on your pen. And as for the phyſiological part of my letter,
"though I have annexed it, leſt you ſhould in vain expect ſome of my thoughts of
"the phyſical part of yours; yet not having a competent knowledge of the matters
"of fact to ground an hypotheſis upon, I have been obliged, for want of ſuch an
"information, and out of an unwillingneſs either to ſay nothing to you or ſpeak
"raſhly, rather to diſcourſe on ſome paſſages of your epiſtle, than to eſtabliſh any
"theory about the thing itſelf, which future relations may oblige me to alter. And
"therefore, as I propoſe my ſudden apprehenſions but as conjectures, that amount
"not to an opinion, ſo I ſhall not be concerned, eſpecially being otherwiſe ſufficiently
"buſied, to contend for them. And I the more willingly ſuffer my occaſions to
"keep me from troubling you with any more of theſe phyſical diſcourſes, becauſe
"you have in ſome ſort addreſſed your letter to the highly learned Dr. *Willis*, from
"whoſe profeſſion and abilities you may expect a better account of what is like to be
"applauded or queſtioned in the phyſical paſſages of your letter, than from him,
"whoſe parting haſte obliges him to leave the hiſtorical part untaken notice of,
"ſave by his wonder at it, and thanks for it, and to ſubſcribe himſelf ſomewhat
"abruptly,

"Sir, your very humble ſervant."

AT the end of the original of this letter of Mr. *Boyle*, I find in his hand-writing an account of ſome cures performed in his preſence in April 1666, by Mr. *Greatraks*, who in the month preceding had been very ſeverely attacked in a pamphlet, ſuppoſed to be written by Mr. *David Lloyd*, reader of the *Charter-houſe*, and printed in 4to, under the title of *Wonders no Miracles: or Mr.* Valentine Greatraks'*s Gift of healing examined, upon occaſion of a ſad Effect of his ſtroking, March the 7th*, 1665, *at one Mr.* Creſſet'*s houſe in* Charter-houſe-yard. *In a letter to a reverend Divine living near that place.* This letter is dated March the 13th, 1665-6, and obliged Mr. *Greatraks* to vindicate himſelf from the imputation caſt upon him in it by publiſhing at *London* in 1666, in 4to, *A brief Account of Mr.* Valentine Greatraks, *and divers of the ſtrange cures by him lately performed. Written by himſelf, in a letter addreſſed to the honourable* Robert Boyle, *Eſq*; *Whereunto are annexed the Teſtimonials of ſeveral eminent and worthy perſons of the chief Matters of Fact therein related.* In this letter, which is dated at *London*, May the 8th, 1666, he obſerves, that though he had not the happineſs formerly to have an acquaintance with Mr. *Boyle*, *yet I was*, ſays he, *no ſtranger to your worth and virtue, which have made you as much admired and reverenced abroad, as honoured and beloved at home. And therefore I have aſſumed the confidence to make this addreſs to your honour, whoſe repute and teſtimony to the world will be ſo powerful (knowing your wiſdom, devotion, and learning to be ſo great) that truth may find belief, God have glory, and his poor inſtrument be juſtified before men, who hath no further deſign in the diſtribution of that talent, which the all-healing God has intruſted him withal, than* *the honour of his Maker, and the good of his poor fellow-creatures, whoſe diſtempers, many of them, neither art nor phyſic probably could reach.* At the concluſion he intimates his deſign, when he had an opportunity, to anſwer all the queries, which Mr. *Boyle* had put to him, in a paper delivered into his hands, and which he was not then at leiſure to ſatisfy. The teſtimonials ſubjoined to this letter are ſigned, among others, by Mr. *Boyle* himſelf, *William Denton*, M.D. *James Faireclough*, M.D. Sir *William Smith*, Bart. Sir *Nathaniel Hobart*, Knt. and Maſter in Chancery, Sir *John Godolphin*, Knt. *Albert Otto Faber*, M. D. Col. *George Weldon*, *William Knight*, alderman of the city of *London*, Sir *Charles Doe*, Sir *Abraham Cullen*, Bart. *John Wilkins*, D.D. afterwards biſhop of *Cheſter*, *Benjamin Wichcot*, D.D. *Ralph Cudworth*, D.D. *George Ruſt*, D.D. and dean of *Connor*, *Simon Patrick*, afterwards biſhop of *Ely*, &c. ſeveral of whom give the higheſt character of Mr. *Greatraks*'s integrity and unblameable converſation in every reſpect.

IN the ſame year, 1666, Dr. *John Wallis* addreſſed to Mr. *Boyle* an *Hypotheſis about the Flux* and *Reflux of the Sea*, by way of letter dated from *Oxford*, April 25, and printed in the *Philoſophical Tranſactions*, N°. XVI. p. 264. which begins thus: “You were earneſt with me, when you laſt went from hence, that I would put in writing ſomewhat of that, which at divers times theſe three or four years laſt paſt I have been diſcourſing with yourſelf and others concerning the *common Center of Gravity of the Earth and Moon*, in order to ſolving the phænomena, as well of the ſea's ebbing and flowing, as of ſome perplexities in aſtronomical obſervations of the places of the cœleſtial bodies. How much the world, and the great bodies therein, are managed according to the laws of motion and ſtatic principles, and with how much of clearneſs and ſatisfaction many of the more abſtruſe phænomena have been ſolved on ſuch principles within this laſt century of years, than formerly they had been, I need not diſcourſe to you, who are well verſed in it. For ſince that *Galileo*, and after him *Torricellio*, and others, have applied mechanic principles to the ſolving of philoſophical difficulties, natural philoſophy is well known to have been rendered more intelligible, and to have made a much greater progreſs in leſs than an hundred years, than before for many ages.”

DR. *Thomas Sydenham* likewiſe dedicated to him that year his *Methodus curandi Febres propriis obſervationibus ſuperſtructa*, printed at *London* in 8vo. This piece being very ſcarce, though a ſecond edition of it appeared in 1668, and it not being reprinted among that great phyſician's works, I ſhall tranſcribe ſome paſſages of the dedication, which places Mr. *Boyle*'s character in a very amiable light, and inform us, that he uſed to attend Dr. *Sydenham* in viſiting many of his patients.

Illuſtriſſimo & excellentiſſimo Domino, Domino ROBERTO BOYLE.

“Uſque adeo bonorum doctorumque animos, quicquid ad te, vir undiquaque nobiliſſime, quoquomodo pertinere conſpicitur, attentos reddit & experrectos, ut eos prævideam ſtatim cognoſcendi avidos, quibus demum fretus rationibus nominis tui longè celeberrimi authoritatem ad hujuſce operis patrocinium aſciſcere ſuſtinuerim. Quid plerique in ſimili negotio pro more cauſari ſoleant, me neutiquam fugit. Eſt ſanè, quod ego (perinde ac faciunt alii) de multis magniſque tuis erga me meritis palam populo commentari poſſem; atque etiam grati animi ſpecimen daturus, honoris ac cultûs, quo te proſequor, duraturum aliquod monumentum, multo verborum apparatu, architectari. Atqui ejuſmodi conſilium (ut liberè

" profitear) in te tam præſidenter interpellando ab animo meo procul abſuit. In
" immenſas laudes tuas deſcendere non inſtitui, aut eximias animi dotes, quibus
" decantatiſſima anteriorum ætatum nomina unus exæquas (uberrimam ſane tum
" mihimet ipſi, tum operi meo qualicunque honorificam præfandi materiam) ediſ-
" ſerere. Enimverò id ipſum tractatus à te jampridem in lucem emiſſi pleniſſimè
" loquuntur; nec alio tibi opus futurum eſt præcone, cujus ſubſidiaria vox nominis
" tui claritatem amplificare ſatagat. Quocirca tritis hiſce jamque tralatitiis præfandi
" argumentis ultro repudiatis, me tractatum hunc ideo in clientelam tuam dediſſe
" profiteor, propterea quod, quemadmodum ego tuo ſuaſu atque inſtinctu hanc pro-
" vinciam ſuſcepi, ita etiam eorum, quæ hic traduntur, veritatem & efficaciam tute
" ipſe, teſtis planè locupletiſſimus, experientiâ aliquoties comprobatam conſpexiſti,
" dum ad illud obſequii identidem deſcenderis (quæ tua & quidem egregia eſt hu-
" manitas) ut temet in adeundis ægris meis comitem mihi adjungeres; in quâ ſanè
" re benignum animi affectum ad honeſta utcunque ſe demittentem, vel invito hujuſce
" ſæculi genio, oſtendiſti."

THE ſame year Mr. *Boyle* publiſhed at *London* in 8vo, his *Hydroſtatical Paradoxes made out by new Experiments, for the moſt part phyſical and eaſy*; which had been preſented to the Royal Society, at whoſe requeſt they had been made in May 1664. In this book, which was publiſhed likewiſe in Latin, he ſhews, that the lower parts of fluids are preſſed by the upper: that a lighter may gravitate upon one that is more ponderous: that if a body contiguous to it be lower than the higheſt level of the water, the lower end of the body will be preſſed upwards by the water beneath: that the weight of an external fluid is ſufficient to raiſe the water in pumps: that the preſſure of an external fluid is able to keep an heterogeneous liquor ſuſpended at the ſame height in ſeveral pipes, though they are of different diameters: that when a body under water hath its upper ſurface parallel to the horizon, the direct preſſure it ſuſtains is no more than that of a column of water, which hath the abovementioned horizontal ſuperficies for its baſis; and if the incumbent water be contained in pipes open at both ends, the preſſure is to be eſtimated by the weight of a pillar of water, the baſis of which is equal to the lower orifice of the pipe (parallel to the horizon) and its height equal to a perpendicular, reaching to the top of the water, though the pipe be much inclined, irregularly ſhaped, and in ſome parts broader than the orifice: that a body in a fluid ſuſtains a lateral preſſure from it, which increaſes in proportion to the depth of the immerſed body in the fluid: that water may be able to depreſs a body lighter than itſelf: that a parcel of oil lighter than water may be kept from aſcending in it: that the cauſe of aſcenſion of water in ſiphons may be explained without the notion of *abhorence* of a *vacuum*: that the heavieſt body known will not ſink of itſelf, without the aſſiſtance of the weight of the water upon it, when it is at a depth greater than twenty times its own thickneſs, though it will nearer the ſurface.

HIS *Origin of Forms and Qualities, according to the Corpuſcular Philoſophy, illuſtrated by Conſiderations and Experiments: written formerly by way of Notes upon an Eſſay about Nitre*, was printed likewiſe in 1666 at *Oxford*, in 4to, and reprinted the year following in 8vo, with the addition of a *Diſcourſe of ſubordinate Forms*. In his *Origin of Forms* he delivers the minds of men from the imaginary and uſeleſs notions of the ſchools about them, which have no foundation in the nature of things, nor in the leaſt promote knowledge, or aſſiſt mankind; but very much diſſerve thoſe great intereſts, by ſetting the underſtanding at reſt in general obſcurities, or employing it in airy ſubtilties and diſputes, and ſo hindering its purſuit of particular cauſes and experimental realities. He lays down the foundations, and delivers the principles of the mechanic philoſophy, which he confirms and illuſtrates by ſeveral agreeable and inſtructive experiments. He ſhews, that the moſt admirable things, which have been taken for the effects of *ſubſtantial Forms*, and are uſed as proofs of notional hypotheſes, may be the reſults of the mere texture and poſition of parts; ſince art is able to make vitriol, as well as nature, and bodies may be produced by human ſkill, the ſuppoſed forms of which have been deſtroyed. He gives us many very ingenious inſtances to prove, that the mechanic motions and order of the parts is ſufficient to yield an account of the difference of bodies, and their affections, without having recourſe to the forms and qualities of the ſchools; as in the reſtoration of camphire to its former ſmell and nature, after its diſſolution and ſeeming extinction; in the changes of the colour, conſiſtence, fuſibleneſs, and other qualities of ſilver and copper; in the odd phænomena of a certain anomalous ſalt, and thoſe of the ſea-ſalt, dried, powdered, and mixed with aqua fortis; in the ſal mirabilis, in the production of ſilver out of gold by his *menſtruum peracutum*, in the tranſmutation of water into earth in a certain diſtillation of ſpirit of wine and oil of vitriol: in ſhort, he makes it evident, that the internal motions, configuration, and poſition of the parts, are all that is neceſſary for alterations and diverſities of bodies; and conſequently, that ſubſtantial forms and real qualities are needleſs and precarious beings.

HE communicated alſo to Mr. *Oldenburgh* ſeveral pieces, which were publiſhed in 1666, in the *Philoſophical Tranſactions*, viz. *A Confirmation of the former Account*, [*Phil. Tranſ.* N°. X. p. 166.] *touching the late Earthquake near* Oxford, *and the Concomitants thereof*: N°. XI. p. 179. *Some Obſervations and Directions about the Barometer*: p. 181. *General Heads for a Natural Hiſtory of a Country, great or ſmall*: p. 186. *A Way of preſerving Birds taken out of the Egg, and other ſmall Fœtuſes*: N°. XII. p. 199. *An Account of a new kind of Baroſcope, which may be called Statical, and of ſome Advantages and Conveniences it hath above the Mercurial*: N°. XIV. p. 256. *A new frigorific Experiment, ſhewing how a conſiderable degree of cold may be ſuddenly produced without the help of ſnow, ice, hail, wind, or nitre; and that at any time of the year*: N°. XV. p. 256. *Other Inquiries concerning the Sea*: N°. XVIII. p. 315. *Promiſcuous Inquiries about Mines*: p. 334. *The Method obſerved by Dr.* Lower *in transfuſing* the blood of one live animal into another: *N°. XX. p.* 353. *Trials propoſed to Dr.* Lower, *to be made by him for the improvement of transfuſing blood out of one live animal into another*: N°. XXII. p. 385. *Propoſals to try the effect of the pneumatical Engine exhauſted, in plants, ſeeds, eggs of ſilk-worms*: No. XXIII. p. 424.

IN 1667 he publiſhed in the ſame *Tranſactions*, *A Confirmation of the Experiments mentioned in* N°. XXVII. *to have been made by Seignor* Fracaſſati *in* Italy, *by injecting acid liquors into the blood*: N°. XXIX. p. 551. *New Experiments concerning the relation between Light and Air in ſhining Wood and Fiſh*, N°. XXXI. p. 581. And *Obſervations and Trials about the Reſemblances and Differences between burning Coal and ſhining Wood*, N°. XXXII. p. 605.

THE ſame year Dr. *Walter Needham* dedicated to him his *Diſquiſitio Anatomica de formato Fœtu*, printed at *London* in 8vo, with this inſcription: *Nobiliſſimo, clariſſimo* Roberto Boyle *Arm. illuſtrium comitum de* Burlington & Orrery *fratri germano, philoſophorum principi, hanc ſuam diſquiſitionem anatomicam ſacram voluit* Gualterus Needham, *M. D.* and he owns in the Preface, that this treatiſe was drawn up and publiſhed at the ſolicitation of Mr. *Boyle*.

In the beginning of the year 1668, he wrote a letter of thanks to Dr. *Thomas Sydenham*, upon receiving from him a preſent of the ſecond edition of his *Methodus curandi Febres*, dedicated to him, as the former had been; to which the doctor returned an anſwer, dated April 2, 1668, in which he confirms his own practice in the ſmall-pox[k], to which Mr. *Locke* had been a witneſs, and with regard to which he declares, that he had found no cauſe from his beſt obſervation, to repent of any thing ſaid by him in this tract *de Variolis*; though he greatly regretted, that he had not ſaid, that conſidering the practices that obtained both amongſt learned and ignorant phyſicians, it had been happy for mankind, that either the art of phyſic had never been exerciſed, or the notion of *malignity* never been ſtumbled upon. "As it is pal-
" pable, *ſays he*, to all the world, how fatal that diſeaſe proves to many of all ages,
" ſo it is moſt clear to me from all the obſervations that I can poſſibly make, that
" if no miſchief be done, either by the phyſician or nurſe, it is the moſt ſlight and
" ſafe of all other diſeaſes. If it ſhall be your hap to be ſeized of that diſeaſe (as
" probably you never may) I ſhould recommend to you, upon the word of a friend,
" the practice mentioned in the 155th page of my book." He then takes notice of ſome accidents, which are incident to that diſeaſe, which he was never maſter of till the ſummer before; and of his own ſucceſs in general. "I have the happineſs,
" adds he, of curing my patients; at leaſt of having it ſaid concerning me, that few
" miſcarry under me; but cannot brag of my correſpondency with ſome others of my
" faculty, who, notwithſtanding my profoundneſs in palmiſtry and chemiſtry, im-
" peach me of great inſufficiency, as I ſhall likewiſe do my taylor, when he makes
" my doublet like a hop-ſack, and not before, let him adhere to what hypotheſis he
" will. Though yet in taking fire at my attempts to reduce practice to a greater
" eaſineſs and plainneſs, and in the mean time letting the mountebank at *Charing-*
" *Croſs* paſs unrailed at, they contradict themſelves, and would make the world be-
" lieve I may prove more conſiderable than they would have me. But to let theſe
" men alone to their books, I have again taken breath, and am purſuing my deſign
" of *ſpecifics*, which, if but a deluſion, ſo cloſely haunts me, that I could not but
" indulge the ſpending of a little money and time at it once more. I have made
" a great progreſs in the thing, and have reaſon to hope not to be diſappointed."
But it does not appear, that this *Deſign of Specifics* was ever executed by Dr. *Sydenham*; at leaſt, it was never communicated to the world.

The Royal Society had from its firſt inſtitution alarmed the zeal of the admirers of the old philoſophy, who affected to repreſent the views of many of its Members to be the deſtruction, not only of true learning, but even of religion itſelf. This gave occaſion to Dr *Sprat*'s *Hiſtory of the Royal Society*, printed in 1667, and to a diſcourſe publiſhed at *London* in 1668, in 8vo, by Mr. *Joſeph Glanvill*, rector of the church of *Bath*, and afterwards chaplain to King *Charles* II. and prebendary of *Worceſter*, under the title of *Plus Ultra: or, the Progreſs and Advancement of Knowledge ſince the days of* Ariſtotle, *in an account of ſome of the moſt remarkable late Improvements of practical uſeful Learning, to encourage philoſophical Endeavours. Occaſioned by a Conference with one of the notional way.* This conference was with Mr. *Robert Croſſe*, vicar of *Great Chew* in *Somerſetſhire*, who maintained, that "*Ariſtotle* had more ad-
" vantages for knowledge than the *Royal Society*, or all the preſent age had, or could
" have, becauſe he did *totam peragrare Aſiam*." Mr. *Glanvill* in his 12th chapter, which treats of *the Royal Society*, ſhews the reaſons of their inſtitution, and their de-

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[k] Vol. VI. p. 648, 649.

[l] *Glanvill*'s *Plus Ultra*, Ch. I.

ſigns; and returns an anſwer to the queſtion, *What have they done?* In the next chapter he gives *an account of what hath been done by the illuſtrious Mr.* Boyle, *for the promotion of uſeful knowledge*; and obſerves [m], that he alone had even then *done enough to oblige all mankind, and to erect an eternal monument to his memory*; *ſo that had this great perſon lived in thoſe days, when men godded their benefactors, he could not have miſſed one of the firſt places among their deified mortals*; and that in his writings are to be found *the greateſt ſtrength and the gentileſt ſmoothneſs, the moſt generous knowledge and the ſweeteſt modeſty, the nobleſt diſcoveries and the ſincereſt relations, the greateſt ſelf-denial and the greateſt love of men, the profoundeſt inſight into philoſophy and nature, and the moſt devout, affectionate ſenſe of God and of religion.* However this piece did not reſtrain Mr. *Henry Stubbe* the phyſician from publickly attacking the Royal Society with prodigious warmth and ſeverity, upon ſeveral chimerical pretences; and particularly, that the Members of it intended to bring contempt upon ancient and ſolid learning, eſpecially the Ariſtotelian philoſophy; to undermine the univerſities; to deſtroy the eſtabliſhed religion, and to introduce Popery in its ſtead. Theſe charges he maintains in various tracts of his, *viz. Legends no Hiſtories: or, a Specimen of ſome Animadverſions upon the* Hiſtory of the Royal Society, *London* 1670, in 4to. *The* Plus Ultra *reduced to a* Non Plus: *or a Specimen of ſome Animadverſions upon the* Plus Ultra *of Mr.* Joſeph Glanvill, *London* 1670, in 4to. *Cenſure of certain paſſages contained in* The Hiſtory of the Royal Society, *&c. Oxford* 1670, in 4to. *Campanella revived; or an Inquiry into* The Hiſtory of the Royal Society, *&c. London* 1670, in 4to. and *Reply unto the Letter written to Mr.* Henry Stubbe, *in a defence of* The Hiſtory of the Royal Society, *&c. Oxford* 1671, in 4to. But amidſt theſe outrages againſt that Society, he kept up a correſpendence with Mr. *Boyle*, who ſhewed a ſingular goodneſs of temper, and an uncommon zeal for the public ſervice, in bearing with ſo much pride, paſſion, and indecent treatment from a perſon, whom he had highly obliged, becauſe he thought him, with all his faults, capable of being uſeful to the world. The following letters will give a clear idea of ſo remarkable a man.

" Honoured Sir,

" I AM much obliged unto you for the trouble you gave yourſelf by imparting " the contents of my letter to the Members of the Royal Society. I wonder " they ſhould imagine this attempt of mine to be an action I was not provoked unto, " whereas Mr. *Glanvill*'s book [n] was enough to juſtify any ſeverity of proceedings, " it being written by a cabal, and not by him alone. And it is much more ſtrange " to me, that they ſhould think, that a man needed particular exaſperations to write " againſt that pernicious *Hiſtory* [o]. There is no man deſires to live more in quiet " than I do; and to purſue my practice without intermeddling with the affairs of the " nation. But if that *Hiſtory* take place, the whole education of this land and all " religion is ſubverted; and I profeſs I am not inclined ſilently to expect, till fire, " faggots, and exile be the common conſequences of being a proteſtant. I know " this great change was no part of the intentions of the King, and ſuch honourable " and pious perſons, as firſt aſſociated; but that the hiſtorian exactly complies with " *Campanella*, is evident; and it is in vain for you not to meddle with religion amongſt " yourſelves, if you thus overthrow it in your writings. Upon this account I ſevered " the caſe of your comical wits from that of the other illuſtrious perſonages; nor is " there any thing in my book, that reflects on the King or any perſon of learning or quality. I gave the licenſer leave to blot out every thing, that he thought might give juſt offence; and that he did too much, even to the diſturbing of the ſenſe, practiſing the freedom I gave him ſo far, that I am forced to add a Review, thereby to illuſtrate ſome paſſages, which he had, I know not how, nor upon what reaſon, curtailed. It is certain and manifeſt, that the *Hiſtory* muſt be written anew; and I wonder, that any ſhould ſcruple my amendments of it, whereof all that comes out now is but a ſpecimen; for I have much more behind.

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[m] P. 92, 93.

[n] *Plus Ultra.*

[o] *Sprat*'s Hiſtory of the Royal Society.

“As for Mr. *Glanvill*, it is much, that the Society ſhould now appoint him their ſecretary (I have not yet ſeen the new model of correſpondence) ſeeing that he is a man, whom the univerſities hate and ſcorn, and that hath ſo notoriouſly diſobliged the phyſicians, and whoſe credit is now irrecoverably loſt; for if my papers ever come out, it will be in vain for any man to go about to excuſe him from the moſt groſs ignorance, that ever any writer committed; and this action looks as if the Society would own him as a man of worth. Beſides his *Plus Ultra*, his Letter againſt *Ariſtotle* is all miſtakes; and it is evident he never read *Laertius*. My reply to that Letter will be printed at *Oxford*, I hope before the end of the next term; but the publiſhing of that depends upon this; not that they ſcruple at it, being earneſt for it, but that I would keep the buſineſs alive by printing one book after another.

“At the ſeſſion of parliament you will ſee a *Cenſure* upon certain paſſages in the *Hiſtory*, with a defence of the *Cenſure* approved by as eminent divines as this nation yields; which will convince you, how juſtly I ſay, that the *Hiſtory* ſubverts the proteſtant religion and church of *England*. I never did any thing more unwillingly than to enter upon divinity, though now the thirty-nine articles, and homilies, and the proteſtant religion, be the ſubject I defend againſt Popery and Socinianiſm. I underſtood of the King's diſpleaſure at me, and that he ſaid ſome ſevere things; but I was not troubled at it much, being content rather to ſerve him, than to flatter him. I did expect to have been impriſoned, when I firſt undertook it, and therefore contrived, how I might be freed again, never purpoſing to decline the other trouble. But when I bring the caſe before the Commons, I am ſure I ſhall here be powerfully abetted, and the Society will have the worſe, and whatever they do before, will add to my plea then. You will ſee before Twelfth-day, that no inconſiderable perſons will ſpeak favourably of my intentions, and avow the bravery and neceſſity of the performance. But I determined not to engage them to diſcourſe their minds before the book came out.

“Beſides that book of *Vander Mye*, there is Fonſeca *de ſanitate tuenda*, which I ſent to let you ſee, what maſtic wood will do in the gout. I aſſure you, it is a wood I have tried in bodies apt to catarrhs, and it doth not only work powerfully in drying them, but purges twice or thrice in a day, as I found particularly in Mr. *Garnet* (that married the lady *Boſwell*) lately, to his great recovery out of a malignant fever, which ended with a perfect ſalivation of three weeks, and after that endangered him of a conſumption. But he was perfectly well and fat again in a few weeks. It is not ill taſted, nor hot, but after it is ſwallowed a while, it creates in the throat ſuch a ſenſe, as remains after drinking pepper-poſſet.

“If there be any thing, wherein I may ſerve you, or any other books you need, you may command them. Had not the Society thus gone about to injure the phyſicians, I had gone on with my collection of Engliſh receipts, illuſtrating every experiment out of the experiments of the greateſt phyſicians. But in this empirical ignorant age ſuch works are not to be publiſhed.

“I suppose you may have ſeen two pieces of *Tachenius*: there are in them many experiments that are pretty, diſcovering the diſcrepancy of ſalts (a thing I have made ſome inquiries into) and that gold is acid; which agrees with a way I learned to make cheeſe and whey with gold colour, which was tried in a conſumption by a lady of my acquaintance.

“I have no more to add, but that I am glad the houſe diſmiſſed that fooliſh impeachment of my lord *Orrery*. I am,

“Sir,

Warwick, Decemb. 17, 1669.

“your obliged humble ſervant,

“Henry Stubbe.”

“I had forgot to ſuggeſt one thing to the conſideration of your Society, that perhaps it may be manifeſted at the parliament, that the hiſtorian by his book is chargeable with high-treaſon; and that there want not ſuch as think, if your Society own it, they incur a præmunire; ſo that whatever they may imagine to draw upon me (though I may ſuffer a while) will fall worſe on them, when the world takes notice of the ground of their actings againſt me, and that they do all this, whom I had repreſented in my preface as unconcerned in the conteſt.”

Mr. *Stubbe* wrote likewiſe another letter to him from *Warwick* on the 18th of May, 1670, which was as follows:

“Sir,

“I never doubted, but that Mr. *Boyle* would never ſwerve from the rules of honour and ſtrict virtue, whatever the other virtuoſi might do. You are ſtill conſtant to yourſelf and worth; but ſo are not they. That you might hear ſeveral perſons of quality incenſe the king againſt me, and that his majeſty let fall ſome paſſages that were ſevere, it is not ſtrange to me: I had an account thereof immediately from many hands. I know the opportunities my enemies have, and under how falſe repreſentations princes diſcern things, who ſee but only with others eyes, and glaſſes obtruded by the virtuoſi. I am ſatisfied in the juſtneſs of my attempt, and the integrity of my deſigns and ſentiments. Had intereſt moved me, and either deſire of glory, or any other vulgar inducement moved me, to have flattered the Royal Society, to have admired the preſent condition of our nation, to have betrayed religion, had been my ſpeedy courſe to effect all my deſigns. But I needed not any new courſes to be known to the world, or in this country to advance my practice; and the firſt thoughts I aſſumed after the deſign of oppoſing the Royal Society, were to deſert the land, to encounter all manner of cenſures and impriſonment, perhaps death; and amidſt theſe perplexities I did reſolve to proceed, if that they would be ſo indiſcrete, as not to diſclaim thoſe two books. Nor have I yet received any intelligence, that propoſeth to me any new and unforeſeen danger, and which a man, who is weary to have out-lived the proſperity, honour, learning of the nation, and who hath no other proſpect than the apparent ruin of his country, would not willingly ſink under. Yet I find amidſt theſe contraſts, that moſt ſober perſons commend my deſign, and that my performances were neceſſary, and not mean; and I dare ſay, that except the miſconſtructions, which my prince puts on my doings, my abettors are more numerous, judicious, and noble than my enemies. But the conduct of my fate I leave to that power, which irreſiſtibly put me upon theſe actings, and permitted me no reſt but in purſuing them. Whatever befalls me, the world will ſay it was malice in the virtuoſi, now that the quarrel is ſo boldly begun by me. If you conſider the writing of *Glanvill*, you cannot ſay, that I proceed in too exaſperating a way; I did but write by his copy; nor had I equalled him but by out-doing him. Beſides it is *reſponſum, non dictum, quia læſit prior*. And though your hiſtorian be the more civil in words, yet is his ſenſe parallel to that of *Glanvill*, as I have ſhewed in my dedication to both univerſities in my *Animadverſions* on the *Hiſtory*. And what reaſon the Royal Society had to deal ſo with the phyſicians, if it be true, that they had ſuch an eſteem for them, and that ſo many eminent perſons joined with them, I know not, except I accommodate all to a ſubſerviency unto *Campanella*; and then it is eaſy to divine. That the biſhop of *Ch.*[p] had made collections in order to the writing of my life, is a thing I am aſſured of from one, who gave me an account of the tendency of the papers, and of ſome particulars of them; as alſo of a letter of *Glanvill*'s, which was a piece of raillery, and paſſed through ſome hands, containing nothing of truth, but like to him. They that put the ſtreſs of their defence on ſuch foundations, are welcome adverſaries; yet ſhall I ſay no more to them, than what I wrote a year ago, when I was firſt threatened with theſe actions, and which is like to be public in my preface to the *Animadverſions* on the *Hiſtory*. The diſingenuity with which I have been treated, admits of no apology; nor can my lord *Brounker* ever acquit himſelf unto the world, for having bragged and told the earl of *Ayleſbury*, that he would licenſe it, if I would ſend it unto him, and yet declined it. It is very falſe printed, nor is the catalogue perfect as to thoſe material. I did appoint ſome to be corrected with a pen; but orders at a diſtance are ill obſerved; and I durſt not come up to ſee it done, for fear of ſome mechanical philoſophers and their ſtratagems.

“I DO not yet underſtand by any hands but yours, that there is ſo great a correſpondence betwixt the College of Phyſicians and Royal Society. That ſundry of thoſe few, that were entered in it, have deſerted and deteſted it, I am ſure; and ſo Dr. *Scarborough* and others inform me. Nor do I hear of four intelligent and eminent phyſicians, that heartily adhere to the Society; and it is evident, that they betray their faculty, who do it. But this point I have largely handled in my fourth part, where Dr. *Merret* is replied unto; which piece I do intend not to ſuffer to be publiſhed till Michaelmas. As to thoſe paſſages, wherein you ſeem concerned in this piece, I did not think fitting to give you a particular account, becauſe you were not directly concerned; for in the aërial column, and that each little alteration of the air may be diſcovered by the barometer, I only oppoſe *Glanvill*, who ſays it is *concluded* to be ſo; not you, who make no ſuch poſitive concluſion. As to the mixtures of liquors with blood, and their operating differently on the blood in a pottinger, and when injected into the veins, or aſſumed by the mouth, I did give you notice long ago thereof, and that I did not think your argument for Sp. C. C. to be concluſive. But there is, I find, a miſtake in what is printed, where it is printed, as if I held the opinion, that ſuch mixtures, when injected, and when made in a pottinger, were the ſame; whereas I deny it totally, and my page 129

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[p] *Cheſter*, Dr. *John Wilkins*.

“ contradicts itſelf and the ſubſequent diſcourſe, if it be taken ſo, as I hear you “ did. I think I gave you an account of ſome *Errata* in my laſt. I did not think “ I needed to mention, that in the *Animadverſions* on the *Hiſtory* I have laughed at “ him, that told you for an arcanum the uſe of ivy-berries; and I have called into “ ſuſpicion the redintegration of nitre, it being manifeſt, I think, that you did not “ uſe the beſt nitre in your calcination. Indeed I think I have there given a great “ inſight into the nature of ſalt-petre; yet could have added more, had I deſigned “ an hiſtory, and not mere *Animadverſions*. Beſides when that was written, I did not “ intend, unleſs forced unto it, to extend my papers beyond ten or twelve ſheets. “ But if any great difference appear betwixt you and me, it will be about the ſecond “ part of your *Uſefulneſs in Experiments for Phyſic*; in which book I think we phyſicians “ do ſuffer much; and that book hath been the occaſion of all the inſolencies “ we have received from *Dowde* to *Glanvill* and *Thompſon*; wherein yet your candour “ would unconcern you, but that others make too much uſe of it to our prejudice; “ and our defence is very facile, as I have briefly ſhewed againſt Dr. *Merret*. It “ was a great unhappineſs, that ſeveral perſons of honour like yourſelf ſhould ever “ mix with ſuch inſignificant talkers, as the generality are; for you could get no “ credit by them, and their arrogance and folly would unavoidably run you all into “ quarrels, if not contempt. And it is apparent, that the Royal Society hath not “ managed things with that diſcretion, which was neceſſary; and being informed of “ their errors, would rather be obſtinate than amend them.

“ As to my transferring the glory of ſome inventions from my countrymen to “ others, I am ſure I have injured no man thereby; for to deprive any man of undue “ praiſes is no injury; for if I had ſaid, that *Picquet* did ſuppoſe the air to be like “ flocks of wool, I had ſaid true; yet I did not, but I ſhall do it e'er long, but my “ reſpects to you hindered me. And I do ſuſpect, that the biſhop of *London*'s chaplains, “ and ſome of the Society, in the diſpunging did join againſt me, or elſe ſome “ others had been charged for plagiaries, or men of no reading, who have eſcaped I “ know not how. But I do think no man was ever uſed as I have been; and it is the “ common concern of mankind, that ſuch courſes be not continued.

“ I HAVE no more to add, but that I ſhall willingly allow many of the Society the “ facility of improving paſt inventions; but I will not ſay, contrary to my knowledge, “ that they did invent them, except they will on the other ſide grant unto me, that “ they are men of no reading, and underſtand no more what hath been printed in “ *Europe*, than what was invented in *China* before our commerce with the *Eaſt-* “ *Indies*.

“ THIS is all, beſides thoſe reſpects, which I ſhall ever pay unto you upon many “ ſcores, and which are ſuch, that except where the common utility of *England* “ forceth me to the contrary, I ſhall ever be

“ Your moſt humble ſervant,

“ HEN. STUBBE.”

I FIND likewiſe a third letter of his dated the 4th of June following:

“SIR,

I RECEIVED yours, as alſo the two books, and I cannot but begin my anſwer with expreſſing my ſorrow to find, that you ſtill adhere to the R. S. Except yourſelf, I do not hear of any ſober perſon, but approves of my deſign againſt the Society, and they that diſſuaded me from it, thought the attempt neceſſary, not dangerous. I beſeech you, Sir, conſider the miſchiefs it hath occaſioned in this once flouriſhing kingdom; and if you have any ſenſe, not only of the glory and religion, but even the being of your native country, abandon that conſtitution. It is too much, that you contributed to its advancement and repute; the only reparation you can make for that fatal error, is to deſert it betimes. Do not you apprehend, that all the inconveniencies that have befallen the land, all the debauchery of the gentry (which ariſeth from that pious and prudent breeding, which was and ought to have been continued) will be charged on your account? Do you not hear, how much the ſerious and ſober men do blame you? You are much a ſtranger to the diſcourſe and ſentiments of men, if you know not, that your name is frequently queſtioned, and that the integrity of Mr. *Boyle* is diſputed, as if you in the council of trade, and by ſome other engagements, had ſhewed yourſelf leſs a patriot and public ſpirited than was imagined. It will be impoſſible for you to preſerve your eſteem, but by a ſeaſonable relinquiſhing of theſe impertinents. I tender you this teſtimony of my ſincere reſpects, that I am thus free with you; and whilſt others flatter you, there is an old ſervant of yours informs you of the truth. What I believe you could never have apprehended, is come to paſs; the body of the nation, and all learned men abet me: the two univerſities (eſpecially that of *Cambridge*, which was moſt inclined to novelty) avow my quarrel; and they that favoured experimental philoſophy, judge thoſe virtuoſi unfit to proſecute it. The biſhop of *Chicheſter* reads againſt them, and intends to hold his lecture a year or two, and that for this reaſon, to overthrow the eſteem of them. The concurrence to hear him is ſuch, as the univerſity never ſaw before. The elect of *Bath* is as much as any for me, and againſt *Glanvill.* In ſum, all men apprehend it now their common intereſt to oppoſe the R. S. I know not what any phyſicians may, as the mode is, tell you to your face; but except it be ſuch as Dr. *Sydenham*, and young *Coxe*, I believe not one lives, that doth not condemn your experimental philoſophy; and either the college, or Sir *Alexander Fraſer*, and the principal of them intend me a letter of thanks for what I have done, with the liberty to print it; and perhaps others will follow the precedent; and how conſiſtent theſe things are with the repute of the Society you adhere unto, judge you. . . . The biſhop of *London* hath licenſed that againſt *Sprat* once more, and approved the preface and dedication to both univerſities, wherein the paſſages are more ſevere than any yet publiſhed . . . being reſolved to keep the controverſy hot, till the R. S. ſubmit to the terms I propoſe, and then they had as good diſſolve. I acquaint you with theſe things, that you may conſult your own eaſe, and let theſe comediants ſtand or fall alone; and then whatever reply I ſhall at the requeſt of the phyſicians at *London* make to you, ſhall be as amicable as it is poſſible. And if it pleaſe God to continue us a nation, perhaps you ſhall ſee a nobler project for the advancement of uſeful knowledge in reference to phyſic, than ever your Society was capable of, and which ſhall add glory to our faculty. It was indeed the

" popiſh intereſt to depreſs the phyſicians, that ſo there might be nothing of learn-
" ing in the nation to oppreſs them. Civil law is gone, the number of learned di-
" vines is ſmall, and your R. S. hath deſtroyed the ſucceſſion. But there remains
" enough to retort the ſhame upon the authors, and cover you all with ignominy. I
" have no more to add, but that I ſhall never quit that reſpect, which becomes

" Your moſt humble ſervant,

Ragley, June 4, 1670.

" HEN. STUBBE."

SUCH was the impetuous genius of this learned man, of whom, as I ſhall have no other occaſion to mention him again, I ſhall add here this ſhort account, that he was ſon of a miniſter, and born at *Partney* near *Spilſbye* in *Lincolnſhire* on the 28th of February 1631, and at ten years of age ſent to *Weſtminſter* ſchool under the care of Dr. *Richard Buſby*, where by the intereſt of Sir *Henry Vane*, jun. he became one of the king's ſcholars, and in 1649 was elected ſtudent of *Chriſt Church* in *Oxford*[q]. In July 1653 he took the degree of Bachelor of Arts[r], and then went to *Scotland*, where he ſerved in the army till 1655; and in December 1656 took the degree of Maſter of Arts. In the beginning of the year following he was appointed ſecond library-keeper of the *Bodleian* library under Dr. *Tho. Barlow*, which poſt he held till the year 1659, when he was removed from it, as well as from his place of ſtudent of *Chriſt Church*, having the ſame year publiſhed a *Vindication* of his patron Sir *Henry Vane*, *An Eſſay on the good old Cauſe*, and a piece entitled, *Light ſhining out of Darkneſs*, with *an Apology for the Quakers*, in which he reflected upon the clergy and the univerſities. He retired then to *Stratford* upon *Avon*, in order to practiſe phyſic there, and after the reſtoration joined himſelf to the church of *England*, *not only*, ſays he in the dedication to biſhop *Morley* of his tranſlation of *Caſa*'s *Arts of Grandeur and Submiſſion*, *upon account of its being publickly impoſed (which in things indifferent is no ſmall conſideration, as I learned from the Scottiſh Tranſactions at* Perth) *but becauſe it is the leaſt defining, and conſequently the moſt comprehenſive and fitting to be national.* In 1661 he went to *Jamaica*, being honoured with the title of his majeſty's phyſician for that iſland; but that climate not agreeing with his conſtitution, he returned to *England*, and at laſt ſettled in *Warwick*, where he gained very conſiderable practice, as likewiſe at the *Bath*, which he frequented in the ſummer-ſeaſon; but was unfortunately drowned in a river about two miles from that city in a journey to *Briſtol* on the 12th of July 1676, and was interred in the great church at *Bath*, his old antagoniſt Mr. *Glanvill* preaching his funeral ſermon.

BUT to return to the year 1669, in which Mr. *Boyle* gave the world his *Continuation of new Expriments phyſico-mechanical touching the Spring and Weight of the Air and their Effects. The firſt Part. Written by way of Letter to the right honourable the lord* Clifford *and* Dungarvan. *Whereto is annexed a ſhort Diſcourſe of the Atmoſpheres of conſiſtent Bodies*; *ſhewing, that even hard and ſolid Bodies (and ſome ſuch, as one would ſcarce ſuſpect) are capable of emitting Effluvia, and ſo of having Atmoſpheres*: Oxford, in 4to *A Diſcourſe of abſolute reſt in Bodies*; added to a ſecond edition of his *Certain phyſiological Eſſays*, London, in 4to. *An Invention to eſtimate the Weight of Water in Water with ordinary Balances and Weights*; printed in the *Philoſophical Tranſactions*,

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[q] Wood, Athen. Oxon. Vol. II.

[r] Col. Faſti Oxon. Vol. II.

N°. L. p. 1001. a tranſlation of which into Latin is annexed to the Latin edition of his *Hydroſtatic Paradoxes*; and a dilucidation of the experiment in an *Hydroſtatic Letter* printed among his tracts in 1672, in 8vo. He wrote alſo the ſame year *a Letter to Dr.* Peter du Moulin, prefixed to the 4th edition of a book, tranſlated by the doctor from French into Engliſh, and printed at *Oxford* 1669, in 12mo, under the title of, *The Devil of* Maſcon: *or, a true Relation of the chief things, which an unclean Spirit did and ſaid at* Maſcon *in* Burgundy, *in the houſe of one Mr.* Francis Pereaud, *miniſter of the reformed church in the ſame town. Publiſhed in French lately by himſelf, and now made Engliſh by one that hath a particular knowledge of the truth of the ſtory.* It was afterwards indeed reported, that Mr. *Boyle* had diſowned the hiſtory of that dæmon, as a clear impoſture; but he declares, that he never did this, in a letter to Mr. *Joſeph Glanvill*, dated at *London* Feb. 10, 1677-8, wherein he has theſe words: “Whatever I may have thought of ſome incidental paſſages in the printed “book, yet as to the main ſtory itſelf, the converſation I had with the author, when “the thing was freſh in men's memories, and the notoriety of divers of the circum- “ſtances, ſurmounted my indiſpoſition to believe ſuch things; and of late years I re- “member not, that I have had occaſion to diſcourſe much of it, much leſs to diſown it, “eſpecially after a learned and intelligent traveller, that ſome years ago paſſed by “that town, and afterwards viſited me, informed me, that he found upon the place “itſelf juſt motives to believe the truth of it.” Dr. *du Moulin*, the tranſlator of the piece laſt mentioned, was ſon of *Peter du Moulin*, the celebrated French proteſtant divine, and had been tutor to the lord viſcount *Dungarvan*, and Mr. *Richard Boyle* his brother, nephews to Mr. *Boyle*; and after the reſtoration was preferred to be chaplain to the king, and prebendary of *Canterbury*. He had an uncommon genius for Latin poetry; and having collected his performances in that kind, he publiſhed them with a dedication to Mr. *Boyle* at *Cambridge* 1670, in 8vo, under the title of *Petri Molinæi P. F.* ΠΑΡΕΡΓΑ. *Poematum Libelli tres.* I. *Hymni in Symbolum Apoſtolorum.* II. *Eccleſiæ Gemitus.* III. *Sylva Variorum.* In the dedication the doctor highly commends Mr. *Boyle*'s excellent talents in verſe, and his particular reliſh for that kind of poetry, which is devoted to religion. *Cum vero nulla literaturæ pars extra eruditionis tuæ pomœrium poſſit evagari, patere, quæſo, etiam huic poetico labori liberum aperiri apud te angulum. Cui ſanè poſtulationi ut te facilem fore mihimet ſpondeam, tria ſunt. Unum eſt, quod ſeveriora ſtudia poetico melle delenire tibi non eſt inſolens, & in carmine ita excellis, ut niſi excelſiores in te dotes eminerent, ad id unum natus poſſis exiſtimari. Alterum eſt, quod horum poematum nonnulla jamdudum in manibus habuiſti, approbâſti, & cum iis familiaritatem inire non aſpernatus es. Potiſſimum eſt, quod cum ingenii & eruditionis tuæ ſublimitatem immenſum quantum excedat tua erga Deum pietas, & in capaciſſimâ mentis arce regina ſedeat, non poterunt tibi non placere piæ ad Deum aſpirationes, quas tutemet poetico ſpiritu altiùs atque feliciùs attolli non ſemel apud me profeſſus, expertus apud expertum; adjectis hortamentis, quæ mihi ſunt mandatorum loco, ut iſtam aream ſeriò tererem.* The doctor in a letter to Mr. *Boyle*, dated at *Canterbury* December 28, 1669 [a], takes notice of one circumſtance, which deſerves to be mentioned, as it ſhews how far the prejudices againſt the Royal Society had poſſeſſed the univerſities at that time; for Dr. *Peter Gunning*, afterwards biſhop of *Ely*, *one of the prime licenſers of the book*, would not ſuffer a copy of heroic verſes, which Dr. *du Moulin* had written in commendation of that Society, to be publiſhed with the reſt of his poems. “It grieves me (adds he, to ſee a feud between that noble Society and the univer-

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[a] See Vol. VI. p. 579.

" ſities, to which Mr. *Glanvill's* books have much contributed. Your great credit, " prudence, and moderation may ſtop that growing evil, if any thing in the world " can."

In 1670, Mr. *Boyle* publiſhed his *Tracts about the coſmical qualities of things*; *coſmical ſuſpicions*; *the temperature of the ſubterraneal regions*; *the temperature of the ſubmarine regions*; *the bottom of the ſea*; to which is prefixed, *an introduction to the hiſtory of particular qualities:* printed at *Oxford*, in 8vo, and tranſlated into Latin. In the tract concerning the *coſmical qualities of things*, he ſhews, 1. That they depend partly upon the influence of external agents, as well as the primary affections of matter: ſo that there are many bodies, which in ſeveral caſes act not, unleſs acted upon, and ſome of them act either ſolely or chiefly, as they are acted on by general and unregarded agents. 2. That there are certain ſubtile bodies in the world, which are ready to inſinuate themſelves into the pores of any body diſpoſed to admit their action, or by ſome other way to effect it; eſpecially if they have the concurrence of other unobſerved cauſes, and the eſtabliſhed laws of the univerſe. 3. That a body, by a mechanical change of the texture, may acquire or loſe a fitneſs to be wrought upon by unheeded agents, and alſo to diverſify their operations on it, by a variation of its texture. He propoſes next the following *coſmical ſuſpicions*, 1. That beſides thoſe more numerous and uniform ſorts of minute corpuſcles, which are by ſome of the modern philoſophers thought to compoſe the æther, there may poſſibly be ſome other kind of corpuſcles fitted to produce conſiderable effects, when they find bodies to be wrought on by them. 2. That ſeveral perſons have diſcovered peſtilential ſteams in the air, before they acted as ſuch upon other bodies. 3. That conſiderable, though ſlow, changes in the internal parts of the earth may occaſion a variation of the mariner's needle. 4. That the ebbing and flowing of the ſea, and ſuch like phænomena, are occaſioned by ſome *coſmical* law of nature; or that the planetary vortex may be not a little concerned in producing ſuch effects. 5. That all endemical and epidemical diſtempers principally depend upon the influence of thoſe globes, which move about us, and the terreſtrial effluvia of our own globe. 6. That thoſe which are thought the grand rules, by which things corporeal are tranſacted, and which ſuppoſe the conſtancy of the preſent fabric of the world, and a regular courſe of things, are not altogether ſo uniformly complied with, as we preſume; at laſt, as to the lines, according to which the great mundane bodies move, and the boundaries of their motions. 7. That on the other hand we may perhaps take ſuch things for exorbitances and deviations from the ſettled courſe of nature, as, if long and attentively obſerved, will be periodical phænomena of very long intervals; but becauſe men have not ſufficient ſkill and curioſity to obſerve them, nor a life long enough to be able to take notice of a competent number of them, they readily conclude them to be but accidental extravagancies, which ſpring not from any ſettled and durable cauſes. In the diſcourſes concerning *the temperature of the ſubterraneal and ſubmarine regions*, he obſerves, that there are different regions below the earth; that the firſt region of the earth is very variable both as to bounds and temperature; that the ſecond ſeems to be generally cold in compariſon of the other two; that in ſeveral places, which by reaſon of their diſtance from the ſurface of the earth might be referred to the middle region of it, the temperature of the air is very different at the ſame ſeaſons of different years; that the third region of the earth has been obſerved to be conſtantly and ſenſibly, but not uniformly warm, being in ſome places conſiderably hot; that there are two different regions below the ſurface of the ſea, the one extended from the ſuperficies of the water as far downward, as the manifeſt operation of the ſun-beams or other cauſes penetrates, and the other from thence to the bottom; ſo that the upper region muſt vary as to its extent, according to the difference of the climate and the heat of the ſun, or the nature of the ſoils about the ſhore; but that the lower region is generally cold: he farther tells us, that the bottom of the ſea is very rough and unequal; that the water gravitates conſiderably upon bodies immerſed in it; and that the bottom of the ſea is not diſturbed with ſtorms, but that the water ſtagnates.

There were publiſhed alſo *the ſame year* in the *Philoſophical Tranſactions* two other pieces of his, viz. *New Pneumatical experiments about reſpiration*; No. LXII. and *A continuation* of the ſame *experiments*; No. LXIII.

AMIDST all theſe labours for the public he had been afflicted with a ſevere paralytic diſtemper; but the regimen which he made uſe of with ſucceſs for his relief, will appear from his letter to his friend *John Mallet*, Eſq;

" *London*, May 23, 1671.

" SIR,

" I AM much obliged to you for the favour of haſtening to me ſuch welcome " news, as that of my brother *Orrery*'s being already put to ſea with ſo fair a " wind, as promiſed he would get quickly to the other ſide of it. Since Monday, " when I received the favour of your letter, there came to my hands a deſire from " him relating to your kinſman, Mr. *Pitt*, which was very needleſs; the relation, that " gentleman has to you, and the concern you expreſs for him, being more than " ſufficient to make me forward to do him all the ſervice I am able. But this, I " fear, will not be very much; for though, through the goodneſs of God, my paralytical diſtemper is much leſſened, yet I am far from being fully cured of it; " and during the ſpace of an eleven months paſt ſince it firſt invaded me, I have " taken ſo many medicines, and found the relief they afforded me ſo very ſlow, that " it is not eaſy for me to tell you what I found moſt good by. The things, which to " me ſeem the fitteſt to be mentioned on this occaſion, are, that cordial medicines, " eſpecially ſuch as peculiarly befriend the *genus nervoſum*, were very frequently and " not unuſefully adminiſtered. That I uſed during this ſickneſs leſs purging phyſic, " and that gentle, than in many years before; and found cauſe to think ſuch evacuations very weakening, and, when they are not very neceſſary, dangerous. That the " dried fleſh of vipers ſeemed to be one of the uſefulleſt cordials I took; but then I " perſevered in taking it daily for a great while. That I ſeldom miſſed a day without taking the air, at leaſt once, and that even when I was at the weakeſt, and was " fain to be carried in men's arms from my chair into the coach. That the beſt thing " I found to ſtrengthen my feet and legs, and which I ſtill uſe, was ſack turned to a " brine with ſea-ſalt, and well rubbed upon the parts every morning and night with " a warm hand. That for my hands I uſe ſeveral things, and particularly palm oil, " which comes from *Guinea*, and a liquor ſomewhat like the *ſpiritus lavendulæ compoſ.* " of the diſpenſatory, and alſo fomentations with cephalic flowers and herbs, one " or other of which I yet daily continue. But yet I found nothing ſo available as " frequent exerciſe of my hands and feet, in which I continued as far as my ſtrength " would poſſibly allow me, getting ſometimes others to bend the joints of my arms and " hands for me. And though this courſe makes me every day ſore and weary, yet I " continue to undergo it, becauſe I think I find more benefit by it alone, than by " all the outward applications of phyſicians and chirurgeons. Theſe, Sir, are in

“ brief the chief things that I have obſerved about my own diſtemper; but how far they will be applicable to that of your friend, I do not know half ſo well, as that in caſe he uſe them, they will prove very effectual, if any thing can be added to their virtue by the good wiſhes of,

“ SIR,

“ Your moſt humble and affectionate ſervant,

“ R. B.”

THE ſame year he publiſhed *Some Conſiderations touching the uſefulneſs of experimental and natural Philoſophy, propoſed in a familiar Diſcourſe to a Friend, by way of invitation to the ſtudy of it. The ſecond tome, containing the latter ſection of the ſecond part:* printed at *Oxford* in 4to. and *Tracts: Of a Diſcovery of the admirable Rarefaction of the Air: New Obſervations about the Duration of the Spring of the Air: New Experiments touching the Condenſation of the Air by mere Cold, and its Compreſſion without mechanical Engines: The admirable differing Extenſion of the ſame Quantity of Air rarified and compreſſed: London,* in 4to; a few copies of which *Tracts* were reprinted in 1739, but with the former date, only the *errata* in the firſt edition were corrected in it. He communicated likewiſe in the *Philoſophical Tranſactions*, N° LXXIV. p. 2216. *An Obſervation of a Spot in the Sun, April 27, 1660.*

HIS next work was *An Eſſay about the Origin and Virtue of Gems; wherein are propoſed and hiſtorically illuſtrated ſome Conjectures about the Conſiſtence of the Matter of Precious Stones, and the ſubjects, wherein their chiefeſt Virtues reſide:* printed at *London*, 1672, in 8vo, and tranſlated into Latin. In this *Eſſay* he informs us, that gems were once fluid, and have their virtues from mineral matter; which he ſhews from their tranſparency, figuration, internal texture, their colours being probably adventitious, heterogeneous matter having been found in their ſubſtance, and metalline or mineral mixtures mixed with their ſmall parts. He obſerves further, that a greater ſpecific gravity in gems argues their metalline or mineral nature, and that metals or minerals may be extracted from them. He tells us, that mineral productions are exceedingly numerous in the bowels of the earth, and that various menſtrua are likewiſe to be found there; and ſhews the formation of gems from a petrifying juice or ſpirit mixing in a ſufficient proportion with the impregnated waters in the earth, ſo as to coagulate and unite with them. That ſome of their real virtues may be derived from hence, that whilſt they were in a fluid form, the petreſcent ſubſtance was mixed with mineral ſolution, tincture, or other impregnated liquor; and that theſe were afterwards coagulated or united, and hardened into a gem.

The ſame year he publiſhed his *Tracts, containing, New Experiments touching the Relation between Flame and Air; and about Exploſion: An hydroſtatical Diſcourſe, occaſioned by ſome Objections of Dr.* Henry More *againſt ſome Explications of new Experiments made by the Author of theſe Tracts. To which is annexed an hydroſtatical Letter, dilucidating an Experiment about a Way of weighing Water in Water: New Experiments of the poſitive or relative Levity of Bodies under Water: About the differing Preſſure of heavy Solids and Fluids: London,* in 8vo. *Some Obſervations about ſhining Fleſh, both of Veal and of Pullet, and that without any ſenſible putrefaction in thoſe bodies:* printed in the *Philoſ. Tranſact.* N° LXXXIX. p. 5108. and *A new Experiment concerning the Effect of the varying Weight of the Atmoſphere upon ſome Bodies in the Water; and ſuggeſting a conjecture, that the very Alteration of the Air in point of Weight may have conſiderable Operations even upon men's ſickneſs or health: Philoſophical Tranſactions*, N° XCI. p. 5156.

THE year 1673 produced to the world his *Eſſays of the ſtrange Subtilty, great Efficacy, determinate Nature of Effluviums. To which are annexed New Experiments to make Fire and Flame ſtable and ponderable; with additional Experiments about arreſting and weighing of igneous Corpuſcles; together with a Diſcovery of the Perviouſneſs of Glaſs to ponderable Parts of Flame:* printed at *London* in 8vo, and tranſlated into Latin: and *A Letter concerning Ambergreaſe, and its being a vegetable production: Philoſ. Tranſact.* N° XCVII. p. 6113.

HE wrote likewiſe, in December that year, the following letter to a gentleman in the *Levant*, whom he deſired to tranſmit to him ſuch obſervations as he thought proper, concerning thoſe countries, and to whom he now ſent ſeveral copies of Dr. *Edward Pococke*'s Arabic tranſlation of *Grotius*'s book *De Veritate Religionis Chriſtianæ*, as a means of propagating Chriſtianity there.

" SIR, *London*, Decem. 10, 1673.

" I WERE as unjuſt to you, as unkind to myſelf, if I ſhould at all deceive you " in the expectation you had, that your letter would not be thought by me, either " to need an apology, or be an impertinence. That liberty of writing to me, which " you out of modeſty are pleaſed to ſtile a privilege, is that, whoſe effects I ſhall " always look upon as favours; and the greater, if you give yourſelf the trouble, " and me the ſatisfaction, to let them convey to me what you call the ſtories and " obſervations of the country you are in; for it cannot but be welcome as well as " inſtructive to a perſon, that does as much deſire as want good obſervations about " the *Levant*, to receive accounts of ſuch remote places and things from ſo ſure a " hand as yours.

" I HOPE I ſhall not allow myſelf the vanity of receiving the compliments you " make me in reference to *Grotius*, but as effects of your civility and charity; what " I have been able to do, having been but to hand over to others thoſe lamps, which " that excellent perſon framed and lighted. But I ſhall confeſs to you, that I have " a great deal of ſatisfaction to find ground to hope, by what you tell me, that my " poor endeavours to promote the Goſpel in thoſe parts, are not like to prove alto- " gether fruitleſs. And though they were firſt and chiefly deſigned for the conver- " ſion of infidels (to which end I hope you will in due time attempt to make them " ſerviceable) yet I muſt not think them miſemployed, if they be bleſſed to inform or " confirm the poor and ill-inſtructed Chriſtians in thoſe parts, eſpecially ſince the " ſame arguments that work on them, may, through his bleſſing, for whoſe truth " they militate, enable them in time to work on their Mahometan countrymen. I " have therefore by the merchant, to whom you directed me, ſent you three dozen " more of thoſe Arabic books, bound as the others were, being not ſolicitous to " exceed that number now, both becauſe I had but ſhort warning given me of the " departure of the ſhips, and becauſe of the danger of the ſeas; but I hope here- " after to ſend a further ſupply; and in the mean time muſt return you my humble " thanks, both for your zeal and concern in a work, that aims at the glory of our "common maſter, and for thoſe obliging prayers, the continuation of which is "earneſtly begged of you by,

"SIR,

"Your moſt affectionate friend and very humble ſervant,

"R. B.

"A SMALL treatiſe of mine being lately ventured abroad, I preſume the reliſh you "formerly had for philoſophical ſtudies, may keep that from being altogether unac-"ceptable to you."

THE ſame year *Anthony le Grand*, an eminent Carteſian Philoſopher, dedicated to him his *Hiſtoria Naturæ variis Experimentis & Ratiociniis elucidata, ſecundum Principia ſtabilita in Inſtitutione Philoſophiæ edita ab eodem Authore*, printed at *London*, in 8vo. This gentleman was born at *Doway* in *Flanders*, and entered himſelf in the order of Dominican friars, and was afterwards ſent to preſide over the miſſion in *England*, where he reſided many years, and in Auguſt 1695 was tutor to the eldeſt ſon and heir of Mr. *Farmer* of *Tuſmore* in *Oxfordſhire*[t]. In his dedication he does juſtice to Mr. *Boyle*'s univerſal reputation for extenſive learning and amazing ſagacity in every part of experimental philoſophy; and applies to him what *Averroes* ſaid of *Ariſtotle*, that nature had formed him as an exemplar of the higheſt perfection, to which mankind can attain; and obſerves, that the Royal Society paid the higheſt deference to his judgment: *Adeo diffuſa eſt, per totam Angliam, imo per orbem univerſum, nominis tui gloria, ut vix inter eruditos annumeretur, qui ſapientiam tuam non agnoſcit, & cultu peculiari non amplectitur. Enimverò quis* unquam *te* doctior, *ut quondam de Ariſtotele Romanus orator*, quis acutior, quis in rebus vel inveniendis vel judicandis acrior fuit? *Ita enim in rebus adinveniendis ſolers es, ita in detegendis naturæ arcanis ſagax, ita in promovendâ experimentis philoſophiâ felix, ut non immerito de te, quod in Ariſtotelis laudes aliquando veluti lymphatus eſt Averroes, dici queat, quod* regula & exemplar ſis, quod natura invenit ad demonſtrandam ultimam perfectionem humanam. *Hæc omnium doctorum, qui te norunt, ſententia, cui ſuffragatur celeberrima Londinenſis Regia Societas, quæ tam præclara artis & induſtriæ ſuæ edidit ſpecimina, dum in omnibus, quæ aggreditur, nihil te inconſulto molitur, te aſſerente, nunquam diſſentit, te impugnante, nunquam obluctatur.*

IN 1674 Mr. *Boyle* publiſhed his *Tracts, conſiſting of obſervations about the ſaltneſs of the ſea: An Account of a Statical Hygroſcope and its uſes, together with an Appendix about the Force of the Air's Moiſture: A Fragment about the natural and preternatural ſtate of Bodies. To all which is premiſed a ſceptical Dialogue about the poſitive or privative nature of Cold: with ſome Experiments of Mr.* Boyle'*s referred to in that diſcourſe:* printed at *London*, in 8vo, and tranſlated into Latin. In the *Obſervations concerning the ſaltneſs of the ſea*, he tells us, that the true cauſe of it is from the ſalt, which is diſſolved in it; which ſalt is ſupplied, not only from rocks and other maſſes of ſalt, which at the beginning were, or in ſome places may yet be found, either at the bottom of the ſea, or at the ſides, where the water can reach them; but alſo from the ſalt, which the rains, rivers, and other waters diſſolve in their paſſage through many parts of the earth, and at length carry along with them into the ſea. He informs us of a method to make ſea-water freſh by diſtillation; and obſerves, that the taſte of ſea-water is not ſuch a ſimple ſaline taſte, as ſpring-water would receive from *ſal-gemmæ*, or ſome other pure terreſtrial ſalt diſſolved in it, but a bitteriſh one: which muſt be derived from ſome peculiar cauſe, which authors commonly overlook; and is occaſioned by the liquid and other bitumen, which is carried by ſprings and other waters into the ſea; and he ſuſpects, that the various motion of the ſea, and its being expoſed to the action of the air and ſun, may contribute to give it a taſte other than ſaline, ſince ſea-ſalt by being barely expoſed for many months to the air, and ſometimes put into a gentle agitation by a digeſtive heat, acquired a very different taſte from the ſimple ſolution of ſea-ſalt in common-water. He remarks likewiſe, that there are various degrees of ſaltneſs in different parts of the ſea. In his *Account of a Statical Hygroſcope* he informs us, that it is deſigned to ſhew, 1. The different variations of weather in the ſame month, day, and hour. 2. How much one year and ſeaſon is drier or moiſter than another. 3. To diſcover and compare the changes of the temperature of the air made by winds, ſtrong or weak; froſty, ſnowy, or other weather. 4. To compare the temperature of different houſes and different rooms in the ſame houſe. 5. To obſerve in a chamber the effects of the preſence or abſence of a fire. 6. To keep a chamber at the ſame degree, or at an aſſigned degree of dryneſs. In the *Appendix about the Force of the Air's Moiſture* he gives us inſtances of that force at all ſeaſons, upon animal ſubſtances, vegetables, metals, and minerals, with a deſcription of a *cord-hygrometer*.

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[t] *Wood. Athen. Oxon.* Vol. II. col. 819. 2d Edit.

The ſecond work, which he publiſhed the ſame year, was intitled, *The Excellency of Theology compared with Natural Philoſophy, as both are the Objects of Mens Study; diſcourſed of in a Letter to a Friend. To which are annexed, ſome occaſional Thoughts about the Excellency and Grounds of the mechanical Hypotheſis:* printed at *London* in 8vo, and tranſlated into Latin. This diſcourſe was written in 1665, while Mr. *Boyle*, to avoid the great plague, which then raged in *London*, was obliged to retire into the country, and frequently to paſs from place to place, unaccompanied with moſt of his books[u].

Next followed his *Tracts: containing*, I. *Suſpicions about ſome hidden Qualities of the Air; with an Appendix touching Celeſtial Magnets, and ſome other Particulars.* II. *Animadverſions upon Mr.* Hobbes's Problemata de Vacuo. III. *A Diſcourſe of the Cauſe of Attraction by Suction.* Printed at *London*, 1674, in 8vo, and tranſlated into Latin. In the *Suſpicions about ſome hidden Qualities of the Air*, he mentions ſome unheeded cauſes of the ſalubrity or inſalubrity of the air, which may be compriſed under the following propoſitions: 1. That it is probable, that in ſeveral places the ſalubrity or inſalubrity of the air in general may in a great meaſure depend on ſubterraneous expirations. 2. That in ſeveral places endemical diſtempers chiefly or partly depend on ſubterraneous ſteams. 3. That epidemical diſtempers are in a great meaſure occaſioned by ſubterraneous *effluvia*. 4. That moſt diſeaſes called new ones, chiefly or concurrently, depend on thoſe *effluvia*. He then points out the uſes of his doctrine of ſubterraneous and celeſtial effluvia in the air; and tells us, 1. That we may take occaſion from thence to conſider, whether ſeveral changes of temperature and conſtitution in the air, both as to manifeſt and latent qualities, may not ſometimes be derived from the ſcarcity or plenty, and peculiar nature of one or both of theſe ſorts of *effluvia*. 2. That poſſibly ſome bodies, which we are converſant with, may have a peculiar nature, diſpoſition, and fitneſs to be wrought upon by, or to be aſſociated with ſome of thoſe foreign *effluvia*, emitted by unknown bodies lodged under ground, or which proceed from ſome particular planet. For what we call *ſympathies* and *antipathies* depending really on the peculiar textures and other modifications of the bodies, between which theſe friendſhips and hoſtilities are ſaid to be exerciſed, I ſee not, ſays he, why it is impoſſible, that there ſhould be an affinity between a body of a fit or convenient texture, eſpecially as to the ſhape and ſize of its pores, and the *effluvia* of any other body, whether ſubterraneal or ſidereal. 3. That it may be conſidered, whether among the bodies, which we are acquainted with here below, there are not ſome, which may prove receptacles, if not alſo the attractives of ſidereal and other foreign *effluvia*, which rove up and down in the air. By attractives are meant ſuch magnetical bodies, as are fitted to detain and join with *effluvia*, when by virtue of the various motions, which belong to the air as a fluid, theſe happen to accoſt them. He mentions likewiſe the great effect of the air in producing colours, and ſome *experiments* to manifeſt ſome *hidden qualities in the air*, and ſome *obſervations about the growth of metals in their ore expoſed to the air*.

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[u] See the publiſher's advertiſement to the reader.

HE communicated alſo to the editor of the *Philoſophical Tranſactions*, *An Account of the two ſorts of the* Helmontian *Laudanum*, *together with the Way of the noble Baron* F. M. Van Helmont, *Son to the famous* Johannes Baptiſta, *of preparing his Laudanum.* No. CVII. p. 147.

IN 1675 he publiſhed at *London*, in 8vo, *Some Conſiderations about the Reconcileableneſs of Reaſon and Religion. By* T. E. *a Layman. To which is annexed by the Publiſher a Diſcourſe of Mr.* Boyle *about the Poſſibility of the Reſurrection.* In the beginning of this treatiſe there is mention made of its being to conſiſt of two parts; one to ſhew, that a Chriſtian *need not* lay aſide his reaſon; and the other, that he is *not commanded* to do ſo; but the author thought fit to keep that paper, which related to the latter, from accompanying the former, which ſeemed the moſt ſeaſonable, and likelieſt to make impreſſions on that ſort of perſons, whom he chiefly deſigned to perſuade. Several pieces of his were likewiſe printed the ſame year in the *Philoſophical Tranſactions*, viz. *A Conjecture concerning the Bladders of Air, that are found in Fiſhes, communicated by* A. J. *and illuſtrated by an Experiment ſuggeſted by the honourable Mr.* Boyle. No. CXIV. p. 310. *A new Eſſay-Inſtrument invented and deſcribed, together with the Uſes thereof.* Ibid. No. CXV. p. 329. *Ten new Experiments about the weakened Spring, and ſome unobſerved Effects of the Air; where occur not only ſeveral Trials to diſcover, whether the Spring of the Air, as it may divers ways be increaſed, ſo may not by other ways than Cold or Dilatation be weakened; but alſo ſome odd Experiments, to ſhew the Change of Colours producible in ſome ſolutions and precipitations by the Operation of the Air.* Ibid. No. CXX. p. 467. *An experimental Diſcourſe of Quickſilver growing hot with Gold.* Ibid. No. CXXII. p. 515. In this laſt piece he obſerves, that he had many years before made trials, whether ſome of the heterogeneous particles, which he found reducible with mercury into a laſting mercurial flux, might not ſo alter it, as to diſpoſe it to heat with gold. *And that there were ſuch*, ſays he, *through God's bleſſing, my trials afforded me poſitive proof about the year* 1652. For when he was alone, that he might not be impoſed upon by others, he took to one part of his prepared mercury ſometimes half the weight, and ſometimes an equal weight of refined gold reduced to a calx or ſubtle powder. This he put into the palm of his left hand, and putting the mercury upon it, ſtirred it and preſſed it a little with the fingers of his right hand, by which the two ingredients were eaſily mingled, and grew not only ſenſibly but conſiderably hot, and that ſo nimbly, that the incaleſcence ſometimes came to its height in about a minute of an hour by a minute clock. He found the experiment ſucceed, whether he took altogether, or but half as much gold as mercury; but the effect ſeemed to be much greater, when they were employed in equal weight. He tried alſo the ſame mercury with refined ſilver reduced to very fine powder; but he could not perceive any heat or warmth at all, though he was apt to think, with a ſufficient quantity of leaf-ſilver it might have been ſenſible. He made trial oftner than once in the hands of others, who were not a little ſurpriſed and pleaſed with the event; and particularly having given the ingredients to Mr. *Oldenburg*, he deſired him to make the experiment in and with his own hand, in which it proved ſucceſsful within ſomewhat leſs than a minute of an hour. Mr. *Boyle* however tells us, that he will not hence determine, whether thoſe that are *mercurii corporum*, and were made, as chemiſts preſume, by extraction only from metals and minerals, will each of them grow hot with gold; “ as (ſays he) if I much miſtake not, I “ found antimonial mercury to do. Nor will I affirm, that every metalline mercury, “ though never ſo diſpoſed to incaleſcence, or even that of ſilver and gold itſelf, is “ the ſame with that, which the Chryſopœan writers mean by their *philoſophic mercury*, “ or is near ſo noble as this. Nay I will not ſo much as affirm, that every mercury “ obtained by extraction, even from the perfect metals themſelves, muſt needs be “ more noble and fit, as alchemiſts ſpeak, for the *philoſophic work*, than that, which “ common mercury ſkilfully freed from its recrementitious and heterogeneous parts, “ and richly impregnated with the ſubtle and active ones of congruous metals or “ minerals.—But if there be any truth in what ſome of the moſt approved Spagy- “ riſts have delivered about a ſolvent of gold, that ſeems of kin, and perhaps is not “ much nobler than one that I had; it ſeems allowable to expect, that even ours “ ſhould be of more than ordinary uſe both in phyſic and alchemy.” This diſcourſe occaſioned Mr. (afterwards Sir) *Iſaac Newton* to write the following [w] letter to Mr. *Oldenburg* from *Cambridge*, dated April 26, 1676.

“ SIR,

“ * * * * Yeſterday I reading the two laſt *Philoſophical Tranſactions* had the op- “ portunity to conſider Mr. *Boyle*'s uncommon experiment about the *incaleſcence of* “ *gold and mercury*. I believe the fingers of many will itch to be at the knowledge “ of the preparation of ſuch a mercury; and for that end ſome will not be wanting “ to move for the publiſhing of it, by urging the good it may do in the world. “ But, in my ſimple judgment, the noble author, ſince he has thought fit to reveal “ himſelf ſo far, does prudently in being reſerved in the reſt. Not that I think any “ great excellence in ſuch a ☿, either for medical or chemical operations; for it “ ſeems to me, that the metalline particles, with which that ☿ is impregnated, may “ be groſſer than the particles of the ☿, and be diſpoſed to mix more readily with “ the ⊙ upon ſome other account than their ſubtilty; and then in ſo mixing, their “ groſſneſs may enable them to give the parts of the gold the greater ſhock, and ſo “ put them into briſker motion than ſmaller particles could do, much after the “ manner that the ſaline particles, wherewith corroſive liquors are impregnated, heat “ many things, which they are put to diſſolve, whilſt the finer parts of common “ water ſcarce heat any thing diſſolved therein, be the diſſolution never ſo quick. “ And if they do heat any thing (as quick lime) one may ſuſpect, that heat is pro- “ duced by ſome ſaline particles lying hid in the body, which the water ſets on work “ upon the body, which they could not act on, whilſt in a dry form. I would compare therefore this impregnated ☿ to ſome corroſive liquor as aqua fortis; the mercurial part of the one to the watery or phlegmatic part of the other, and the metallic particles with which the one is impregnated, to the ſaline particles with which the other is impregnated; both which I ſuppoſe may be of a middle nature between the liquor which they impregnate, and the bodies they diſſolve, and ſo enter thoſe bodies more freely, and by their groſſneſs ſhake the diſſolved particles more ſtrongly than a ſubtiler agent would do. If this analogy of theſe two kinds of liquor may be allowed, one may gueſs at the little uſe of the one, by the indiſpoſition of the other, either to medicine or vegetation. But yet becauſe the way by which ☿ may be ſo impregnated, has been thought fit to be concealed by others that have known it, and therefore may poſſibly be an inlet to ſomething more noble, not to be communicated without immenſe damage to the world, if there ſhould be any verity in the Hermetic writers; therefore I queſtion not, but that the great wiſdom of the noble author will ſway him to high ſilence, till he ſhall be reſolved of what conſequence the thing may be, either by his own experience, or the judgement of ſome other that thoroughly underſtands what he ſpeaks about; that is, of a true Hermetic philoſopher, whoſe judgment (if there be any ſuch) would be more to be regarded in this point, than that of all the world beſide to the contrary, there being other things beſide the *tranſmutation of metals*, (if thoſe great pretenders brag not) which none but they underſtand. Sir, becauſe the author ſeems deſirous of the ſenſe of others in this point, I have been ſo free as to ſhoot my bolt; but pray keep this letter private to yourſelf.

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[w] In the poſſeſſion of *William Jones*, Eſq.

“ Your ſervant,

“ ISAAC NEWTON.”

IN 1676 Mr. *Boyle* gave the public his *Experiments, Notes, &c. about the mechanical origin or production of divers particular qualities: among which is inſerted a Diſcourſe of the Imperfection of the Chemiſts Doctrine of Qualities; together with ſome Reflections upon the Hypotheſis of Alkali and Acidum:* and likewiſe diſcourſes *of the mechanical origin of Heat and Cold: Experiments and Obſervations about the mechanical production of Taſtes: Of Odours: Advertiſements about the Experiments and Notes relating to chemical Qualities: Experiments and Notes about the mechanical Origin and production of Volatility:—Of Fixedneſs:—Of Corroſiveneſs and Corroſibility: Of the mechanical Cauſes of chemical Precipitation: Experiments and Notes about the mechanical Production of Magnetiſm, and of Electricity.* Printed at *London* in 8vo. In the *Mechanical Origin of Heat and Cold* he ſhews, how a great degree of coldneſs may be produced at any time of the year. That a coldneſs may be produced by the mixture of warm bodies, and heat by the mixture of cold bodies. That ebullition is no argument of heat. That the ſame body, which cools ſome liquors, may heat others. That heat is mechanically producible. That agitation, and various determinations, and ſmalneſs of the moving particles, are requiſite to heat. And that not only weather-glaſſes, but likewiſe our ſenſes, may give us miſinformation about cold by ſeveral prediſpoſitions, and the temper of our ſenſories, as we find it colder in the air coming out of a hot bath, than when only out of a warm room. In his *Experiments and Obſervations about the mechanical Production of Taſtes*, he defines taſte, as belonging to the object, to be that quality, or whatever elſe it be, which enables a body, by its operation, to produce in us that ſenſation which we feel, or perceive, when we ſay we taſte; and then ſhews, that this quality depends upon the ſhape, ſize, motion, and other mechanical affections of the ſmall parts of the taſted body, and reſults from the aſſociation of two or more of them, not excluding their congruity or incongruity to the organs of taſting. In his *Experiments and Notes about the mechanical Origin and Production of Volatility,* he informs us, that to make a body volatile, the parts muſt be very ſmall, not too ſolid or heavy, but conveniently ſhaped for motion; and that they looſely adhere, or at leaſt be united in ſuch a manner, as does not much indiſpoſe them to be ſeparated by the fire in the form of fumes or vapours. With regard to *Fixedneſs,* he ſuppoſes the qualifications that conduce moſt to it are, 1. The groſſneſs or the bulk of corpuſcles the body conſiſts of. 2. The ponderouſneſs or ſolidity of thoſe corpuſcles. 3. The inaptitude of them for avolation, by reaſon of their branchedneſs, irregular figures, crookedneſs, or other inconvenient ſhape, which entangles the particles among one another, and makes them difficult to be extracted. He explains the theory of *Corroſiveneſs,* by obſerving, 1. That it is requiſite, that a corroſive medium ſhould abound with corpuſcles not too big to get in at the pores of the body to be diſſolved, nor yet ſo minute as to paſs through them, as the rays of light through glaſs; or to be unable, by reaſon of their great ſlenderneſs and flexibility, to disjoin the parts which they invade. 2. That theſe corpuſcles be of a ſhape fit to inſinuate themſelves into the pores abovementioned, in order to diſſociate the ſolid parts. 3. That they have a competent degree of ſolidity to disjoin the particles of the body to be diſſolved; which ſolidity of ſolvent particles is ſomewhat diſtinct from their bulk, as may appear by comparing a ſtalk of wheat and a metalline wire of the ſame diameter. 4. That the corpuſcles of the menſtruum be ſuch, as are fit to ſeparate the parts of the invaded body, either by their ſhape, minuteneſs, or fitneſs to have their action befriended by aſſiſting cauſes; ſuch as, firſt, the preſſure of the atmoſphere, which may impel them into the pores of bodies, not filled with a ſubſtance ſo reſiſting as common air; and ſecondly, that the intruding corpuſcles may be fitted to receive in thoſe pores by the tranſcurſion of ſubtile ætherial matter, or by the numerous pulſes of the ſwimming corpuſcles of the menſtruum itſelf, whereby, like ſo many little wedges and levers, they may be enabled to force aſunder the little parts between which they inſinuate. This theory he confirms by experiments. He then explains that of *Corroſibility,* which property appears to him to conſiſt in three requiſites: 1. That the body to be corroded have pores of ſuch a magnitude and figure, that the corpuſcles of the ſolvent may enter them, and agitate the ſolid parts that incloſe them. 2. That its conſiſtent corpuſcles be of ſuch a bulk and ſolidity, as does not render them incapable of being disjoined by the action of the inſinuating corpuſcles of the menſtruum. 3. This property conſiſts in ſuch a coheſion of the parts, as is not too ſtrong to be ſuperable by the action of the menſtruum. This theory he likewiſe confirms by experiments. With reſpect to *Precipitation* he obſerves, that by it is meant ſuch an agitation or motion of an heterogeneous liquor, as makes the parts of it ſubſide in the form of a powder or other conſiſtent body; and that it may be mechanically accounted for from the gravity and bulk of the diſſolved body, from weakening the ſolvent, and by preſenting another body to be wrought upon, *&c.* And with regard to *Electricity,* he tells us, that electrical bodies act mechanically; and that electricity is producible by mechanical changes.

HE communicated to the public the ſame year, in the *Philoſophical Tranſactions, New Experiments about the ſuperficial Figures of Fluids, eſpecially of Liquors contiguous to other Fluids* No. CXXXI. p. 775. and *A Continuation of the Experiments publiſhed in the next foregoing Tract about Fluids contiguous to other Fluids.* Ibid. No. CXXXII. p. 799.

HE had been for many years a director of the *Eaſt-India* company, and greatly inſtrumental in procuring the charter of it, an abſtract of which is extant among his papers. But his health now not permitting him to attend the committee of that company, he wrote the following letter to *Robert Thompſon*, Eſq; in order to recommend to them the propagation of the Goſpel in thoſe countries, where their commerce gave them an opportunity.

" Worthy SIR, *Pall-Mall*, March 5, 1676-7.

" THE continuance of my diſtemper not permitting me to wait on you, as I " gladly would, in *London*, you will, I doubt not, give me leave to preſent you " in this paper ſome of the things, that I would more fully have acquainted you with " by word of mouth. You may remember, that when my health, and the kindneſs " of the *Eaſt-India* company, allowed me to ſit in your committee, I ventured to " make a motion, that ſome courſe might be thought on of doing ſome conſiderable " thing for the propagation of the goſpel among the natives, in whoſe countries we " have flouriſhing factories. And indeed it ſeemed to me very fit, that we, whoſe " endeavours God had of late ſo ſignally proſpered, ſhould pay him ſome little acknowledgment of his many bleſſings, and that remembering ourſelves to be Chriſtians, as well as merchants, we ſhould attempt to bring thoſe countries ſome ſpiritual good things, whence we ſo frequently brought back temporal ones. And I " was ſomewhat the more concerned to ſucceed in the motion I made, becauſe I wiſhed " the company were enough to deſire, that they in particular ſhould have the honour " to ſilence the reproaches of thoſe who I wiſh had leſs pretence to upbraid the Proteſtants, and among them the Engliſh, with the neglect of making proſelytes to the " Chriſtian religion; to the advancement of which I endeavoured to excite you by what " hath been done by the Dutch company in *Batavia*. I muſt do and have done thoſe " that were preſent at my propoſal, the right to ſay, that they gave me a very favourable hearing, and readily conſented to take the matter into further conſideration. " But as my ſickneſs diſabled me from going ſo far as the *Eaſt-India* houſe, ſo other " accidents have from time to time been able to produce ſuch delays, that notwithſtanding the interpoſition of my lord *Berkeley*, (who uſes not to be backward, when " good is to be done) the good intentions of the company have hitherto proved ineffectual. But I am glad to find, that now the conſideration of that buſineſs is to " be ſeriouſly reſumed among you, my lord *Berkeley* having yeſterday done me the " honour to viſit me, and diſcourſe with me about this affair. And ſince I cannot " wait on you myſelf, and have acquainted him with ſome of my thoughts, I ſhall " deſire you at your next meeting to confer with his lordſhip about the ways of promoting ſo good a work. And I the rather ſolicit this now, becauſe ſince the beginning of this week I received the honour of a viſit from the biſhop of *Oxford*, " with whom diſcourſing of divers things relating to religion, his lordſhip ſeemed to " wonder, that the *Eaſt-India* company ſhould do nothing towards the ſpreading of " it in the country they trade to, and ſeemed unwilling to ſend able men thither for " miniſters. And though I took this occaſion to right the company by ſhewing him " that he had been miſinformed, and to relate to him the matter of fact, which I could " do in great part upon my own knowledge; yet his lordſhip having aſſured me, that " in caſe reaſonable encouragement were given, ſober and learned men ſhould be fitted " in the univerſity to be ſent into *India*, and furniſhed not only with the Arabic tongue, " but, if it were deſired, with arithmetic and other qualifications fit to recommend

" them, and make them appear more conſiderable, and grow more uſeful in thoſe " parts; I cannot but take this riſe to ſolicit you to reſume in good earneſt the " thoughts of ſpeedily doing ſomething worthy of the famous *Eaſt-India* company of " *England*. The way I leave to your wiſdoms, not deſpairing, that if but ſo much " be done at firſt as may be carried without conſiderable oppoſition, the goodneſs of " the work will procure a bleſſing on it that will make it proſperous; and the ſucceſs will invite perhaps many more than your own company to be co-operators " with the truth, and contributors to the enlarging the Pale of the Chriſtian church. " If you ſhould now aſk me, what is to be done with any ſtock or income that may " be raiſed for ſuch an uſe? I ſhall willingly at large acquaint you with my poor " thoughts the next time you paſs this way; and in the mean time give me leave to " put you in mind of what is done in the corporation (whereof you are a member) " for goſpellizing (as they phraſe it) the natives of *New England*. For the ways we " employ to convert and inſtruct them, are chiefly theſe: Firſt, We have cauſed " the holy ſcriptures and ſome few choice practical books to be tranſlated into their " chiefeſt language. To which you may add the publiſhing of a ſolid but civilly " penned confutation of the authentic books wherein the Bramins religion is " contained.

" NEXT, we have cauſed ſome of ours to learn their tongue; and having convinced " them of their idolatry and the ſinfulneſs of their courſes, to preach to them, and to " catechiſe them in their own language.

" AND then we breed ſome of their hopeful forward youths to that knowledge of " the Engliſh tongue and European learning, that they may afterwards be able to confute the idolatrous prieſts, and convert and inſtruct their own countrymen.

" OTHER methods we do on ſome occaſions employ; but to particulariſe and inſiſt " on them, would require more than a letter; and this, I fear, is grown too tedious " already, and therefore I ſhall here conclude it with my hearty wiſhes, that, eſpecially at a time when the company wants not enemies, they may be directed to do " a thing ſo good in itſelf, and of ſo good a report as that which has been recommended to you, I hope, by your own piety, as well as by the motion of,

" SIR,

" Your moſt affectionate friend, and moſt humble ſervant,

" ROBERT BOYLE."

HIS zeal for the intereſt of the Chriſtian religion, appeared the year following in another remarkable inſtance; for he was at the expence of printing five hundred copies of the *four Goſpels*, and *Acts of the Apoſtles*, in the Malayan tongue, under the direction of Mr. (afterwards Dr.) *Thomas Hyde*, keeper of the Bodleian library. This work was publiſhed at *Oxford* in 1677 in 4to, with this title: *Jang Ampat Evangelia derri tuan Kitu Jeſu Chriſti duan Beorboutan derri jang Apoſtoli Borſacti Berſalin dallam Baſſa Malayo. That is, The four Goſpels of our Lord Jeſus Chriſt, and the Acts of the holy Apoſtles, tranſlated into the Malayan tongue:* with a preface by Dr. *Thomas Marſhal*, rector of *Lincoln* college in *Oxford*, and afterwards dean of *Glouceſter*, and with the following dedication by Mr. *Hyde*, which I ſhall here inſert from the original.

“*To the honourable* Robert Boyle, *Eſq; one of the directors of the* Eaſt-India *company for trade, and governor of the corporation for the propagation of the Goſpel and the converſion of the American natives in* New England.

“SIR,

“YOUR pious deſign of printing the holy Goſpels, and the Acts of the Apoſtles, in the Malayan tongue, being by God's aſſiſtance finiſhed at your coſt and charge, as correct as all my diligence could poſſible make it, the work now done returns to you, offering itſelf to be employed to that good end and purpoſe for which you intended it, *viz.* for God's glory, and the good of the ſouls of thoſe poor Indians, to whom the glorious rays of the Goſpel have not yet ſhone, or at leaſt in a very obſcure manner; that ſo by this means they who ſat in darkneſs and the ſhadow of death, may have great light riſen. And the converſion of ſuch miſerable infidels from their groſs idolatry, and the gaining of ſouls to heaven, being a work ſo very acceptable to both God and man; we well hope, that this your good example will prove an incitement to others of our nation to procure and promote ſuch like laudable undertakings. But your charity is not limited only to the Eaſt-Indians, for the poor ſouls of the Weſt-Indians are alſo bound to bleſs you, you being the head of that corporation, which is eſtabliſhed by his Majeſty at *London* for the receiving and diſpoſing of the benefactions of well-minded Chriſtians (to which the ſaid corporation do uſually add of their own no ſmall mites) to be tranſmitted to the commiſſioners of the united colonies in *New England*, and there to be employed for the propagation of the Goſpel. By the ſole coſt and care of this forementioned corporation it was, that the whole Bible, and ſome other books of piety, were tranſlated into the language of *New England* by the pains of the reverend Mr. *Eliot*, who made a grammar, now alſo in print, for that language, and who daily labours in the work of the Goſpel there, he having an honorary ſtipend continued to him by the abovementioned corporation. Wherefore, if charity is not grown too cold in theſe northerly parts of the world, thoſe things, which have been already performed, may juſtly be expected to be ſufficient incentives to kindle and actuate the zeal of many good people, by whoſe pious endeavours, in the promoting of the Goſpel of our Lord Jeſus Chriſt, all the ends of the earth may in time ſee the ſalvation of God. All which I have now to add is, that God in his goodneſs would be pleaſed to continue to you long life and happineſs; that ſo you may chearfully go on to finiſh the good works which you have begun, and which you farther intend; which is the hearty prayer of,

“SIR,

Sept. 14, 1677, from the public
library in *Oxford*.

“Your very obliged friend,

“and moſt humble ſervant,

“THOMAS HYDE.”

THE ſame year there was publiſhed at *Geneva*, in 4to, a collection of ſome of our author's works, which had been tranſlated into Latin, under this title, ROBERTI BOYLE, *nobiliſſimi Angli, & Societatis Regiæ digniſſimi ſocii, opera varia*; of which collection Mr. *Oldenburg*, in the *Philoſophical Tranſactions*, No. CXXX. p. 766, 767, gives the following account: 1. That is was publiſhed without the conſent or knowledge of the author. 2. That the year in the frontiſpiece of it is one and the ſame, as if the ſeveral treatiſes contained in this volume had been publiſhed in one year; and that the enumeration of thoſe treatiſes, made in the catalogue of this Latin edition, is not according to the time wherein they were firſt printed; for the *firſt* of the books mentioned in that catalogue was publiſhed in Engliſh in 1660, the *fifth* and *ſixth* in 1661, the *ſecond* in 1662, the *ſeventh* in 1664, the *fourth* in 1666, the *third* in 1670, the *eighth* in 1671, the *tenth* in 1672, and the *ninth* in 1673, "ſo prepoſterouſly are thoſe " books ranged in this catalogue and volume; which the reader was to be informed " of, that by comparing the ſeveral true dates of the firſt edition of this author's " works with the books of others ſince printed, the priority of the experiments, and " conſiderations reſpectively contained in them, may be truly ſtated." 3. That there is no mention made in the general title, nor in any advertiſement, that theſe books are all of them tranſlations out of Engliſh, in which tongue the author wrote them all. 4. That the treatiſe of the *Origin of Forms and Qualities*, and that of *ſubordinate Forms*, are both omitted in this volume, though they were printed even in Latin, at *Oxford*, ever ſince the year 1669, as they had been printed in Engliſh in 1667.

IN 1678 Mr. *Boyle*'s *Short Memorial of ſome Obſervations made upon an artificial Subſtance, that ſhines without any preceding Illuſtration*, was publiſhed in Mr. *Hook*'s *Lectiones Cutler*. No. II. p. 57. and his *Hiſtorical Account of a Degradation of Gold made by an Anti-Elixir, a ſtrange chemical Narrative*, was printed at *London* in 4to, and reprinted there in 1739 in 4to. In this piece, *Pyrophilus*, one of the interlocutors of the Dialogue, obſerves, that though he could not affirm, that " he had with his own " hands made *projection* (as chemiſts are wont to call the ſudden tranſmutation made " by a ſmall quantity of their admirable elixir) yet I can affirm, *ſays he*, much of " what hath been urged for the poſſibility of ſuch a ſudden change of a metalline body " by a way which, I preſume, will ſurpriſe you. For to make it more credible, that " other metals are capable of being graduated or exalted into gold, by way of *pro-* " *jection*, I will relate to you, that *by the like way* gold has been degraded or imbaſed. " If, *continues he*, it can be made appear, that art has produced an *anti-elixir*, or " agent, that is able in a very ſhort time to work a very notable though deteriorating " change upon a metal, in proportion to which its quantity is very inconſiderable; " I ſee not why it ſhould be thought impoſſible, that art may alſo make a *true elixir*, " or powder, capable of ſpeedily tranſmuting a great proportion of a baſer metal into " ſilver or gold; eſpecially if it be conſidered, that thoſe that treat of theſe *arcana*, " confeſs, that it is not every matter which may be juſtly called the *Philoſophers* " *Stone*, that is able to tranſmute other metals in vaſt quantities, ſince ſeveral of theſe " writers (and even *Lully* himſelf) make differing orders or degrees of *elixir*, and ac- " knowledge, that a medicine or tincture of the firſt or loweſt order, will not tranſmute " above ten times its weight of an imperfect metal." However the celebrated chemiſt *Otto Tachenius* was of opinion, that notwithſtanding what is related in this narrative, ſalt of tartar is capable of recovering gold thus debaſed to its original purity.

THE regard which the great *Newton* had for Mr. *Boyle*, will appear from a very curious letter which the former wrote to him, explaining his ſentiments upon one of the moſt abſtruſe points of philoſophy with reſpect to the ætherial medium, which in his *Optics* he propoſes as the mechanical cauſe of gravitation. This letter having never before ſeen the light, will be proper to be inſerted here.

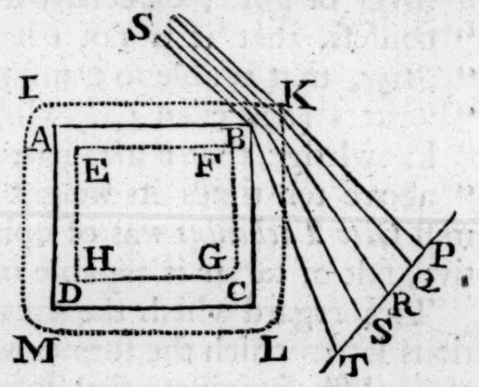
“Honoured SIR,

“I HAVE ſo long deferred to ſend you my thoughts about the phyſical qualities we ſpeak of, that did I not eſteem myſelf obliged by promiſe, I think I ſhould be aſhamed to ſend them at all. The truth is, my notions about things of this kind are ſo indigeſted, that I am not well ſatisfied myſelf in them; and what I am not ſatisfied in, I can ſcarce eſteem fit to be communicated to others; eſpecially in natural philoſophy, where there is no end of fancying. But becauſe I am indebted to you, and yeſterday met with a friend, Mr. *Maulyverer*, who told me he was going to *London*, and intended to give you the trouble of a viſit, I could not forbear to take the opportunity of conveying this to you by him.

“IT being only an explication of qualities which you deſire of me, I ſhall ſet down my apprehenſions in the form of ſuppoſitions, as follows. And firſt, I ſuppoſe, that there is diffuſed through all places an æthereal ſubſtance, capable of contraction and dilatation, ſtrongly elaſtic, and, in a word, much like air in all reſpects, but far more ſubtile.

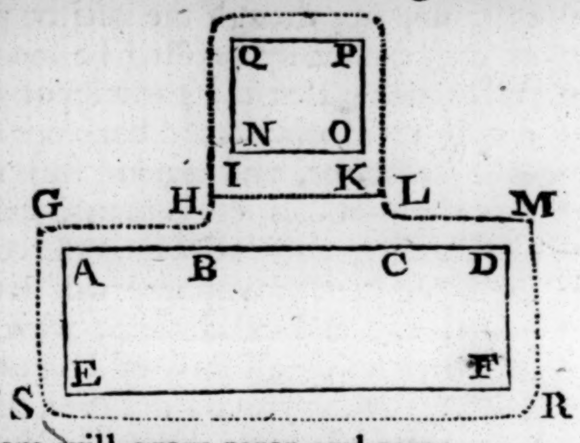
“2. I SUPPOSE this æther pervades all groſs bodies, but yet ſo as to ſtand rarer in their pores than in free ſpaces, and ſo much the rarer, as their pores are leſs. And this I ſuppoſe (with others) to be the cauſe why light incident on thoſe bodies is refracted towards the perpendicular; why two well poliſhed metals cohere in a receiver exhauſted of air; why ☿ ſtands ſometimes up to the top of a glaſs pipe, though much higher than 30 inches; and one of the main cauſes, why the parts of all bodies cohere; alſo the cauſe of filtration, and of the riſing of water in ſmall glaſs pipes above the ſurface of the ſtagnating water they are dipped into: for I ſuſpect the æther may ſtand rarer, not only in the inſenſible pores of bodies, but even in the very ſenſible cavities of thoſe pipes. And the ſame principle may cauſe menſtruums to pervade with violence the pores of the bodies they diſſolve, the ſurrounding æther, as well as the atmoſphere, preſſing them together.

“I SUPPOSE the rarer æther within bodies, and the denſer without them, not to be terminated in a mathematical ſuperficies, but to grow gradually into one another; the external æther beginning to grow rarer, and the internal to grow denſer, at ſome little diſtance from the ſuperficies of the body, and running through all intermediate degrees of denſity in the intermediate ſpaces. And this may be the cauſe why light, in *Grimaldo*’s experiment, paſſing by the edge of a knife, or other opake body, is turned aſide, and as it were refracted, and by that refraction makes ſeveral colours. Let ABCD be a denſe body, whether opake or tranſparent, EFGH the outſide of the uniform æther, which is within it, IKLM the inſide of the uniform æther, which is without it; and conceive the æther, which is between EFGH and IKLM, to run through all intermediate degrees of denſity between that of the two uniform æthers on either ſide. This being ſuppoſed, the rays of the ſun SB, SK, which paſs by the edge of this body between B and K, ought in their paſſage through the unequally denſe æther there, to receive a ply from the denſer æther, which is on that ſide towards K, and that the more by how much they paſs nearer to the body, and thereby to be ſcattered through the ſpace PQRST, as by experience they are found to be. Now the ſpace between the limits EFGH and IKLM, I ſhall call the ſpace of the æther’s graduated rarity.



“4. WHEN

“ When two bodies moving towards one another come near together, I ſuppoſe the æther between them to grow rarer than before, and the ſpaces of its graduated rarity to extend further from the ſuperficies of the bodies towards one another; and this, by reaſon that the æther cannot move and play up and down ſo freely in the ſtrait paſſage between the bodies, as it could before they came ſo near together. Thus, if the ſpace of the æther's graduated rarity reach from the body ABCDFE only to the diſtance GHLMRS, when no other body is near it, yet may it reach farther, as to IK, when another body NOPQ approaches: and as the other body approaches more and more, I ſuppoſe the æther between them will grow rarer and rarer.



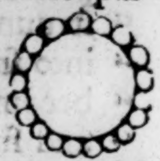
“ These ſuppoſitions I have ſo deſcribed, as if I thought the ſpaces of graduated æther had preciſe limits, as is expreſſed at IKLM in the firſt figure, and GMRS in the ſecond: for thus I thought I could better expreſs myſelf. But really I do not think they have ſuch preciſe limits, but rather decay inſenſibly, and, in ſo decaying, extend to a much greater diſtance than can eaſily be believed, or need be ſuppoſed.

“ 5. Now from the fourth ſuppoſition it follows, that when two bodies approaching one another; come ſo near together as to make the æther between them begin to rarefy, they will begin to have a reluctance from being brought nearer together, and an endeavour to recede from one another: which reluctance and endeavour will encreaſe, as they come nearer together, becauſe thereby they cauſe the interjacent æther to rarefy more and more. But at length, when they come ſo near together, that the exceſs of preſſure of the external æther, which ſurrounds the bodies, above that of the rarefied æther, which is between them, is ſo great, as to overcome the reluctance which the bodies have from being brought together; then will that exceſs of preſſure drive them with violence together, and make them adhere ſtrongly to one another, as was ſaid in the ſecond ſuppoſition. For inſtance, in the ſecond figure, when the bodies ED and NP are ſo near together, that the ſpaces of the æther's graduated rarity begin to reach to one another, and meet the line IK; the æther between them will have ſuffered much rarefaction, which rarefaction requires much force, that is, much preſſing of the bodies together: and the endeavour which the æther between them has to return to its former natural ſtate of condenſation, will cauſe the bodies to have an endeavour of receding from one another. But, on the other hand, to counterpoiſe this endeavour, there will not yet be any exceſs of denſity of the æther which ſurrounds the bodies, above that of the æther which is between them at the line IK. But if the bodies come nearer together, ſo as to make the æther in the mid-way line IK grow rarer than the ſurrounding æther, there will ariſe from the exceſs of denſity of the ſurrounding æther a compreſſure of the bodies towards one another: which when by the nearer approach of the bodies it becomes ſo great, as to overcome the aforeſaid endeavour the bodies have to recede from one another, they will then go towards one another and adhere together. And, on the contrary, if any power force them aſunder to that diſtance, where the endeavour to recede begins to overcome the endeavour to accede, they will again leap from one another. Now hence I conceive it is chiefly, that a fly walks on water without wetting her feet, and conſequently without touching the

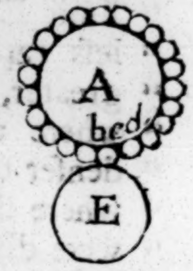
“ water; that two poliſhed pieces of glaſs are not without preſſure brought to contact, no, not though the one be plain, the other a little convex; that the particles of duſt cannot by preſſing be made to cohere, as they would do, if they did but fully touch; that the particles of tinging ſubſtances and ſalts diſſolved in water do not of their own accord concrete and fall to the bottom, but diffuſe themſelves all over the liquor, and expand ſtill more, if you add more liquor to them. Alſo, that the particles of vapours, exhalations, and air, do ſtand at a diſtance from one another, and endeavour to recede as far from one another, as the preſſure of the incumbent atmoſphere will let them: for I conceive the confuſed maſs of vapours, air, and exhalations, which we call the atmoſphere, to be nothing elſe but the particles of all ſorts of bodies, of which the earth conſiſts, ſeparated from one another, and kept at a diſtance, by the ſaid principle.

“ FROM theſe principles the actions of menſtruums upon bodies may be thus explained. Suppoſe any tinging body, as cochineal, or logwood, be put into water; ſo ſoon as the water ſinks into its pores and wets on all ſides any particle, which adheres to the body only by the principle in the ſecond ſuppoſition, it takes off, or at leaſt much diminiſhes the efficacy of that principle to hold the particle to the body, becauſe it makes the æther on all ſides the particle to be of a more uniform denſity than before. And then the particle being ſhaken off, by any little motion, floats in the water, and with many ſuch others makes a tincture; which tincture will be of ſome lively colour, if the particles be all of the ſame ſize and denſity; otherwiſe of a dirty one. For the colours of all natural bodies whatever ſeem to depend on nothing but the various ſizes and denſities of their particles; as I think you have ſeen deſcribed by me more at large in another paper. If the particles be very ſmall (as are thoſe of ſalts, vitriols, and gums) they are tranſparent; and as they are ſuppoſed bigger and bigger, they put on theſe colours in order, black, white, yellow, red; violet, blue, pale green, yellow, orange, red; purple, blue, green, yellow, orange, red, *&c.* as is diſcerned by the colours, which appear at the ſeveral thickneſſes of very thin plates of tranſparent bodies. Whence, to know the cauſes of the changes of colours, which are often made by the mixtures of ſeveral liquors, it is to be conſidered, how the particles of any tincture may have their ſize or denſity altered by the infuſion of another liquor.

“ WHEN any metal is put into common water, the water cannot enter into its pores, to act on it and diſſolve it. Not that water conſiſts of too groſs parts for this purpoſe, but becauſe it is unſociable to metal. For there is a certain ſecret principle in nature, by which liquors are ſociable to ſome things, and unſociable to others. Thus water will not mix with oil, but readily with ſpirit of wine, or with ſalts. It ſinks alſo into wood, which quickſilver will not; but quickſilver ſinks into metals, which, as I ſaid, water will not. So aqua fortis diſſolves ☽, not ☉, aqua regis ☉, not ☽, *&c.* But a liquor, which is of itſelf unſociable to a body, may, by the mixture of a convenient mediator, be made ſociable. So molten lead, which alone will not mix with copper, or with regulus of Mars, by the addition of tin is made to mix with either. And water, by the mediation of ſaline ſpirits, will mix with metal. Now when any metal is put in water impregnated with ſuch ſpirits, as into aqua fortis, aqua regis, ſpirit of vitriol, or the like, the particles of the ſpirits, as they, in floating in the water, ſtrike on the metal, will by their ſociableneſs enter into its pores, and gather round its outſide particles, and, by advantage of the continual tremor the particles of the metal are in, hitch themſelves in by degrees between thoſe particles and the body, and looſen them from it; and the water entering into the pores together with the ſaline ſpirits, the particles of the metal will be thereby ſtill more looſed, ſo as, by that motion the ſolution puts them into, to be eaſily ſhaken off, and made to float in the water: the ſaline particles ſtill encompaſſing the metallic ones as a coat or ſhell does a kernel, after the manner expreſſed in the annexed figure. In which figure I have made the particles round, though they may be cubical, or of any other ſhape.



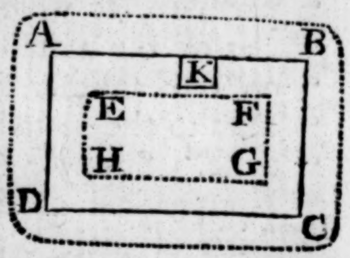
“ IF into a ſolution of metal thus made be poured a liquor, abounding with particles, to which the former ſaline particles are more ſociable than to the particles of the metal (ſuppoſe with particles of ſalt of tartar) then ſo ſoon as they ſtrike on one another in the liquor, the ſaline particles will adhere to thoſe more firmly than to the metalline ones, and by degrees be wrought off from thoſe to encloſe theſe. Suppoſe A a metalline particle, encloſed with ſaline ones of ſpirit of nitre, E a particle of ſalt of tartar, contiguous to two of the particles of ſpirit of nitre b and c, and ſuppoſe the particle E is impelled by any motion towards d, ſo as to roll about the particle c, till it touch the particle d, the particle b adhering more firmly to E than to A, will be forced off from A. And by the ſame means the particle E, as it rolls about A, will tear off the reſt of the ſaline particles from A, one after another, till it has got them all, or almoſt all, about itſelf. And when the metallic particles are thus diveſted of the nitrous ones, which, as a mediator between them and the water, held them floating in it; the alcalizate ones crowding for the room the metallic ones took up before, will preſs theſe towards one another, and make them come more eaſily together: ſo that by the motion they continually have in the water, they ſhall be made to ſtrike on one another, and then, by means of the principle in the ſecond ſuppoſition, they will cohere and grow into cluſters, and fall down by their weight to the bottom, which is called precipitation.



“ IN the ſolution of metals, when a particle is looſing from the body, ſo ſoon as it gets to that diſtance from it, where the principle of receding deſcribed in the fourth and fifth ſuppoſitions begins to overcome the principle of acceding, deſcribed in the ſecond ſuppoſition, the receding of the particle will be thereby accelerated; ſo that the particle ſhall as it were with violence leap from the body, and putting the liquor into a briſk agitation, beget and promote that heat we often find to be cauſed in ſolutions of metals. And if any particle happen to leap off thus from the body, before it be ſurrounded with water, or to leap off with that ſmartneſs, as to get looſe from the water; the water, by the principle in the fourth and fifth ſuppoſitions will be kept off from the particle, and ſtand round about it, like a ſpherically hollow arch, not being able to come to a full contact with it any more. And ſeveral of theſe particles afterwards gathering into a cluſter, ſo as by the ſame principle to ſtand at a diſtance from one another, without any water between them, will compoſe a bubble. Whence I ſuppoſe it is, that in briſk ſolutions there uſually happens an ebullition.

“ THIS is one way of tranſmuting groſs compact ſubſtances into aëreal ones. Another way is, by heat. For as faſt as the motion of heat can ſhake off the particles of water from the ſurface of it, thoſe particles, by the ſaid principle, will float up and down in the air, at a diſtance both from one another, and from the particles of air, and make that ſubſtance we call vapour. Thus I ſuppoſe it is, when the particles of a body are very ſmall (as I ſuppoſe thoſe of water are) ſo

“ that the action of heat alone may be ſufficient to ſhake them aſunder. But if the particles be much larger, they then require the greater force of diſſolving menſtruums, to ſeparate them, unleſs by any means the parties can be firſt broken into ſmaller ones. For the moſt fixed bodies, even gold itſelf, ſome have ſaid will become volatile, only by breaking their parts ſmaller. Thus may the volatility and fixedneſs of bodies depend on the different ſizes of their parts.



“ AND on the ſame difference of ſize may depend the more or leſs permanency of aerial ſubſtances, in their ſtate of rarefaction. To underſtand this, let us ſuppoſe A B C D to be a large piece of any metal, E F G H the limit of the interior uniform æther and K a part of the metal at the ſuperficies A B. If this part of particle K be ſo little, that it reaches not to the limit E F, it is plain, that the æther at its center muſt be leſs rare, than if the particle were greater; for were it greater, its center would be further from the ſuperficies A B, that is, in a place, where the æther (by ſuppoſition) is rarer. The leſs the particle K therefore, the denſer the æther at its center, becauſe its center comes nearer to the edge A B, where the æther is denſer than within the limit E F G H. And if the particle were divided from the body, and removed to a diſtance from it, where the æther is ſtill denſer, the æther within it muſt proportionally grow denſer. If you conſider this, you may apprehend, how by diminiſhing the particle, the rarity of the æther within it will be diminiſhed, till between the denſity of the æther without, and the denſity of the æther within it, there be little difference; that is, till the cauſe be almoſt taken away, which ſhould keep this and other ſuch particles at a diſtance from one another. For that cauſe explained in the fourth and fifth ſuppoſitions, was the exceſs of denſity of the external æther above that of the internal. This may be the reaſon then, why the ſmall particles of vapours eaſily come together, and are reduced back into water, unleſs the heat, which keeps them in agitation, be ſo great as to diſſipate them as faſt as they come together: but the groſſer particles of exhalations raiſed by fermentation keep their aërial form more obſtinately, becauſe the æther within them is rarer.

“ NOR does the ſize only, but the denſity of the particles alſo, conduce to the permanency of aërial ſubſtances. For the exceſs of denſity of the æther without ſuch particles above that of the æther within them is ſtill greater. Which has made me ſometimes think, that the true permanent air may be of a metallic original; the particles of no ſubſtances being more denſe than thoſe of metals. This, I think, is alſo favoured by experience, for I remember I once read in the Philoſophical Tranſactions, how M. *Huygens* at *Paris* found, that the air made by diſſolving ſalt of tartar would in two or three days time condenſe and fall down again, but the air made by diſſolving a metal continued without condenſing or relenting in the leaſt. If you conſider then, how by the continual fermentations made in the bowels of the earth there are aërial ſubſtances raiſed out of all kinds of bodies, all which together make the atmoſphere, and that of all theſe the metallic are the moſt permanent, you will not, perhaps, think it abſurd, that the moſt permanent part of the atmoſphere, which is the true air, ſhould be conſtituted of theſe; eſpecially ſince they are the heavieſt of all other, and ſo muſt ſubſide to the lower parts of the atmoſphere, and float upon the ſurface of the earth, and buoy up the lighter exhalation and vapours to float in greateſt plenty above them. Thus, I ſay, it ought to be with the metallic exhalations raiſed in the bowels of the

“ earth by the action of acid menſtruums, and thus it is with the true permanent
“ air; for this as in reaſon it ought to be eſteemed the moſt ponderous part of the
“ atmoſphere, becauſe the loweſt, ſo it betrays its ponderoſity, by making vapours
“ aſcend readily in it, by ſuſtaining miſts and clouds of ſnow, and by buoying up
“ groſs and ponderous ſmoke. The air alſo is the moſt groſs unactive part of the
“ atmoſphere, affording living things no nouriſhment, if deprived of the more
“ tender exhalations and ſpirits, that float in it: and what more unactive and remote
“ from nouriſhment than metallic bodies?

“ I SHALL ſet down one conjecture more, which came into my mind now as I was
“ writing this letter. It is about the cauſe of gravity. For this end I will ſuppoſe
“ æther to conſiſt of parts differing from one another in ſubtilty by indefinite degrees:
“ that in the pores of bodies there is leſs of the groſſer æther, in proportion to the
“ finer, than in open ſpaces; and conſequently, that in the great body of the earth
“ there is much leſs of the groſſer æther, in proportion to the finer, than in the regions
“ of the air: and that yet the groſſer æther in the air affects the upper regions
“ of the earth, and the finer æther in the earth the lower regions of the air, in ſuch
“ a manner, that from the top of the air to the ſurface of the earth, and again from
“ the ſurface of the earth to the center thereof, the æther is inſenſibly finer and finer.
“ Imagine now any body ſuſpended in the air, or lying on the earth: and the æther
“ being by the hypotheſis groſſer in the pores, which are in the upper parts of the
“ body, than in thoſe which are in its lower parts, and that groſſer æther being leſs
“ apt to be lodged in thoſe pores, than the finer æther below, it will endeavour to
“ get out and give way to the finer æther below, which cannot be without the bodies
“ deſcending to make room above for it to go out into.

“ FROM this ſuppoſed gradual ſubtilty of the parts of æther ſome things above
“ might be further illuſtrated, and made more intelligible; but by what has been
“ ſaid, you will eaſily diſcern, whether in theſe conjectures there be any degree of
“ probability, which is all I aim at. For my own part, I have ſo little fancy to
“ things of this nature, that had not your encouragement moved me to it, I ſhould
“ never, I think, have thus far ſet pen to paper about them. What is amiſs therefore,
“ I hope, you will the more eaſily pardon in

“ Your moſt humble ſervant,

Cambridge, Feb. 28, 1678-9.

“ and honourer,

“ ISAAC NEWTON.”

THIS letter of our incomparable *Newton* may perhaps receive ſome illuſtration from another[y], which he wrote a few years before to Mr. *Oldenburg*, and was as follows:

“ SIR,

“ I RECEIVED both yours, and thank you for your care in diſpoſing thoſe
“ things between me and Mr. *Linus*. I ſuppoſe his friends cannot blame you at
“ all for printing his firſt letter, it being written, I believe, for that end, and they

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[y] In the poſſeſſion of *William Jones*, Eſq.

never complaining of the printing of that, but of the not printing that, which followed, which I take myſelf to have been *per accidens* the occaſion of, by refuſing to anſwer him. And though I think I may truly ſay, I was very little concerned about it, yet I muſt look upon it as the reſult of your kindneſs to me, that you was unwilling to print it without an anſwer.

“As to the paper of Obſervations, which you move in the name of the Society to have printed, I cannot but return them my hearty thanks for the kind acceptance they meet with there, and know not how to deny any thing, which they deſire ſhould be done. Only I think it will be beſt to ſuſpend the printing of them for a while, becauſe I have ſome thoughts of writing ſuch another ſet of Obſervations for determining the manner of the productions of colours by the priſm, which, if done at all, ought to precede that now in your hands, and will do beſt to be joined with it. But this I cannot do preſently, by reaſon of ſome incumbrances lately put upon me by ſome friends, and ſome other buſineſs of my own, which at preſent almoſt take up my time and thoughts.

“The additions, that I intended, I think I muſt, after putting you to ſo long expectations, diſappoint you in; for it puzzles me how to connect them with what I ſent you; and if I had thoſe papers, yet I doubt the things I intended will not come in ſo freely as I thought they might have done. I could ſend them deſcribed without dependance on thoſe papers; but I fear I have already troubled your Society and yourſelf too much with my ſcribbling, and ſo ſuppoſe it may do better to defer them till another ſeaſon. I have therefore at preſent only ſent you two or three alterations, though not of ſo great moment, that I need have ſtaid you for them; and they are theſe:

“Where I ſay, that *the frame of nature may be nothing but æther condenſed by a fermental principle*, inſtead of theſe words write, that it may be nothing but various contextures of ſome certain ætherial ſpirits or vapours condenſed, as it were, by precipitation, much after the manner, that vapours are condenſed into water, or exhalations into groſſer ſubſtances, though not ſo eaſily condenſable; and after condenſation wrought into various forms, at firſt by the immediate hand of the Creator, and ever ſince by the power of nature, who by virtue of the command, *Increaſe and multiply*, became a complete imitator of the copies ſet her by the Protoplaſt. Thus perhaps may all things be originated from æther, *&c.*

“A little after, when I ſay, the ætherial ſpirit may be *condenſed in fermenting or burning bodies, or otherwiſe inſpiſſated in the pores of the earth to a tender matter, which may be, as it were, the* ſuccus nutritius *of the earth, or primary ſubſtance, out of which things generable grow:* inſtead of this you may write, that that ſpirit may be condenſed in fermenting or burning bodies, or otherwiſe coagulated in the pores of the earth and water into ſome kind of humid active matter, for the continual uſes of nature, adhering to the ſides of thoſe pores after the manner that vapours condenſe on the ſides of a veſſel.

“In the ſame paragraph there is, I think, a parentheſis, in which I mention volatile ſalt-petre. Pray ſtrike out that parentheſis, leſt it ſhould give offence to ſomebody.

“Also where I relate the experiment of little papers made to move variouſly with a glaſs rubbed, I would have all that ſtruck out, which follows about trying the experiment with leaf-gold.

“Sir, I am interrupted by a viſit, and ſo muſt in haſte break off.

“Yours,

Jan. 25, 1675-6. “Isaac Newton.”

But to return to Mr. *Boyle*; in the year 1680, he gave the world the following tracts, *viz. The Aerial Noctiluca: or ſome new Phænomena, and a proceſs of a factitious ſelf-ſhining ſubſtance*; London, in 8vo. *A new Lamp*, printed in Mr. *Hooke's Philoſophical Collections*, No. II. p. 33. and *Divers Experiments and Notes about the producibleneſs of chemical Principles*, ſubjoined to the ſecond edition of his *Sceptical Chemiſt*, at *Oxford*, 1680, in 8vo.

The Royal Society, of which he had been ſo long one of the greateſt ornaments, now thought proper at their annual election on St. *Andrew's* day, November 30, this year, to chooſe him for their preſident. But after a mature conſideration he excuſed himſelf from accepting that poſt, for reaſons which ſhew his extreme tenderneſs and delicacy in all matters of conſcience, and were repreſented by him in the following letter to Mr. *Hooke:*

"SIR, *Pall-Mall*, Dec. 18, 1680.

" THOUGH ſince I laſt ſaw you, I met with a lawyer, who has been a member
" of ſeveral parliaments, and found him of the ſame opinion with my council
" in reference to the obligation to take the teſt and oaths you and I diſcourſed of;
" yet not content with this, and hearing that an acquaintance of mine was come
" to town, whoſe eminent ſkill in the law had made him a judge, if he himſelf had
" not declined to be one, I deſired his advice (which becauſe he would not ſend me
" till he had peruſed the Society's charter, I received not till late laſt night) and by
" it I found, that he concurred in opinion with the two lawyers already mentioned,
" and would not have me venture upon the ſuppoſition of my being unconcerned
" in an act of parliament, to whoſe breach ſuch heavy penalties are annexed. His
" reaſons I have not now time to tell you, but they are of ſuch weight with me,
" who have a great (and perhaps peculiar) tenderneſs in point of oaths, that I muſt
" humbly beg the Royal Society to proceed to a new election, and do ſo eaſy a
" thing, as among ſo many worthy perſons, that compoſe that illuſtrious company,
" to chooſe a preſident, that may be better qualified than I for ſo weighty an em-
" ployment. You will oblige me alſo to aſſure them, that though I cannot now
" receive the great honour they were pleaſed to deſign me, yet I have as much ſenſe
" of it, as if I actually enjoyed all the advantages belonging to it. And accord-
" ingly, though I muſt not ſerve them in the honourable capacity they were pleaſed to
" think, of for me, yet I hope, that God aſſiſting, I ſhall not be an uſeleſs Member
" of that learned Body, but ſhall manifeſt in that capacity both my zeal for their
" work, and my ſenſe of their favours. This you will oblige me to repreſent in ſuch
" a way, as may perſuade the Virtuoſi, that you will diſcourſe with, how concerned
" I am to retain the favourable opinion of perſons, that have ſo great a ſhare in his
" eſteem, who ſhall reckon your good offices on ſo important an occaſion among the
" welcomeſt favours you can ever do,

" SIR,

" your moſt affectionate friend and humble ſervant,

" Robert Boyle."

" Superſcribed,
" *Theſe for my much reſpected friend Mr.*
" Robert Hooke, *profeſſor of mathe-*
" *matics at* Greſham College."

About this time Dr. *Burnet* being employed in compiling his admirable *Hiſtory of the Reformation*, Mr. *Boyle* contributed very largely to the expence of publiſhing it; which is acknowledged by the doctor in his *Preface* to the *ſecond volume*, in theſe words: “The noble Mr. *Boyle*, as he employs both his time and wealth for the good of mankind, (for which he conſiders himſelf as chiefly born, and which he has promoted not only in his own excellent writings, that have made him ſo famous over all the world, but in many other deſigns that have been chiefly carried on at his coſt) ſo hath he renewed his kindneſs to me in largeſſes ſuitable to ſo great a mind.”

In 1681 Mr. *Boyle* publiſhed his *Diſcourſe of things above Reaſon; inquiring, whether a Philoſopher ſhould admit there are any ſuch? To which are annexed by the Publiſher, for the Affinity of the Subject, ſome Advices about judging of things ſaid to tranſcend Reaſon:* London, in 8vo. as he did the year following his *New Experiments and Obſervations made upon the icy Noctiluca: to which is added, A chemical Paradox grounded upon new Experiments, making it probable, that chemical Principles are tranſmutable; ſo that out of one of them others may be produced:* London, in 8vo. and *A Continuation of new Experiments phyſico-mechanical touching the Spring and Weight of the Air and their Effects. The ſecond part; wherein are contained divers Experiments made both in compreſſed and alſo in factitious Air, about Fire, Animals,* &c. *Together with a Deſcription of the Engines, wherein they were made:* London, in 8vo.

It was probably in the beginning of the year 1681, that he wrote a letter to Mr. *John Eliot* of *New England*, who was one of the firſt who preached the goſpel among the Indians of that part of *America*, having learned their language to great perfection, and tranſlated into it the Bible [z] and ſeveral books of piety. Mr. *Boyle*'s letter ſeems to be in anſwer to one from Mr. *Eliot*, dated Nov. 4, 1680 [a]; and as it is a ſtrong evidence of his zeal againſt perſecution on account of religious opinions, it deſerves a place here.

“SIR,

“I AM very glad to find, by the favour of your very kind letter, that God is pleaſed to continue you ſtill an active and an uſeful inſtrument in the propagation of the Goſpel of his Son among the poor Indians, whoſe having been ſo true to Chriſtianity, and ſerviceable to the Engliſh intereſt, may well prove matter of rejoicing both to you and us. That little, which I have contributed to their good, deſerves not ſo advantageous a mention, as your letter makes of it; and duties of that kind have ſuch recompences apportioned to them by God, that the performers need not ſeek them from the acknowledgments of men. I am obliged to you for what I perceive you have done for Mr. *Daniel*, whom I look upon both as an ingenious man, and a friend to *New England*, as well by his good offices here, as by the moderation, to which he endeavours to perſuade his friends there, which laſt clauſe I the rather employ, becauſe of late I have, to my trouble, heard the government of the Maſſachuſets ſharply cenſured for their great ſeverity to ſome diſſenters, who, contrary to order, had convened at a meeting-houſe to worſhip God. This ſevere proceeding ſeems to be the more ſtrange, and the leſs defenſible in thoſe who having left their native country, and croſſed the vaſt ocean to

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[z] It was firſt printed in *Cambridge* in *New England* in 1663, in 4to.

[a] Inſerted in the *Appendix* to this *Life*.

"ſettle in a wilderneſs, that they may there enjoy the liberty of worſhipping God "according to their own conſcience, ſeem to be more engaged than other men to "allow their brethren a ſhare in what they thought was ſo much all good men's due. "And, indeed, though perſecution for innocent, though perhaps erroneous opinions, "taken up for conſcience ſake, were not unſuitable to the equity and gentleneſs of "the Goſpel; yet many of your friends here think it would be a very improper "courſe to be taken by you at this time, and fear, that if your rigorous proceedings "againſt diſſenters ſhould be talked of here (as if you quickly forbear them not, "they will be) it would open men's mouths againſt your government, and furniſh "your enemies with objections, that your friends would not be able to anſwer; and "beſides may be of very bad conſequence to that ſort of men here, who do moſt "ſymbolize with you in point of opinion and worſhip. You will eaſily believe, that "I, who am never like to viſit your colony, have no private ends of my own in "what I have now written; and therefore I hope you will take it, as it is meant, for "a friendly (and perhaps not unſeaſonable) admonition, the deſpiſing of which may "probably be more prejudicial to your colony, than many among you ſeem to be "aware of. Our worthy friend, alderman *Aſhurſt* (though now, thanks be to God, "in a more hopeful condition) was on Wedneſday laſt ſo ill, that the corporation "could not meet at his houſe; and the preſence of that good man was much miſſed "amongſt us, and particularly in reference to your deſire of having the Old Teſtament "reprinted in the Indian language. In his abſence I read to the company that "part of your letter to me, which concerns that affair, and the buſineſs was diſcourſed "of among us; but in regard we have had no letters from the commiſſioners "about it, and that the court thought they might hear further before the New "Teſtament and Pſalms would be printed off; they did not think fit to determine "any more about that buſineſs, till they ſhould have a particular account of the "progreſs and expence of the work already begun; by which account they expect "to be aſſiſted to take further meaſures. This I acquaint you with by order of the "company, divers of whoſe members on this occaſion manifeſted the kindneſs and "eſteem, which they had for Mr. *Eliot*. I briefly acquainted them alſo with what "you wrote to me about the * * * people, whoſe language is almoſt the ſame with "that of the Maſſachuſets. If any further diſcoveries be made of them, you will "oblige me to impart it to him, that in that and all your other endeavours to enlarge "the kingdom of Chriſt, wiſhes you moſt proſperous ſucceſs, and is unfeignedly,

"SIR,

"your moſt affectionate and humble ſervant."

IN 1683 he wrote *A Letter to the learned Dr.* John Beale, *F. R. S. concerning freſh Water made out of Sea-Water, printed at the deſire of the Patentees*, in a tract, intitled, *Salt Water ſweetened; or a true account of the great advantages of this new invention both by ſea and land: together with a full and ſatisfactory anſwer to all apparent difficulties. Alſo the approbation of the College of Phyſicians:* London, 1683, in 8vo. This piece was written by *R. Fitz-Gerald*, Eſq; who in his dedication to the King obſerves, "the experiment was in a great degree owing to the eminent Mr. *Boyle*; and indeed well "worthy ſo ingenious a promoter, being ſo much more the favourite of his happy "genius, as it is univerſally uſeful to mankind."

THE year following produced his *Memoirs of the natural Hiſtory of human Blood, eſpecially the Spirit of that Liquor*; *with an Appendix*: printed at *London*, in 8vo, and tranſlated into Latin; and his *Experiments and Conſiderations about the Poroſity of Bodies, in two Eſſays*: printed likewiſe at *London*, in 8vo. The firſt eſſay treats of the porouſneſs of *animal bodies*, and the ſecond of that of *ſolar bodies*. A Latin tranſlation of this work was alſo publiſhed there in 8vo, the ſame year, under the title *Tentamen Porologicum*.

HIS writings grew now ſo numerous, that Dr. *Ralph Cudworth*, the excellent author of *the true intellectual Syſtem of the Univerſe*, recommended to him in a letter dated October 16, 1684, the procuring of all his works to be tranſlated into Latin, and publiſhed together. "Then, *adds he*, what you ſhall ſuperadd, will be eaſily collected and added afterwards. And I pray God continue your life and health, that you may ſtill inrich the world with more. The writers of hypotheſes in natural philoſophy will be confuting one another a long time, before the world will ever agree, if ever it do. But your pieces of natural hiſtory are unconfutable, and will afford the beſt grounds to build hypotheſes upon. You have much outdone Sir *Francis Bacon* in your natural experiments; and you have not inſinuated any thing, as he is thought to have done, tending to irreligion, but the contrary."

IN 1685, he gave the world his *Short Memoirs for the natural experimental Hiſtory of mineral Waters, addreſſed by way of Letter to a Friend*: printed at *London*, in 8vo. In this book he gives us heads for the natural hiſtory of thoſe waters, with a great many curious experiments. With regard to the methods of trying them, he gives the following directions: 1. To obſerve the changes of the colours made by tinctures in a good light, in order to diſtinguiſh what mineral tinctures they are impregnated with. 2. To vary the ſhades of colours made by mineral waters, either by dropping ſuch waters upon paper, whoſe pores are ſaturated with powder of vitriol, or tinged with a decoction of logwood. Several variations alſo of colours may alſo be made by dropping either medicinal liquors into mineral waters, or an infuſion of galls either before or after. 3. He recommends for ſuch experiments the aſtringent parts of plants or animals, or eſpecially mineral ſubſtances.

THE next work, which he publiſhed that year, was, *An Eſſay of the great Effects of even languid and unheeded motion: whereunto is annexed, An experimental Diſcourſe of ſome little obſerved Cauſes of the Inſalubrity and Salubrity of the Air and its Effects*: *London*, in 8vo, and reprinted there in the ſame form in 1690. In this *eſſay* he proves, by ſeveral inſtances, 1. The great efficacy of celerity in bodies very ſmall, eſpecially when the ſpace which they move through is but very ſmall, as in lightning. 2. That the inſenſible motion of ſo ſoft bodies as fluids may have a ſenſible effect upon ſolid bodies, as in ſounds, when they ſhake the windows in houſes, *&c.* at a conſiderable diſtance. 3. That the number of the inſenſible parts of matter put into motion, enable them to perform ſeveral things. 4. That local motion may be propagated through ſeveral mediums, and even ſolid bodies. 5. That the effects of particular modifications of the inviſible motions of fluids on animal bodies to be diſpoſed to be worked upon by them, are very conſiderable. Thus a particular note of a muſical inſtrument, hath a peculiar effect upon particular animals. 6. That the effects of fluids upon inanimate bodies, by a particular texture and modification of the agent and the patient, are alſo conſiderable. 7. That ſome bodies are thought to have their parts abſolutely at reſt, when they are only in a forced ſtate, as of tenſion, compreſſion, *&c.* 8. That one principal reaſon why ſuch motions, as we ſpeak of, are overlooked, is, that we ſcarce ever take notice but of thoſe motions of ſolid bodies, wherein in one whole body drives away another, or at leaſt viſibly hits againſt it; whilſt many effects proceed from the inteſtine motions produced by the external agent in and among the parts of the ſame body.

He publiſhed alſo, the ſame year, *An hiſtorical Account of a ſtrangely ſelf-moving Liquor*; printed in the *Philoſophical Tranſactions*, No. CLXXVI. p. 1188; and a diſcourſe, intitled, *Of the Reconcileableneſs of ſpecific Medicines to the corpuſcular Philoſophy: to which is annexed, A Diſcourſe about the Advantages of the Uſe of ſimple Medicines:* printed at *London*, in 8vo, and tranſlated into Latin, in which language it was publiſhed there the year following in 8vo. In this book he obſerves, that there are three ſorts of qualities mentioned in the writings of phyſicians, under the notion of ſpecific virtues; for by ſome of them a medicine is ſaid to have a ſpecific quality, becauſe it is eminently and peculiarly beneficial to a particular part of the body, as the heart, the brain, the eyes, *&c.* Others ſay, that a ſpecific remedy attracts and evacuates ſome determinate humour, as choler, phlegm, *&c.* But the moſt uſual account upon which a medicine is ſaid to be ſpecific, is, that by ſome hidden property it cures this or that particular diſeaſe, as a pleuriſy, an aſthma, the cholic, a dropſy, *&c.* This being the common ſenſe in which the word is employed, Mr. *Boyle* makes uſe of it in that ſenſe. He then urges ſeveral arguments for the exiſtence of ſpecific remedies; and enquires, whether the notion of them may be accommodated to the mechanical philoſophy; and lays down the ſix following propoſitions: 1. A ſpecific medicine may cure by diſcuſſion, or reſolving the morbific matter, and thereby fitting it for expulſion through the greater outlets of the body and cuticular pores. 2. It may ſubdue the too great proportion of acid or other luxuriant particles in the maſs of blood, and prevent their coagulating or other pernicious effects. 3. It may prove ſerviceable by precipitating the peccant matter out of the blood or other fluid of the body. 4. It may peculiarly ſtrengthen and cheriſh the heart, and by that means, or otherwiſe, relieve the part affected. 5. It may have its effect, by producing in the maſs of blood a diſpoſition to enable nature, by correcting, expelling, *&c.* to ſurmount the cauſe of the diſeaſe. 6. It may unite its particles with thoſe of the peccant matter, and thereby conſtitute ſome different ſubſtance to be eaſily thrown off, or ſafely continued in the body. He then encourages the uſe of *ſimple medicines*, from the advantage of foreſeeing their effects, and their being more ſafe and commodious for exhibition, eaſily procurable, and likely to improve the knowledge of the *Materia Medica.* He obſerves likewiſe, that chemical remedies ſhould be ſimple; and enquires, whether ſimple remedies may cure complex diſtempers.

Besides theſe philoſophical tracts, he gave the world likewiſe, the ſame year, an excellent theological one, *Of the high Veneration Man's Intellect owes to God, peculiarly for his Wiſdom and Power:* printed at *London*, in 8vo. and tranſlated likewiſe into Latin. In the advertiſement prefixed to this tract, we are informed, that "the abrupt "beginning of it will not be wondered at, when it is declared, that the whole excur"ſion is to be looked upon as a fragment of a diſcourſe, from which, for certain rea"ſons, it has been ſeparated in its preſent form; in which it ought to paſs for a "rough draught; the nobleneſs, ſublimity and ſacredneſs of the ſubject not allow"ing the author to preſume, that the firſt thoughts he committed to paper about it, "might be for good and all parted with by him, till he ſhould have heedfully reviſed "and corrected them, and left them as few faults, as the diſproportion of ſo vaſt and "ſublime a ſubject to his ſlender abilities would permit."

In 1685-6, he publiſhed his *Free Enquiry into the vulgarly received Notion of Nature; made in an Eſſay addreſſed to a Friend:* printed at *London*, in 8vo, and tranſlated into Latin, and printed there in 1687 in 12mo. In this diſcourſe he obſerves, that this notion is prejudicial to religion and philoſophy; and he advances a new notion of it, diſtinguiſhing between the *univerſal* and the *particular* nature of things. With regard to *univerſal nature*, he tells us, that nature is the aggregate of the bodies, which make up the world in its preſent ſtate, conſidered as a principle, by virtue whereof they act and ſuffer, according to the laws of motion preſcribed by the author of things. And that this makes way for the other ſubordinate notion, ſince the *particular nature* of an individual conſiſts in the *general nature* applied to a diſtinct portion of the univerſe; or ſuppoſing that place, as it is, in a world framed by God, like ours, it muſt be a convention of the mechanical properties (ſuch as magnitude, figure, order, ſituation and local motion) of parts convenient and ſufficient to conſtitute or intitle to its particular ſpecies or denomination the particular body which they make up; the concourſe of all theſe being conſidered as the principle of motion, reſt, and changes in the body.

In June the ſame year, Dr. *Burnet*, afterwards biſhop of *Saliſbury*, tranſmitted to him from the *Hague* the manuſcript account of his travels[a], which he had drawn up in the form of letters addreſſed to Mr. *Boyle*; who in his anſwer to the doctor, dated the 14th of that month, obſerves, that he was glad to find, that "all men do not "travel, as moſt do, to obſerve buildings, and gardens, and modes, and other amuſe-"ments of a ſuperficial and almoſt inſignificant curioſity; for your judicious remarks "and reflections, *ſays he*, may not a little improve both a ſtateſman, a critic, and a "divine, as well as they will make the writer paſs for all three."

The next work of our great author which ſaw the light, was, *The martyrdom of Theodora and Didymus*, drawn up in his youth, and printed at *London*, 1687, in 8vo; and the year following he publiſhed *Receipts ſent to a Friend in America: London*, in 12mo; which edition contains only the firſt five decads of *choice remedies*, and hath a preface before it, which was omitted in the ſecond edition in 1692, containing the ſecond five decads. He publiſhed likewiſe, in 1688, *A Diſquiſition about the final Cauſes of natural things; wherein it is enquired whether, and (if at all) with what caution a Naturaliſt ſhould admit them. To which are ſubjoined, by way of Appendix, ſome uncommon Obſervations about vitiated Sight: London*, in 8vo. In this *Diſquiſition* he examines, whether theſe final cauſes are knowable by men; and obſerves, that they ſignify, 1. Some grand and general ends of the univerſe, ſuch as exerciſing and diſplaying the Creator's wiſdom, the communication of his goodneſs, and the admiration and thanks due to him from his intelligent creatures. 2. In a more reſtrained ſenſe, the ends deſigned in the number, fabric, ſituation, and motion of great maſſes of matter which make large parts of the world; ſince it is very probable that theſe bodies, ſuch as the ſun, moon, fixed ſtars, and the terraqueous globe, were ſo framed and placed, as not only to perſevere in their own preſent ſtate, but alſo to conduce to the univerſal ends of the creation, and the good of the whole, whereof they are conſiderable parts. Upon which accounts theſe ends may be called *coſmical* or *ſyſtematical*, as they regard the ſymmetry of the great ſyſtem of the world. 3. Ends, which more peculiarly concern the parts of animals, and perhaps plants too; or thoſe to which the peculiar parts of animals are deſtined for the welfare of the whole creature, conſidered as an intire and diſtinct ſyſtem of organized parts, deſigned to preſerve himſelf, and propagate his ſpecies upon that ſtage on which his ſtructure and circumſtances determine him to act his part; which ends Mr. *Boyle* calls *animal ends*. 4. The ſame expreſſion may ſignify another ſort of ends, which, becauſe they relate particularly to man, may be called *human ends*, and are thoſe aimed at by nature, where ſhe is ſaid to frame animals, vegetables, *&c.* for the uſe of man. And theſe ends may be further diſtinguiſhed into *mental* and *corporeal*, not only as man is an animal, framed, like others, for his own preſervation, and the propagation of his ſpecies; but alſo as he is made to have dominion over other animals and works of nature, and fitted to make them ſubſervient to his purpoſes. This diſtinction being thus ſettled, Mr. *Boyle* declares his diſſent, as well from the vulgar notion of final cauſes, which allows of none but thoſe, which are above called human ones, as from theirs, who wholly reject them all. He obſerves, that chance is an imaginary being; that revelation allows us to ſpeak more poſitively of final cauſes than natural philoſophy; that it is often allowable, from the manifeſt and appoſite uſes of the parts of animal bodies to collect ſome of the particular ends for which the Creator deſigned them; and in ſome caſes, from the known nature and ſtructure of the parts, to draw probable conjectures about the particular offices of them; and that it is rational from the manifeſt fitneſs of ſome things to *coſmical* or *animal ends* to infer, that they were ordained to them by an intelligent agent; that we ought not to be haſty in concluding upon the particular uſe of a thing, or the motive which induced the author of nature to frame it in a peculiar manner; and that the naturaliſt ſhould not ſuffer the ſearch or diſcovery of final cauſes to make him undervalue or neglect the enquiry after their efficients. In the *Obſervations concerning vitiated Sight* he obſerves, that white objects are moſt viſible to a dim ſight; that both eyes are concerned in real viſion; that the parts of the eye are capable of great dilatation without prejudice; and that colours appear different to diſordered organs.

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[a] See Vol. VI. p. 625, 626.

He publiſhed likewiſe in May the ſame year 1688, at *London*, in two pages in folio, *An advertiſement about the loſs of many of his writings, addreſſed to* J. W. *to be communicated to thoſe of his friends that are virtuoſi*; *which may ſerve as a kind of preface to moſt of his mutilated and unfiniſhed writings.* And to the ſame purpoſe I find the following papers of his, which ſhew in what manner he had been treated by Plagiaries.

" In the month of May 1688, I thought myſelf obliged to give notice to the pub-
" lic, that I had, partly by ſome men's fraud, and partly by miſchance, loſt ſo many
" of my eſſays and other tracts, and had ſo many of my remaining papers enda-
" maged by corroſive liquors, that the curious were not thenceforth to expect from
" me any thing but imperfect and mutilated. And yet ſince that time all my care
" and circumſpection has not hindered me from loſing ſix centuries of matters of fact
" in one parcel, beſides ſo many other papers of leſſer bulk, that I am reduced to
" remind the curious of my former advertiſement; not for any pleaſure I take in
" complaining, or troubling others with my misfortunes, but to render a reaſon of
" the courſe I am by theſe miſadventures driven to, though otherwiſe I ſhould not
" think it eligible. This is to ſecure the remaining part of my writings, eſpecially
" thoſe that contain moſt matters of fact, by ſending them maimed and unfiniſhed, as
" they come to hand, to the preſs."

" Being wont, when I firſt turned a writer, to ſet down, as well as others uſe to
" do, my thoughts and obſervations on papers bound up into books, I was quite diſ-
" couraged from that practice, by the loſſes I made at ſeveral times of manuſcripts,
" which I ſtrongly ſuſpected to have been ſurreptitiouſly conveyed away by ſome, that
" though they expected to find valuabler things than I ſuppoſed they met with, would

not probably have ſtolen away thoſe papers, if their bulk had not been a main temptation to the theft.

“ WHEREFORE I afterwards reſolved to write in ſingle ſheets, and other looſe papers, that the ignorance of the coherence might keep men from thinking them worth ſtealing. And though I could not, by ſo doing, prevent the loſing ſometimes a paper or two by chance, or other men's fraud, yet I thought ſuch inconveniencies (which I could ſometimes eaſily repair out of my memory) much inferior to thoſe of loſing an entire diſcourſe, or a whole diſcourſe at a time. Now that it may ſeem the leſs ſtrange, that the particulars in the following manuſcript appear to be huddled together, without any either method or connection, I am to advertiſe, that when I had ſettled the number and titles of the ſubjects to be treated of in theſe papers, I found myſelf both invaded with a ſickneſs threatening enough, and likely to be hindered by other diſtracting avocations, from ſetting down the particulars belonging to the enſuing treatiſe, in ſuch an uninterrupted ſeries as I had propoſed to myſelf; which made me reſolve, rather than venture the loſs of my obſervations, to ſet down under each title or ſection thoſe that did more particularly belong unto it, without any other order than that wherein I lighted on them, contenting myſelf to leave competent blanks or intervals between the diſtinct obſervations, notions, *&c.* which courſe I pitch upon, partly, that if God ſhould be pleaſed to ſpare me life and leiſure, I might be able to fill them up in the requiſite connexions, tranſitions, *&c.* partly that I might preſerve the materials, ſome of which do not perhaps deſerve to periſh, but gave me ſome hopes, that though I myſelf ſhould not complete with them the fabric I chiefly deſign them for, yet perhaps a ſkilful builder may think fit to perform it; and that however they may at laſt prove uſeful for other purpoſes.

“ THAT the author may not be diſcouraged from continuing to impart clearly and freely his experiments and notions to the commonwealth of letters, I think it neceſſary to advertiſe equitable readers, that he has been the worſe dealt with by ſeveral writers, upon the very account of that candour and faithfulneſs he has exerciſed in delivering matters of fact. For whereas when experiments and obſervations are related by men whoſe faithfulneſs is dubious, the more cautious ſort of plagiaries think themſelves obliged to mention the names of their authors, leſt an experiment not proving true, its falſity ſhould be (as it juſtly may be) imputed to them, they think they may ſo ſafely rely on the truth of what our author relates, that their reputation runs no venture in making any experiment that he delivers, paſs for their own.

“ THIS hath emboldened ſeveral writers, both formerly and of late, to uſurp from him a great many things whereof they were not the authors; ſometimes tranſcribing this or that particular out of his book into theirs, and ſometimes transferring whole ſets of experiments, if not reaſonings too, perhaps ſomewhat abridged or otherwiſe diſguiſed. And this hath been done by ſome of them, without ſo much as naming the true author, and ſometimes naming him indeed as it were incidentally, and peradventure reflectingly, for ſome inconſiderable part of what they took from him. Of theſe ſeveral ſorts of plagiaries, it would not be difficult to give particular inſtances: it will be done, if it be thought fit and deſired.

“ IT is not unknown to a great part of the curious, that our author hath given eminent proofs of his readineſs to acknowledge thoſe whoſe writings he tranſcribes into

“ his, inſomuch, that ſome new experiments, that were never in the preſs till he ſent them thither, and ſo might eaſily have been adopted by him, were declared by him not to be his, though he yet knew not whoſe they were. It will therefore become ingenious readers, who would not diſcourage his candor and communicativeneſs, to think, when they meet with the ſame experiment in his writings and in another's, that a perſon that has been ſo converſant with nature, and uſes a comprehenſive method in examining the ſubjects he ſolemnly treats of, may be at leaſt as likely, as moſt others have been, the author of what he delivers upon his own knowledge. And particularly it is deſired and hoped, that the equitable reader will ſo far concern himſelf in the reputation of a perſon, who hazarded it only to ſerve the public, that he will take notice of the time when our author's books came abroad, and that wherein the firſt edition of the books, containing the ſame or the like things with his, was publiſhed, it having been the ſubtile practice of ſeveral plagiaries, as well as *Claudius Berigardus*, to aſſign to the ſecond or other ſubſequent editions of their books the ſame date with the firſt impreſſion, and ſtealing into theſe after-editions many paſſages, both of other authors and divers other experimental writers, and particularly divers members of the Royal Society, as if they were as ancient as the firſt edition of their books, wherein a due collation will diſcover them not to be extant.

“ THIS will, it is hoped, appear but a reaſonable requeſt, ſince the thing deſired is neceſſary to keep a man, that has long faithfully ſerved the commonwealth of learning, from being thought to have taken from others ſuch things as others have really taken from him.

“ THOUGH many learned and ingenious foreign writers, eſpecially in *Germany* and *Italy*, have thought fit, when they made uſe of our author's experiments and obſervations, to do him juſtice, and ſometimes even obligingly; yet divers others have been far from imitating their laudable example. For they have made uſe, not only of an experiment or two of his here and there, but ſome of them of whole ſets of experiments; and others have made bold with the hiſtorical part of whole chapters out of his writings, without taking notice of them, or perhaps ſo much as naming him. This way of proceeding offended many, and particularly the late learned ſecretary of the Royal Society, Mr. *H. O.* who publicly complained of it in general terms, and ſhewed the prejudice ſuch plagiaries muſt naturally do to the commonwealth of learning, eſpecially by diſcouraging the induſtry of thoſe many that do not undervalue fame, and by encouraging lazineſs in thoſe numerous pretenders to the new philoſophy, who will never take much pains to promote experimental knowledge, whilſt they find it far eaſier to uſurp experiments than to make them, and think they may ſecurely, by turning plagiaries, paſs for philoſophers. But though our author's ſtrong inclination to ſerve the public, and gratify the curious, has hitherto enabled him to ſurmount the abovementioned diſcouragement; yet thoſe virtuoſi, that are earneſtly deſirous, that the more hiſtorical parts of his remaining writings may not be expoſed to the accidents by which ſo many of their companions have been loſt already, nor the curious kept from things that may be of uſe or of delight to them, ſo long as till they may be publiſhed with leſs danger of being uſurped by foreigners; they have undertaken, for prevention of this danger, both to haſten the tranſlation of the writings, that will, God granting him life and health, from time to time come abroad; and whether he be alive or not, to have now and then a liſt drawn up and printed, wherein the particular experiments

“ that may have been taken from Mr. *Boyle*'s writings without owning him for them, will be mentioned, though without any ſevere reflections upon the writers. This courſe is on this occaſion the rather thought fit, not only that it may take off a diſcouragement which might have too much influence upon another than Mr. *B.* but becauſe in a time where ſo many falſe, obſcure, or imperfectly delivered experiments, eſpecially chemical ones, fly abroad, it cannot be indifferent to a wary reader, from what hand he receives a matter of fact which one writer may deliver, not only much more faithfully and perſpicuouſly, but (what in ſome ſorts of nice experiments is very conſiderable) with greater ſkill than another.”

THE decay of Mr. *Boyle*'s health began now to interrupt his communications to the Royal Society, as he obſerves himſelf in a letter to monſieur *Le Clerc*, dated May 30, 1689[b]; and this, added to the ill ſituation of his affairs in *Ireland* upon the revolution, obliged him to reſign his poſt of governor of the corporation for propagating the Goſpel in *New England*, &c. which he did by the following letter to that corporation.

“ Honoured Gentlemen, **Aug. 22, 1689, *Pall-Mall.***

“ AS it hath not been without great ſatisfaction, that I have for many years ſerved ſuch worthy perſons as I now write to, in promoting ſo excellent a work as the propagation of the Goſpel among ſavages, that were utter ſtrangers to it, and worſhipped the grand enemy to it; ſo it is not without much trouble that I am now obliged to reſign ſo honourable, and to me grateful, an employment. And, indeed, it is none of the leaſt uneaſy circumſtances of the almoſt continual ſicklineſs, that out of his juſtice, and, I hope, his goodneſs too, God has been pleaſed to exerciſe me with of late years, that my infirmities have diſabled me to perform the conditions that the late laws require of thoſe that would continue in that publick ſtation, wherein I counted it a great honour, and ſuch as deſerved my moſt humble thanks to God for it, that I was preferred to aſſiſt you ſo long in carrying on your pious and charitable work. But on the other ſide, it may very probably turn to the advantage of the honourable corporation, who, inſtead of a perſon that wants his health, and is for the preſent deprived of all his eſtate in *Ireland*, may make choice of a governor, vigorous, active, verſed in letting, ſetting, and other œconomical affairs, who, eſpecially if he be a ſingle man, may further your pious endeavours, and contribute to the welfare of your ſociety, not only by his counſel and direction, but with his purſe. I hope the neceſſity of changing an inſtrument, will not diſcourage you from chearfully proſecuting a deſign which you may juſtly expect that God, whoſe glory it aims at, will bleſs and graciouſly both proſper and reward. For my part, I have found among you ſo particular a regard, and ſo much kindneſs to me, during the long continuance of my relation to you, that gratitude obliges me, as well for your own ſake as for that of the work you are engaged in, to wiſh and pray that you may be directed in your choice, and your next governor may be as faithful and affectionate as your laſt, but much more capable and proſperous. And though I muſt ceaſe to ſerve you in my former ſtation, yet you will not find me more backward than formerly to ſerve you faithfully in my reduced capacity; and I hope you will do me the right

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[b] Vol. VI. p. 61.

" to believe, that to ſee the great and good work, you are purſuing, proſper in your " charitable hands, though I can have the honour to contribute but my good " wiſhes to it, will heartily rejoice,

" Honoured Gentlemen,

" Your moſt faithful and

" moſt humble ſervant."

IT was probably about this time, that he thought proper to publiſh the following advertiſement, in order to decline viſits on certain days, that he might have leiſure to finiſh ſome of his works, then lying by him.

" MR. *Boyle* finds himſelf obliged to intimate to thoſe of his friends and acquaint- " ance, that are wont to do him the honour and favour of viſiting him, 1. That he " has by ſome unlucky accidents (whereof he has given notice to the public) had " many of his writings corroded here and there, or otherwiſe ſo maimed, that with- " out he himſelf fill up the *lacunæ* out of his memory or invention, they will not be " intelligible. 2. That his age and ſickelineſs have for a good while admoniſhed him " to put his ſcattered, and partly defaced, writings into ſome kind of order, that " they may not remain quite uſeleſs. And, 3. that his ſkilful and friendly phyſi- " cian[z], ſeconded by Mr. *Boyle*'s beſt friends, has preſſingly adviſed him againſt " ſpeaking daily with ſo many perſons, as are wont to viſit him, repreſenting it as " that which cannot but much waſte his ſpirits, and by obliging him to ſit a great " deal too much for a perſon ſubject to the ſtone of the kidneys, and on ſeveral " other accounts impair his health, and diſable him for holding out long. And he " is alſo obliged further to intimate, that by theſe and other inducements he does at " length, though unwillingly, find himſelf reduced to deny himſelf part of the ſa- " tisfaction frequently brought him by the converſation of his friends and other " ingenious perſons, and to deſire to be excuſed from receiving viſits (unleſs upon " occaſions very extraordinary) two days in the week, namely on the forenoon of " Tueſdays and Fridays (both foreign poſt days) and on Wedneſdays and Saturdays " in the afternoon, that he may have ſome time, both to recruit his ſpirits, to range " his papers, and fill up the *lacunæ* of them, and to take ſome care of his affairs in " *Ireland*, which are very much diſordered, and have their face often changed by " the public calamities there."

HE ordered likewiſe a board to be placed over his door, with an inſcription ſignifying when he did and did not receive viſits.

AMONG the works, which he finiſhed during theſe intervals of retirement, was probably a collection of elaborate proceſſes in chemiſtry, which he ſent to a friend, with the following letter:

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[z] Sir *Edmund King*.

“ SIR,

“ I CONFESS you are not the only perſon among my friends, to whom it hath ſeemed ſomewhat ſtrange, that I, who have ſpent many of my thoughts, ſome of my money, and, what I value far more, of my time too, upon chemiſtry, as well as divers other parts of learning, have not been taken notice of to have found any *particulars*, as chemiſts ſpeak, or other lucriferous experiments upon metals and minerals, nor have pretended to be poſſeſſor of thoſe difficult and compounded experiments, that are magnified by chemiſts as excellent *Hermetic Arcana*.

“ BUT, Sir, ſince I find you in the liſt of thoſe that have made the newly-mentioned reflection, I am content to give you ſuch a ſummary account of my comportment, as may at leaſt leſſen your wonder at it. I muſt inform you then, that when, among other ſtudies, I applied myſelf to the cultivating of natural philoſophy, I ſoon perceived, that ſome inſight into chemical operations was, though not abſolutely neceſſary, yet highly conducive to the true knowledge of nature, and eſpecially to the indagation of ſeveral of her moſt abſtruſe myſteries. On this ſcore I was induced to make a nearer inſpection into chemiſtry than virtuoſi are wont to think it worth while to do; and I did not repent me of my labour. But as I cultivated chemiſtry, not ſo much for itſelf, as for the ſake of natural philoſophy, and in order to it, ſo moſt of the experiments I deviſed and purſued, were generally ſuch as tended not to multiply proceſſes, or gain the reputation of having ſtore of difficult and elaborate ones; but to ſerve for foundations, and other uſeful materials for an experimental hiſtory of nature, on which a ſolid theory may in proceſs of time be ſuperſtructed. For this purpoſe I judged, that plain and eaſy experiments, and as ſimple, or as little compounded as may be, would, *cæteris paribus*, be the fitteſt, as being the moſt eaſy to be tried (and, if need be, repeated) and to be judged of, both in relation to their cauſes, and to their effects. And for theſe reaſons, though I had by me a not inconſiderable number of more compounded and elaborate proceſſes, ſome of which I had made, and others I received as great ſecrets from noted artiſts; I purpoſely forbore to mention any number of them in my writings about phyſics, being deſirous rather to increaſe knowledge, than make any oſtentation of any that I thought would puzzle moſt readers more than it would inſtruct them.

“ THIS, Sir, I hope, will appear to you a fair account of your not finding my phyſical diſcourſes larded with long and intricate proceſſes, ſome of which may, I willingly grant, produce notable effects, and for that reaſon are valuable, but are leſs fit than far more ſimple ones to diſcover the cauſes of things, which yet is the chief ſcope of a naturaliſt, as ſuch. And to thoſe that think it ſtrange, that among my other experiments about metals and minerals, I have not produced thoſe gainful ones, that chemiſts call *particulars*, it may, I hope, ſuffice to repreſent, that being a bachelor, and through God's bounty furniſhed with a competent eſtate for a younger brother, and freed from any ambition to leave my heirs rich, I had no need to purſue lucriferous experiments, to which I ſo much preferred luciferous ones, that I had a kind of ambition (which I now perceive to have been a vanity) of being able to ſay, that I cultivated chemiſtry with a diſintereſted mind, neither ſeeking nor ſcarce caring for any other advantages by it, than thoſe of the improvement of my own knowledge of nature, the gratifying the curious and induſtrious, and the acquiſt of ſome uſeful helps to make good and uncommon medicines.

“If I may be allowed to judge of courſes by the ſucceſs, the entertainment, that the public has been pleaſed to give my endeavours to ſerve it, will not make me repent of the way I have made choice of to do it in. But, however, ſince I find myſelf now grown old, I think it time to comply with my former intentions to leave a kind of Hermetic legacy to the ſtudious diſciples of that art, and to deliver candidly, in the annexed paper, ſome proceſſes chemical and medicinal, that are leſs ſimple and plain than thoſe barely luciferous ones I have been wont to affect, and of a more difficult and elaborate kind than thoſe I have hitherto publiſhed, and more of kin to the nobleſt Hermetic ſecrets, or, as *Helmont* ſtiles them, *arcana majora*. Some of theſe I have made and tried; others I have (though not without much difficulty) obtained, by exchange or otherwiſe, from thoſe that affirm they knew them to be real, and were themſelves competent judges, as being ſome of them diſciples of true adepts, or otherwiſe admitted to their acquaintance and converſation. Moſt of theſe proceſſes are clearly enough delivered; and of the reſt there is plainly ſet down, without deceitful terms, as much as may ſerve to make what is literally taught to be of great utility, though the full and complete uſes are not mentioned, partly becauſe, in ſpite of my philanthropy, I was engaged to ſecrecy, as to ſome of theſe uſes, and partly becauſe I muſt ingenuouſly confeſs it, I am not yet, or perhaps ever ſhall be acquainted with them myſelf. The knowledge I have of your great affection for the public good, and your particular kindneſs for me, invites me, among the many virtuoſi, in whoſe friendſhip I am happy, to intruſt the following papers in your hands, earneſtly deſiring you to impart them to the public faithfully, and without envy, *verbatim*, in my own expreſſions, as a monument of my good affections to mankind, as well in my chemical capacity, as in the others, wherein I have been ſolicitous to do it ſervice. I am, with ſincere reſpect,

“SIR,

“Your moſt faithful and moſt humble ſervant,

“ROBERT BOYLE.”

THIS collection of chemical proceſſes is not now to be found among his manuſcripts, though there are ſtill extant among them a great number relating to chemiſtry; and it appears, that he left orders, that after his death all his papers upon that ſubject ſhould be examined by three phyſicians nominated by him for that purpoſe[a]; being unwilling that they ſhould be loſt to the public. And indeed it is highly reaſonable to ſuppoſe, that many important diſcoveries were contained in them, chemiſtry being his favourite ſtudy, and opening to him perpetually ſuch a new ſcene of wonders, as eaſily perſuaded him of the poſſibility of the tranſmutation of metals into gold. This perſuaſion of his is evident from ſeveral parts of his writings, and was avowed by himſelf to the great Dr. *Halley*, the late royal aſtronomer, who related to me his converſation with him upon that ſubject; and it was probably in conſequence of this opinion, that Mr. *Boyle* procured by his intereſt the following *act* to be paſſed in Auguſt 1689 *for the repeal of a ſtatute made in the fifth year of King* Henry IV. *againſt the multiplying of gold and ſilver* [b].

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[a] Letter of Mr. *John Warr* ſenior to his ſon Mr. *John Warr*, executor to Mr. *Boyle*, dated July 16, 1692.

“ WHEREAS by a ſtatute made and enacted in the parliament held in the fifth year “ of the reign of King *Henry* the fourth, late King of *England*, it was, amongſt “ other things, enacted in theſe words, or to this effect, namely, *that none from* “ *thenceforth ſhould uſe to multiply gold or ſilver, or uſe the craft of multiplication*; *and* “ *if any the ſame do, they ſhould incur the pain of felony*: And whereas ſince the making “ of the ſaid ſtatute divers perſons have, by their ſtudy, induſtry, and learning, “ arrived to great ſkill and perfection in the art of melting and refining of metals, “ and otherwiſe improving them and their ores (which very much abound within this “ realm) and extracting gold and ſilver out of the ſame; but dare not exerciſe their “ ſaid ſkill within this realm, for fear of falling under the penalty of the ſaid ſtatute, “ but exerciſe the ſaid art in foreign parts, to the great loſs and detriment of this “ realm:

“ BE it therefore enacted by the king's and queen's moſt excellent majeſties, by “ and with the advice and conſent of the lords ſpiritual and temporal, and commons, “ in this preſent parliament aſſembled, that from henceforth the aforeſaid branch, “ article, or ſentence, contained in the ſaid act, and every word, matter, and thing, “ contained in the ſaid branch or ſentence, ſhall be repealed, annulled, revoked, and “ for ever made void; any thing in the ſaid act to the contrary in any wiſe whatſoever “ notwithſtanding.

“ PROVIDED always, and be it enacted by the authority aforeſaid, that all the gold “ and ſilver, that ſhall be extracted by the aforeſaid art of melting and refining of “ metals, and otherwiſe improving of them and their ores, as before ſet forth, be “ from henceforth employed for no other uſe or uſes whatſoever, but for the increaſe “ of monies; and that the place hereby appointed for the diſpoſal thereof ſhall be “ their majeſties mint within the Tower of *London*; at which place they are to receive “ the full and true value for their gold and ſilver ſo extracted from time to time, according “ to the aſſay and fineneſs thereof; and ſo for any greater or leſſer weight: “ and that none of that metal of gold and ſilver, ſo refined and extracted, be permitted “ to be uſed or diſpoſed in any other place or places within their majeſties “ kingdoms and dominions.

“ PROVIDED alſo, and be it further enacted by the authority aforeſaid, that no “ mine of copper, tin, iron, or lead, ſhall hereafter be adjudged, reputed, or “ taken to be a royal mine, although gold or ſilver may be extracted out of the “ ſame.”

IN 1690 he gave the world his *Medicina Hydroſtatica: or Hydroſtatics applied to the* Materia Medica; *ſhewing how by the weight that divers bodies uſed in phyſic have in water, one may diſcover whether they be genuine or adulterate. To which is ſubjoined, A previous hydroſtatical way of eſtimating Ores*; London, in 8vo. In the *Poſtſcript* he “ obſerves, that when he firſt ſent to the preſs his *Medicina Hydroſtatica*, he intended “ it ſhould, in the ſame book or volume, be accompanied by another help or two, “ to explore and improve the *Materia Medica*. But when, *ſays he*, the Eſſay itſelf, “ and the annexed Epiſtle about a previous Exploration of Ores had been printed “ off, I could not but perceive, that the bulk of thoſe two tracts ſo far exceed what

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[b] Statutes at Large, Vol. II. p. 1545. cap. 30, edit. *London*, 1706.

“ I ex-

“ I expected, that if I ſubjoined what at firſt I deſigned to add to it, it would prove a miſ-ſhapen book, and inconvenient to be opened. Wherefore it ſeemed expedient to divide the whole intended work in two volumes or tomes, whereof what had already paſt the preſs ſhould make the firſt; which, that it might be the ſooner ſerviceable, ſhould forthwith come abroad by itſelf; and the ſecond ſhould conſiſt partly of the other papers abovementioned, as relating to the *Materia Medica*, and partly of a ſupplement to the firſt tome, containing divers hiſtorical paralipomena, that by miſtake were omitted, and are fit to be there ſupplied out of a fuller copy than that, which by an overſight was made uſe of at the preſs.” But this ſecond tome never appeared. In the ſame year he publiſhed likewiſe another excellent work, entitled, *The Chriſtian Virtuoſo; ſhewing, that by being addicted to experimental philoſophy a man is rather aſſiſted than indiſpoſed to be a good Chriſtian. The firſt part. To which are ſubjoined,* I. *A Diſcourſe about the Diſtinction that repreſents ſome things as above reaſon, but not contrary to reaſon.* II. *The firſt Chapters of a Diſcourſe, entitled, Greatneſs of Mind promoted by Chriſtianity.* Printed in the *Savoy*, in 8vo. In the *Advertiſement* prefixed to the *Reflections upon a Theological Diſtinction, according to which it is ſaid, that ſome Articles of Faith are above Reaſon, but not againſt Reaſon*, he obſerves, that after he had begun the *ſecond part* of the *Chriſtian Virtuoſo*, and made ſome progreſs in it, which he deſigned to continue till he had completed it, he was obliged to leave the country, where he enjoyed ſome leiſure, and to remove to *London*; where ſickneſs, and buſineſs, and a multitude of viſits he could not avoid receiving, did ſo diſtract him, that theſe remoras, added to the fertility of the ſubjects that remained to be treated of, which he found much greater than he was at firſt aware of, made him lay aſide the materials he had prepared for the *ſecond part* to a fitter opportunity, and comply with the occaſions he had to publiſh ſome tracts that required more. He did not live to finiſh this *ſecond part*; but the papers, which he left behind him for that purpoſe, as well as for an *Appendix* to the *firſt part*, are printed in the preſent edition.

THE year following he communicated to Monſieur *de la Croſe*, author of *the Hiſtory of Learning*, an *Account of ſome Obſervations made in the great Congregation of Waters, by lowering Bottles down into the ſea ſix hundred feet deep from the ſurface, January the 2d*, 1677-8, which was printed in that work for the month of July 1691[c], with a ſhort letter from Mr. *Boyle* to the author, in which he obſerves, that this “ experiment made a great noiſe in the court of King *Charles* II. and will, *ſays he*, reſolve all the difficulties in the queſtions, which you propoſed to me, concerning the coldneſs of water. It was made by a captain of a ſhip, a man of very good ſenſe, and in the preſence of a great many perſons; inſomuch that there can be no manner of doubt concerning it.”

THE laſt work, which he publiſhed himſelf, was his *Experimenta & Obſervationes Phyſicæ: wherein are briefly treated of ſeveral Subjects relating to Natural Philoſophy in an experimental Way. To which is added, a ſmall Collection of ſtrange Reports.* Part I. *London* 1691, in 8vo. The *ſecond part* never appeared.

BEING now ſenſible from the decays of his health, that his death could not be very remote, he determined upon drawing up his laſt will, which he ſigned and ſealed upon the 18th of July 1691, and of which I ſhall ſubjoin a copy in the *Appendix* to this *Life*; but before he proceeded to the perfecting of it, he wrote down the following proteſtation.

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[c] Page 57, 58.

"WHEREAS I am this day about to perfect my laſt will and teſtament, I do hereby, to prevent and ſecure myſelf from all ſcruples, ſolemnly proteſt and declare, that I do not intend by ſigning and ſealing the ſaid will, or any other will or codicil that I may hereafter ſign, to abridge myſelf of any power that law or equity, or the nature of a will, do or can give me, to diſpoſe freely of all or any of my temporal concerns, even thoſe aſſigned to pious or charitable uſes therein mentioned; and that I reſerve to myſelf a full liberty, when this or any other teſtament ſhall be perfected, to annul, revoke, or alter the whole will, or any part of it, and diſpoſe otherwiſe of my concerns, as freely as if the ſaid will, or any part or codicil of it, had never been ſigned and publiſhed, or ſo much as intended by me. Witneſs my hand this 18th day of July 1691.

"ROBERT BOYLE.

"Witneſs
"JOHN WARR."

IN October following we find by a letter of his, dated the 8th of that month, to Dr. *Turberville* of *Saliſbury*[d], that his ſiſter the lady *Ranelagh* and himſelf were extremely ill; and he particularly complains of a diſtemper in his eyes, which greatly ſurprized and afflicted him, having continued with him for about a month. "The caſe in ſhort, *ſays he*, is this: in the day time, I ſee, thanks be to God, as I uſe to do, and ſo till five o'clock in the afternoon; but then, as ſoon as candles are brought in, I find a very ſenſible decay in my ſight; ſo that though I can ſee all the ſame groſs objects as I did before, and could, if I durſt, read printed books, as I have often tried, yet the reflection from thoſe objects is not vivid, as it was wont to be; and if I look upon ſomewhat diſtant objects, methinks I ſee them through a thin miſt, or a little ſmoke; but when the candles are newly ſnuffed, and ſo the light increaſed, I ſee far better for a little while, till it begin to have more ſnuff. This diſtemper continues as long as I make uſe of candle-light; but the next morning, by God's goodneſs, I find myſelf as before; only now and then there ſeems to fall ſlowly down, ſometimes in one eye, and ſometimes in another, a faintly ſhining vapour, which immediately diſappears. I have ſuch apparitions of late, for theſe two or three years, without any bad conſequence. What this diſtemper may proceed from, I know not, though I remember I have heard you more than once take notice of the narrowneſs of my pupil. Sight is a thing ſo dear to all men, and eſpecially to ſtudious perſons, that I earneſtly beg you would be pleaſed to conſider my caſe deliberately, and acquaint me with your thoughts of the cauſe; and more particularly to ſend preſcriptions of the receipts you would have me employ, and your directions what elſe you would have me do towards the cure of it.—I forgot to tell you, that for ſome months laſt paſt I have been much troubled with what they call vapours, or fumes of the ſpleen, and with ſome ſcorbutic diſaffections."

HIS ſiſter *Ranelagh*'s indiſpoſition at length terminated in her death, on the 23d of December following; and the loſs of ſuch a perſon was perhaps as fatal to him as it was important to the world. She had lived the longeſt on the moſt public ſcene, and made the greateſt figure in all the revolutions of theſe kingdoms for above fifty years, of any woman of that age. She employed her whole time, intereſt, and eſtate, in doing good to others; and as her great underſtanding, and the vaſt eſteem ſhe was in, made all perſons in their ſeveral turns of greatneſs deſire and value her friendſhip, ſo ſhe gave herſelf a clear title to uſe her intereſt with them for the ſervice of others, by this, that ſhe never made any advantage of it to any end or deſign of her own. She was contented with what ſhe had; and though ſhe was twice ſtript of it, ſhe never moved on her own account, but was the general interceſſor for all perſons of merit or in want. This had in her the better grace, and was both more Chriſtian and more effectual, becauſe it was not limited within any narrow compaſs of parties or relations. When any party was depreſſed, ſhe had credit and zeal enough to ſerve them; and ſhe employed that ſo effectually, that in the next turn ſhe had a new ſtock of credit, which ſhe laid out wholly in that *labour of love*, in which ſhe ſpent her life. And though ſome particular opinions might ſhut her up in a divided communion, yet her ſoul was never of a party. She divided her charities and friendſhips, her eſteem as well as her bounty, with the trueſt regard to merit and her own obligations, without any difference made upon the account of opinion. She had, with a vaſt reach both of knowledge and apprehenſion, an univerſal affability and eaſineſs of acceſs; an humility, that deſcended to the meaneſt perſons and concerns, an obliging kindneſs and readineſs to adviſe thoſe who had no occaſion for any further aſſiſtance from her. And with all theſe and many other excellent qualities ſhe had the deepeſt ſenſe of religion, and the moſt conſtant turning of her thoughts and diſcourſes that way, that was known perhaps in that age. Such a ſiſter became ſuch a brother; and it was but ſuitable to both their characters that they ſhould have improved the relation, under which they were born, to the more exalted and endearing one of friend[e]. And as they were *pleaſant in their lives, in their death they were not divided*; for as he had lived with her for the greateſt part of forty-ſeven years, ſo he did not ſurvive her above a week, for he died in the ſixty-fifth year of his age, on Wedneſday, December 30, 1691, at three quarters of an hour after twelve at night[f], and was interred on the 7th of January following, at the upper end of the ſouth ſide of the chancel of St. *Martin*'s *in the Fields* in *Weſtminſter*, near the body of his ſiſter *Ranelagh*, his funeral ſermon being preached by Dr. *Gilbert Burnet*, biſhop of *Saliſbury*, upon this text, *Eccleſ.* ii. 26. *For God giveth to a man, that is good in his ſight, wiſdom, knowledge, and joy.* His funeral was decent; and though without pomp, yet honoured with a great appearance of perſons of the higheſt diſtinction, beſides his own numerous relations.

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[d] Vol. VI. p. 61, 62.

BEFORE I proceed to his character, I ſhall inſert an account of his poſthumous works, which were as follows:

I. *The General Hiſtory of the Air deſigned and begun.* Printed at *London*, 1692, in 4to. Mr. *Locke*, in a letter to *William Molyneux*, Eſq; dated December 26, 1692, obſerves, that "though this treatiſe was left imperfect, yet, I think, *ſays he*, the very "deſign of it will pleaſe you; and it is caſt into a method, that any one, "who pleaſes, may add to it under any of the ſeveral titles, as his reading or ob-"ſervation ſhall furniſh him with matter of fact. If ſuch men as you are, curious "and knowing, would join to what Mr. *Boyle* had collected and prepared, what "comes in their way, we might hope in ſome time to have a conſiderable hiſtory of "the air, than which I ſcarce know any part of natural philoſophy would yield more "variety and uſe. But it is a ſubject too large for the attempts of any one man,

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[e] Biſhop *Burnet*'s Funeral Sermon on Mr. *Boyle*, p. 33, 34.

[f] Mr. *Warr*'s MSS.

"and will require the aſſiſtance of many hands to make it a hiſtory very ſhort of "complete." Mr. *Molyneux*, in his anſwer, dated March 2, 1692-3, writes thus: "I am extremely obliged to you for Mr. *Boyle*'s book of the air, which lately came "to my hands. It is a vaſt deſign, and not to be finiſhed but by the united labours "of many heads, and indefatigably proſecuted for many years; ſo that I deſpair of "ſeeing any thing complete therein. However, if many will lend the ſame helping "hands that you have done, I ſhould be in hopes; and certainly there is not a "chapter in all natural philoſophy of greater uſe to mankind, than what is here pro"poſed."

II. *Medicinal Experiments: or a Collection of choice remedies, for the moſt part ſimple and eaſily prepared: London* 1692, 12mo. This is a *ſecond edition* of the *Receipts ſent to a Friend in America:* printed in 1688, with a new preface, and the addition of a *ſecond part*, or the latter five decads. Theſe were again reprinted in 1696, and called the *firſt* and *ſecond* volume; and in 1698 a *third volume* was added to them.

III. *General Heads for the Natural Hiſtory of a country great or ſmall, drawn out for the Uſe of Travellers and Navigators.* To which are added, *Other Directions for Navigators, &c. with particular Obſervations of the moſt noted Countries in the world. By another Hand. London* 1692, in 12mo. Theſe *General Heads* were firſt printed in the *Philoſophical Tranſactions*, No. II. p. 186. No. XVIII. p. 315. No. XIX. p. 330. being drawn up by Mr. *Boyle*, at the requeſt of the Royal Society. The other directions added in this edition were drawn up by various perſons at divers times, by order of the *Royal Society*, and printed in different numbers of the *Philoſophical Tranſactions*; but being in purſuance of the plan ſketched out by Mr. *Boyle*, were very properly annexed to the preceding ones.

IV. *A paper of the honourable* Robert Boyle's, *depoſited with the Secretaries of the Royal Society, October* 14, 1680, *and opened ſince his death; being an Account of his making the Phoſphorus, &c. September* 30, 1680. Printed in the *Philoſophical Tranſactions*, No. CXCVI. p. 583. anno 1692-3.

V. *An Account of a Way of examining Waters, as to freſhneſs and ſaltneſs. To be ſubjoined as an Appendix to a lately printed Letter about ſweetened Water: October* 30, 1683: publiſhed in the *Philoſ. Tranſact.* No. CXCVII. p. 627.

VI. *A Free Diſcourſe againſt Cuſtomary Swearing, and a Diſſuaſive from Curſing. London* 1695, in 8vo.

VII. *Medicinal Experiments: or a Collection of choice remedies, chiefly ſimple and eaſily prepared; uſeful in Families, and fit for the Service of Country People. The third and laſt Volume, publiſhed from the Author's original MSS. Whereunto is added, ſeveral other uſeful notes, explicatory of the ſame. London* 1698, in 12mo. This and the two preceding volumes have been ſince ſeveral times reprinted all together.

MR. *Boyle* was tall of ſtature, but ſlender, and his countenance pale and emaciated[g]. His conſtitution was ſo tender and delicate, that he had divers ſorts of cloaks to put on when he went abroad, according to the temperature of the air; and in this he governed himſelf by his thermometer[h]. He eſcaped indeed the ſmall-pox during his life[i]; but for almoſt forty years he laboured under ſuch a feebleneſs of body, and ſuch lowneſs of ſtrength and ſpirits, that it was aſtoniſhing, how he could read, meditate, try experiments, and write as he did. He had likewiſe a weakneſs in his eyes, which made him very tender of them, and extremely apprehenſive of ſuch diſtempers as might affect them. He imagined alſo, that if ſickneſs ſhould confine him to his bed, it might raiſe the pains of the ſtone to a degree, which might be above his ſtrength to ſupport; ſo that he feared leſt his laſt minutes ſhould prove too hard for him. This was the ground of all the caution and apprehenſion, which he was obſerved to live in. But as to life itſelf, he had that juſt indifference to it, which became ſo true a Chriſtian. However, his ſight began not to grow dim above four hours before he died; and when death came upon him, he had not been above three hours in bed, before it made an end of him with ſo little pain, that it was plain the light went out merely for want of oil to maintain the flame[k]. The ſimplicity of his diet was in all appearance that which preſerved him ſo long beyond all men's expectation. This he practiſed ſo ſtrictly, that in a courſe of above thirty years he neither eat nor drank to gratify the varieties of appetite, but merely to ſupport nature; and was ſo regular in it, that he never once tranſgreſſed the rule, meaſure, and kind, which were preſcribed him[l].

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[g] Letter of *John Evelyn*, Eſq; to Dr. *Wotton*, March 29, 1696.

[h] Information of Sir *Hans Sloan*, Bart.

[i] Letter of the Rev. Mr. *Kirkwood* to Mr. *Boyle*, dated July 13, 1689.

In his firſt addreſſes, when he was to ſpeak or anſwer, he ſometimes heſitated a little, rather than ſtammered, or repeated the ſame word; and this, as it rendered him ſlow and deliberate, ſo after the firſt effort he proceeded without the leaſt interruption in his diſcourſe[m].

He was never married; but Mr. *Evelyn* was aſſured, that he courted the beautiful and ingenious daughter of *Cary*, earl of *Monmouth*; and that to this paſſion was owing his *Seraphic Love*[n]. But it does not appear from any of his papers, that he had ever entertained the leaſt thoughts of that kind; and he wrote the following letter, when he was very young, to the lady *Barrimore*, his niece, upon a report of his being actually married, of which that lady had informed him.

"* * * It is high time for me to haſten the payment of the thanks I owe your "ladyſhip for the joy you are pleaſed to wiſh me, and of which that wiſh poſſibly gives "me more than the occaſion of it would. You have certainly reaſon, Madam, to "ſuſpend your belief of a marriage celebrated by no prieſt but fame, and made un- "known to the ſuppoſed bridegroom. I may poſſibly ere long give you a fit of the "ſpleen upon this theme; but at preſent it were incongruous to blend ſuch pure "raillery, as I ever prate of matrimony and amours with, among things I am ſo "ſerious in as thoſe this ſcribble preſents you. I ſhall therefore only tell you, that "the little gentleman and I are ſtill at the old defiance. You have carried away too "many of the perfections of your ſex, to leave enough in this country for the re- "ducing ſo ſtubborn a heart as mine, whoſe conqueſt were a taſk of ſo much diffi- "culty, and is ſo little worth it, that the latter property is always likely to deter any, "that hath beauty and merit enough to overcome the former. But though this "untamed heart be thus inſenſible to the thing itſelf called love, it is yet very ac- "ceſſible to things very near of kin to that paſſion; and eſteem, friendſhip, reſpect, "and even admiration, are things, that their proper objects fail not proportionably

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[k] *Burnet*'s Funeral Sermon, p. 31.

[l] Id. Ibid.

[m] Mr. *Evelyn*'s letter of March 29, 1696.

[n] Ibid.

"to exact of me, and conſequently are qualities, which in their higheſt degrees are "really and conſtantly paid my lady *Barrimore* by her,

"Moſt obliged humble ſervant,

"and affectionate uncle,

"ROBERT BOYLE."

IN the memorandums of Mr. *Boyle*'s life, ſet down by biſhop *Burnet*, it is remarked, that *he abſtained from purpoſes of marriage, at firſt out of policy, afterwards more philoſophically*; *and upon a general propoſition with many advantages, he would not know the perſon's name.* And we find by a letter of Dr. *John Wallis* to him, dated at *Oxford*, July 17, 1669[o], that he had an overture made to him with reſpect to the lady *Mary Haſtings*, ſiſter to the earl of *Huntingdon*, and eminent for her admirable temper, great piety, and uncommon underſtanding, with every other accompliſhment proper to make him an excellent wife. But he ſtill perſiſted in his firſt reſolution of living ſingle, though few men were more facetious and agreeable in converſation with the ladies, whenever he happened to be engaged among them[p]. And indeed ſometimes, upon other occaſions, he diſtinguiſhed himſelf by ſo copious and lively a flow of wit, that Mr. *Cowley*, and Sir *William Davenant*, both thought him equal in that reſpect to the moſt celebrated geniuſes of that age[q].

HE had ſo profound a veneration for the Deity, that the very name of God was never mentioned by him without a pauſe and a viſible ſtop in his diſcourſe; in which Sir *Peter Pett*[r], who knew him for almoſt forty years, affirms, that he was ſo exact, that he did not remember to have obſerved him once to fail in it. He was very conſtant and ſerious in his ſecret addreſſes to God; and it appeared to thoſe who converſed moſt with him in his enquiries into nature, that his main deſign in that, on which as he had his own eye moſt conſtantly, ſo he took care to put others often in mind of it, was to raiſe in himſelf and others more elevated thoughts of the greatneſs and glory, and of the wiſdom and goodneſs of the Deity[s]. This was ſo deep in his mind, that he concludes the article of his will, which relates to the Royal Society, in theſe words: *Wiſhing them alſo a happy ſucceſs in their laudable attempts to diſcover the true nature of the works of God, and praying, that they and all other ſearches into phyſical truths, may cordially refer their attainments to the glory of the great Author of Nature, and to the comfort of mankind.* For this purpoſe he founded his lecture in the city of *London*, charging, by a codicil annexed to his will, and dated July 28, 1691, his meſſuage or dwelling-houſe in St. *Michael*'s *Crooked-Lane*, in that city, with the payment of the clear yearly rents and profits thereof to ſome learned divine in *London*, or within the bills of mortality, to be elected for a term not exceeding three years by Dr. *Teniſon*, afterwards archbiſhop of *Canterbury*, Sir *Henry Aſhurſt*, Sir *John Rotherham*, and *John Evelyn*, Eſq. The buſineſs which he appointed thoſe lecturers, was, among others, *to be ready to ſatisfy real ſcruples, and to anſwer ſuch new objections and difficulties, as might be ſtarted, to which good anſwers had not been made*; and alſo *to preach eight ſermons in the year, the firſt Monday of January, February, March,*

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[o] Vol. VI. p. 458, 459, 460.

[p] Mr. *Evelyn*'s letter of March 29, 1696.

[q] Sir *Peter Pett*'s papers relating to Mr. *Boyle*.

[r] Ibid.

[s] *Burnet*'s Funeral Sermon, p. 25.

April, and May, and of September, October, and November. The ſubject of theſe ſermons was to be *the proof of the Chriſtian Religion againſt notorious Infidels, viz. Atheiſts, Theiſts, Pagans, Jews, and Mahometans, not deſcending lower to any controverſies, that are among Chriſtians.* But by reaſon the lectures were ſeldom continued above a year, and that the houſe ſometimes ſtood empty, and tenants broke, or failed in due payment of their rent, therefore the ſalary ſometimes remained long unpaid, or could not be gotten without ſome difficulty. To remedy which inconvenience, archbiſhop *Teniſon* procured a yearly grant of 50 pounds to be paid quarterly for ever, charged upon a farm in the pariſh of *Brill*, in the county of *Bucks*; which ſtipend is accordingly very duly paid, when demanded, without fee or reward[t].

HE was likewiſe at the charge of the tranſlation and impreſſion of the *Four Goſpels* and *Acts of the Apoſtles* into the Malayan language, as I have remarked above; and this book he ſent over all the *Eaſt-Indies.* He gave a noble reward to Dr. *Edward Pococke*, who tranſlated *Grotius*'s incomparable treatiſe *of the Truth of the Chriſtian Religion* into Arabic, and was at the charge of a whole impreſſion, which was finiſhed at *Oxford* in 1660, in 4to, and which he took care to order to have diſperſed in all the countries, where that language is underſtood. He was reſolved to have carried on the impreſſion of the New Teſtament in the Turkiſh language; but the Company thought, that it became them to perform that work, and ſo ſuffered him only to give a large ſhare towards it[u]. He was at 700 pounds charge in the edition of the *Iriſh* bible, which he ordered to be diſtributed in *Ireland*[v]; a particular account of which will appear from the *Appendix*, No. III.

HE contributed largely alſo to the impreſſions both of the Welch bible, and of the Iriſh bible, for the uſe of the *High-Lands* in *Scotland*, as may be likewiſe ſeen in the *Appendix*, No. IV.

HE gave, during his life, 300 pounds to advance the deſign of propagating the Chriſtian religion in *America*; and his zeal and generoſity in that reſpect are acknowledged in many letters of Mr. *Eliot*, of *New-England*; which are given in the *Appendix*, No. V.

HE was no leſs a friend to that colony in their civil affairs, as appears by letters of thanks ſigned by the governor, &c. ſent to him, and printed in the *Appendix*, No. VI.

HE had the greateſt regard for the clergy, of which he gave many inſtances. When he underſtood, what ſhare he had in impropriations, he ordered very large gifts to be made to the incumbents in thoſe pariſhes, and to the widows of ſuch as died before he had reſolved upon this charity. A perſon who was concerned in two diſtributions which were made, declared, that the ſums upon thoſe two occaſions amounted to near 600 *l.* *And another very liberal one*, ſays biſhop *Burnet*[w], *is alſo ordered by his will, but in an indefinite ſum; I ſuppoſe, by reaſon of the preſent condition of eſtates in* Ireland; *ſo plentifully did he ſupply thoſe who ſerved at the altar out of that, which was once devoted to it, though it be now converted to a temporal eſtate.* It appears likewiſe, by a letter of his to one of his ſtewards in *Ireland*, dated January 22, 1684-5, that he had ordered a former ſteward to ſet aſide every year about a fifth part (rather more than leſs) of the clear annual income of his tithes and impropriations, to be employed in pious uſes. The orders to the new ſteward run thus: “ I muſt deſire “ you at every half year's rent day, or when the rents then due ſhall be paid into “ you, to lay aſide, till you receive contrary orders, the full fifth part of what you “ judge you receive for me *de claro* upon the account of tithes and impropria- “ tions, to be diſpoſed,” *&c.* And in a letter of Sir *Robert Southwel* to him from *Kingſale*, March 20, 1675[x], are theſe words: *It has often come into my mind, and indeed I thought, that I had accordingly wrote lately of it, that concern of yours, which long ſince you were pleaſed to honour me with the truſt and care of, I mean, that ſtock of money, which, out of your great bounty and charity, you were pleaſed to diſtribute amongſt thoſe miniſters that had ſerved the cures upon your impropriate eſtate in this kingdom, and of the diſtribution of which I did long ſince return you an account, and alſo what balance remained unto you, after ſatisfying all the concerned, which was forty-five pounds, &c.* I find alſo, by a liſt of names, which he had procured, together with ſeveral letters of thanks ſent him out of *Wales*, that he extended his diffuſive charity to ſuch poor nonconformiſt miniſters there, as were men of piety and good learning.

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[t] See the note upon Dr. *Will. Derham*'s dedication of his *Phyſico-Theology*, fifth edit. *London* 1720, in 8vo.

[u] *Burnet*'s Funeral Sermon, p. 26.

[v] Id. ibid. See likewiſe *Burnet*'s Life of Dr. *Will. Bedell*, biſhop of *Kilmore*, p. 136, 137. Edit. *London* 1685. in 8vo.

[w] Funeral Sermon, p. 27.

HE was conſtant to the eſtabliſhed church, and went to no ſeparate aſſemblies, how charitably ſoever he might think of their perſons, and how plentifully ſoever he might have relieved their neceſſities[y]. He had once indeed, before the reſtoration, as he told Sir *Peter Pett*[z], the curioſity to go to Sir *Henry Vane*'s houſe, and there heard him preach in a large thronged room a long ſermon, on the text of *Dan.* xii. 2. *And many of them that ſleep in the duſt of the earth ſhall awake, ſome to everlaſting life, and ſome to ſhame and everlaſting contempt.* The whole ſcope of Sir *Henry*'s ſermon was to ſhew, that many doctrines of religion, that had long been dead and buried in the world, ſhould before the end of it be awakened into life; and that many falſe doctrines being then likewiſe revived, ſhould, by the power of truth, be then doomed *to ſhame and everlaſting contempt.* When Sir *Henry* had concluded his diſcourſe, Mr. *Boyle* ſpoke to this effect to him before the people; That being informed, that in ſuch private meetings it was not uncuſtomary for any one of the hearers, who was unſatiſfied about any matters there uttered, to give in his objections againſt them, and to prevent any miſtakes in the ſpeakers or hearers; he thought himſelf obliged for the honour of God's truth to ſay, that this place in *Daniel* being the cleareſt one in all the Old Teſtament for the proof of the reſurrection, we ought not to ſuffer the meaning of it to evaporate into allegory; and the rather, ſince that inference is made by our Saviour in the New Teſtament by way of aſſerting the reſurrection from that place of *Daniel* in the old. And that if it ſhould be denied, that the plain and genuine meaning of thoſe words in the Prophet is to aſſert the reſurrection of dead bodies, he was ready to prove it to be ſo, both out of the words of the text and context in the original language, and from the beſt expoſitors both Chriſtian and Jewiſh. But that if this be not denied, and Sir *Henry*'s diſcourſe of the reſurrection of doctrines true and falſe was deſigned by him only in the way of occaſional meditations from thoſe words in *Daniel*, and not to enervate the literal ſenſe as their genuine one, then he had nothing further to ſay. Mr. *Boyle* then ſitting down, Sir *Henry* roſe up and ſaid, that his diſcourſe was only in the way of ſuch occaſional meditations, which he thought edifying to the people; and declared, that he agreed, that the literal ſenſe of the words was the reſurrection of dead bodies; and ſo that meeting broke up. Mr. *Boyle* afterwards ſpeaking of this conference to Sir *Peter Pett*, obſerved, that Sir *Henry Vane* at that time being in the height of his authority in the ſtate, and his auditors at that meeting conſiſting chiefly of dependents on him and expectants from him, the fear of loſing his favour would probably have reſtrained them from contradicting any of his interpretations of ſcripture, how ridiculous ſoever. " But " I (ſaid Mr. *Boyle)* having no little awes of that kind upon me, thought myſelf " bound to enter the liſts with him, as I did, that the ſenſe of the ſcriptures might " not be depraved."

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[x] Vol. VI. p. 301.

[y] *Burnet*'s Funeral Sermon, p. 29.

[z] Sir *Peter Pett*'s papers relating to Mr. *Boyle*.

HE had poſſeſſed himſelf with ſuch an amiable view of Chriſtianity, ſeparated from either ſuperſtitious practices or the ſourneſs of parties, that as he was fully perſuaded of the truth of it, he rejoiced in every diſcovery, which nature furniſhed him with to illuſtrate it, or to take off the objections againſt any part of it. He always conſidered it as a ſyſtem of truths, which ought to purify the hearts, and govern the lives of thoſe who profeſs it. He loved no practice, which ſeemed to leſſen that, nor any nicety, which occaſioned diviſions amongſt Chriſtians. He thought, that pure and diſintereſted Chriſtianity was ſo bright and glorious a ſyſtem, that he was much troubled at the diſputes and diviſions which had riſen about ſome leſſer matters, while the great and the moſt important, as well as moſt univerſally acknowledged truths were by all ſides almoſt as generally neglected, as they were confeſſed [a]. He loved no narrow thoughts, no low or ſuperſtitious opinions in religion; and therefore as he did not ſhut himſelf within a party, ſo neither did he ſhut any party out from him [b]. His zeal was lively and effectual in the greateſt and trueſt concerns of religion; but he avoided to enter far into the unhappy breaches, which had long weakened as well as diſtracted Chriſtianity any otherwiſe, than to have a great averſion to all thoſe opinions and practices, which ſeemed to him to deſtroy morality and charity. He had a moſt particular zeal againſt all ſeverities and perſecution upon the account of religion; *and I have ſeldom*, ſays biſhop *Burnet* [c], *obſerved him to ſpeak with more heat and indignation, than when that came in his way. He did thoroughly agree with the doctrines of our church, and conform to our worſhip*; *and he approved of the main of our conſtitution*; *but he much lamented ſome abuſes, that he thought remained ſtill among us.* And Dr. *Thomas Dent*, prebendary of *Weſtminſter*, who was a particular friend of his, obſerves [d], that " he always heard him expreſs his judgment and " inclination to the church of *England*; but he was for moderation to thoſe who " diſſented from us, and not to force tender conſciences, for which he ſeemed to " expreſs great averſeneſs. He had frequent conferences on this ſubject with the " preſent archbiſhop [*Teniſon*], biſhop of *Sarum* [*Burnet*], but particularly the late " biſhop of *Worceſter*, the learned Dr. *Stillingfleet*, for whoſe depth of learning and " ſolid judgment he had always the greateſt value and eſteem." Sir *Peter Pett* likewiſe affirms, that he was peculiarly warm in his expreſſions againſt perſecution on account of religion; and relates, that ſoon after the reſtoration Mr. *Boyle* and he diſcourſing of the ſeverities practiſed by the biſhops towards the Puritans in the reign of King *Charles* I. and of thoſe which were returned upon the epiſcopal divines, during the following uſurpations; and being apprehenſive, that the reſtored clergy might be tempted by their late ſuffeeings to ſuch a vindictive retaliation, as would be contrary to the true meaſures of Chriſtianity and politics, they came at laſt to an agreement, that it would tend to the public good, to have ſomething written and publiſhed in defence of liberty of conſcience. Mr. *Boyle* undertook to engage Dr. *Thomas Barlow*, whoſe judgment in that point he very well knew to treat of the theological part of the queſtion; and deſired Sir *Peter* to write of the political part; which the latter conſented to, on condition that Mr. *Boyle* would let him read his manuſcript before it was committed to the preſs, and give him his opinion about the whole. Mr. *Boyle* frankly promiſed this, and was willing to ſtate the fact of the allowance of liberty of conſcience in foreign parts. But afterwards conſidering, that Mr. *John Dury*, who had ſpent many years in his travels, eſpecially in the northern parts of *Europe*, when he was engaged in his ſcheme for reconciling the Lutherans and Calviniſts, was capable of writing on that ſubject with more extent and exactneſs than himſelf, he prevailed upon Mr. *Dury* to write upon it, and rewarded him for it, and delivered the treatiſe drawn by him to Sir *Peter*, who publiſhed it at the end of his own in 1660, (though the bookſellers, according to their cuſtom, antedated in the title page 1661) and inſcribed both thoſe treatiſes with the laſt letters only of the writers names. But he did not print Dr. *Barlow*'s diſcourſe upon the ſame ſubject; becauſe as, on the one hand, it would not, how ſtrong ſoever its reaſonings were, be ſufficient to reſtrain the rigorous meaſures reſolved upon againſt the Nonconformiſts, ſo, on the other, it might expoſe the doctor to the reſentment of his brethren, whom he had offended by writing, juſt before the reſtoration, a letter to Mr. *John Tombes*, the famous Anabaptiſt, in which he had expreſſed ſome prejudice againſt the practice of infant-baptiſm, and by refuſing, even after the reſtoration, to retract that letter, notwithſtanding he was in danger of loſing, by that refuſal, his ſtation in the univerſity of *Oxford*, and all his hopes of future preferment. However his diſcourſe abovementioned was publiſhed after his death, under the title of *The Caſe of a Toleration in Matters of Religion*, in a collection, intitled, *Several miſcellaneous and weighty Caſes of Conſcience learnedly and judiciouſly reſolved by the Right Reverend Father in God Dr.* Thomas Barlow, *late Lord Biſhop of* Lincoln; printed at *London* 1692, in 8vo.

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[a] *Burnet*'s Funeral Sermon, p. 25, 26. [b] Ibid. p. 29. [c] Ibid p. 27.

[d] Letter to Dr. *William Wotton*, of May 20, 1699.

MR. *Boyle*'s charity to thoſe who were in want, and his bounty to all learned men who were put to wreſtle with neceſſities, were very extraordinary. Great ſums went eaſily from him without the partialities of ſect, country, or relations; for he conſidered himſelf as a part of the human nature, and a debtor to the whole race of men. He took care to do this ſo ſecretly, thal even thoſe who knew all his other concerns, could never find out what he did that way. And indeed he was ſo ſtrict to our Saviour's precept, that except the perſons themſelves, or ſome one whom he truſted to convey it to them, no body ever knew how that great ſhare of his eſtate, which went away inviſibly, was diſtributed; even he himſelf kept no account of it, for that he thought might fall into other hands[e]. "I ſpeak (ſays biſhop *Burnet*[f]) upon full "knowledge on this article, becauſe I had the honour to be made uſe of by him in "it. If thoſe that have fled hither from the perſecutions of *France*, or from the calamities of *Ireland*, feel a ſenſible ſinking of their ſecret ſupplies, with which they "were often furniſhed, without knowing from whence they came, they will conclude, "that they have loſt not only a purſe, but an eſtate, that went ſo very liberally among "them, that I have reaſon to ſay, that for ſome years his charity went beyond a thouſand pounds a year."

HE was very plain, unaffected, and temperate in the manner of his life, and had about him all that neglect of pomp in clothes, lodging, furniture, and equipage, which agreed with his grave and ſerious courſe of life[g].

He was extremely candid and courteous in his converſation. His conſtitution indeed inclined him to be choleric; but he gained ſo perfect an aſcendent over this paſſion,

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[e] *Burnet*'s Funeral Sermon, p. 31.
[f] Ibid. p. 32.
[g] Ibid p. 30.

ſion, that it never appeared, except ſometimes in his countenance upon a very high provocation[h]. He had brought his mind to ſuch a freedom, that he was not apt to be impoſed upon; and his modeſty was ſuch, that he did not dictate to others, but propoſed his own ſenſe with a due and decent diſtruſt, and was ever ready to hearken to what was ſuggeſted to him by others. When he differed from any, he expreſſed himſelf in ſo humble and ſo obliging a way, that he never treated things or perſons with neglect; and he was never known to have offended any perſon in his whole life by any part of his deportment. For if at any time he ſaw cauſe to ſpeak roundly to any, it was never in paſſion, or with any reproachful or indecent expreſſions. And as he was careful to give thoſe who converſed with him no cauſe or colour for diſpleaſure; ſo he was yet more careful of thoſe who were abſent, never to ſpeak ill of any. If the diſcourſe began to be hard upon any perſon, he was immediately ſilent; and if the ſubject was too long dwelt upon, he would at laſt interpoſe, and between reproof and raillery divert it[i].

WHATEVER he was in the ſight of men, how pure and ſpotleſs ſoever his character appeared to the world, he was in reality the ſame in his moſt ſecret receſſes. He affected nothing which was ſolemn or ſupercilious, nor uſed any methods to make multitudes run after him, or depend upon him. It was never diſcovered, that there was any thing hid under all this appearance of goodneſs which was not truly ſo; for he concealed both his piety and charity all he could, and lived in the due methods of civility, and would never aſſume the authority which all the world was ready to pay him. He allowed himſelf a great deal of decent chearfulneſs, without the leaſt tincture of that moroſeneſs to which philoſophers think they have ſome right; nor of thoſe affectations which men of an extraordinary pitch of devotion ſometimes run into, without being well aware of them. He had indeed nothing of frolic and levity in him; he had no reliſh for the idle and extravagant madneſs of the men of pleaſure. He did not waſte his time, nor diſſipate his ſpirits in fooliſh mirth; but he poſſeſſed his own ſoul in patience, full of that ſolid joy which his goodneſs as well as his knowledge afforded him. He, who had neither deſigns nor paſſions, was capable of little trouble from any concerns of his own. He had about him all the tenderneſs of good-nature, as well as all the ſoftneſs of friendſhip. Theſe gave him a large ſhare of other men's concerns; for he had a quick ſenſe of the miſeries of mankind. He had alſo a feeble body, which needed to be looked to the more, becauſe his mind went faſter than his body could keep pace with it. Yet his great thoughts of God, and his contemplation of his works, were to him ſources of continual joy, which never could be exhauſted. The ſenſe of his own integrity, and of the good which he found it did, afforded him the trueſt of all pleaſure, *ſince they gave him*, ſays biſhop *Burnet*[k], *the certain proſpect of that fulneſs of joy, in the ſight of which he lived ſo long.*

HE ſpake of the government even in times which he diſliked, and upon occaſions which he ſpared not to condemn, with an exactneſs of reſpect[l]. King *Charles* II. King *James* II. and King *William*, were ſo highly pleaſed with his converſation, that they often uſed to diſcourſe with him with great familiarity. His four elder brothers being all noblemen, he was ſeveral times offered a peerage, which he conſtantly refuſed to accept[m]; but he procured a title, which, without derogating from the dignity of Kings, muſt be acknowledged to be beyond their prerogative[n]. He had too unblemiſhed a candor, to be capable of thoſe arts and practices which the world generally

[h] *Burnet*'s MS. papers.

[i] *Burnet*'s Funeral Sermon, p. 29, 30.

[k] Id. p. 38.

[l] Id. p. 35.

[m] *Budgell*'s Life of the Earl of *Orrery*, p. 145.

[n] *Burnet*'s Funeral Sermon, p. 23.

terms wiſdom. He could neither lie nor equivocate, but could well be ſilent; and by practiſing that much, he covered himſelf upon many uneaſy occaſions. He made true judgments of men and things; and his advices were ſolid and ſound; and if caution and modeſty gave too ſtrong a bias, his invention was fruitful to ſuggeſt good expedients. He had great notions of what human nature might be brought to; but ſince he ſaw mankind not capable of them, he withdrew himſelf early from courts and affairs, *notwithſtanding the diſtinction*, ſays biſhop *Burnet*[o], *with which he was always uſed by our late Princes.* He had the principles of an Engliſhman, as well as of a proteſtant, too deep in him to be corrupted, or to be cheated out of then; and in theſe he ſtudied to fortify all who converſed much with him. He had a very particular ſagacity in obſerving what men were fit for; and had ſo vaſt a ſcheme of different performances, that he could eaſily furniſh every man with work, who had leiſure and capacity for it; and as ſoon as he ſaw him engaged in it, then an handſome preſent was made to enable him to go on with it[p].

THE reputation which he had acquired among foreign nations was ſo great, that no ſtrangers, who came among us, and had any taſte for learning and philoſophy, left *England* without ſeeing him. He received them with a certain openneſs and humanity which were peculiar to him; and though theſe viſits made a great waſte of his time, yet as he was ſtrict in not ſuffering himſelf to be denied when he was at home, ſo he ſaid, that he knew the heart of a ſtranger, and how much eaſed his own had been, while he was travelling, if admitted to the converſation of thoſe whom he deſired to ſee; and therefore he thought that his obligation to ſtrangers was more than mere civility, and that it was a point of religious charity in him[q].

HIS knowledge was of prodigious extent. He was a great maſter of the *Greek* language, and read the New Teſtament in the original with ſuch attention, that he could have quoted it almoſt as readily as the Engliſh verſion[r]. He carried the ſtudy of the Hebrew tongue very far into the Rabbinical writings; and drew up a grammar in it for his own uſe. He learned likewiſe the Chaldee and Syriac, and would have gained a thorough knowledge of the Arabic, if the infirmity of his eyes had not interrupted his progreſs in it[s]. He had read ſo much of the Fathers, that he had formed out of it a clear judgment of all the eminent ones. He had read over a vaſt number of commentators on the ſcriptures, and had gone with great exactneſs through the whole controverſies of religion, and had a juſt idea of the intire body of divinity. He run the whole compaſs of the mathematical ſciences; and though he did not ſet up for an inventor in them, yet he knew even the abſtruſeſt parts of geometry. Geography in the ſeveral branches of it, which related to navigation or travelling, hiſtory, and books of travels, were his diverſions. He went very accurately through all the parts of phyſic; only the tenderneſs of his nature made him leſs able to endure the exactneſs of anatomical diſſections, eſpecially of living animals, though he knew theſe to be moſt inſtructing. But for the hiſtory of nature ancient and modern, of the productions of all countries, of the virtues and improvements of plants, ores, and minerals, and all the varieties which are in them in different climates, his knowledge was unrivalled in that age[t]. But his peculiar and favourite ſtudy was chemiſtry, “ in which, *ſays biſhop* Burnet[u], he engaged with “ none of thoſe ravenous and ambitious deſigns that drew many into them. His “ deſign was only to find out nature, to ſee into what principles things might be

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[o] *Burnet*'s Funeral Sermon, p. 35, 36. [p] Ibid. p. 36. [q] Ibid. p. 30. [r] *Burnet*'s MS. papers. [s] Ibid. [t] *Burnet*'s Funeral Sermon, p. 36. [u] Ibid p. 37.

" reſolved, and of what they were compounded, and to prepare good medicaments for the bodies of men. He ſpent neither his time nor his fortune upon the vain purſuits of high promiſes and pretenſions. He always kept himſelf within the compaſs, that his eſtate might well bear. And as he made chemiſtry much the better for his dealing in it, ſo he never made himſelf the worſe or the poorer for it. It was a charity to others, as well as an entertainment to himſelf; for the produce of it was diſtributed by his ſiſter, and others, into whoſe hands he put it." His laboratory was conſtantly open to the curious, whom he permitted to ſee moſt of his proceſſes. It is true, he found out ſome things in the courſe of his experiments, which he looked upon himſelf as obliged to conceal for the good of mankind. Of this nature were ſeveral ſorts of poiſons, and a certain liquor with which, he aſſures us, he could diſcharge all the writing of any deed upon paper and parchment, and leave nothing but the parties names who ſigned it; and that the place whence the writing had been diſcharged, would bear ink again as well as ever [w].

His merit as a writer in natural philoſophy and chemiſtry, is univerſally acknowledged. Dr. *Herman Boerhaave* [x], after having declared lord *Bacon* to be the father of experimental philoſophy, aſſerts, that "Mr. *Boyle*, the ornament of his age and country, ſucceeded to the genius and inquiries of the great chancellor *Verulam*. Which of Mr. *Boyle*'s writings ſhall I recommend? All of them. To him we owe the ſecrets of fire, air, water, animals, vegetables, foſſils; ſo that from his works may be deduced the whole ſyſtem of natural knowledge." Dr *Richard Bentley*, in his *fourth ſermon*, preached at the lecture founded by Mr. *Boyle*, thus expreſſes himſelf: *The mechanical or corpuſcular philoſophy, though peradventure the oldeſt as well as beſt in the world, had lain buried for many ages in contempt and oblivion, till it was happily reſtored and cultivated anew by ſome excellent wits of the preſent age. But it principally owes its re-eſtabliſhment and luſtre to Mr.* Boyle, *that honourable perſon of ever-bleſſed memory, who hath not only ſhewn its uſefulneſs in phyſiology, above the vulgar doctrines of real qualities and ſubſtantial forms, but likewiſe its great ſerviceableneſs to religion itſelf.* Mr. *John Hughes* likewiſe obſerving [y], that Mr. *Boyle* was born the ſame year in which lord *Bacon* died, tells us, that "he was the perſon, who ſeems to have been deſigned by nature, to ſucceed to the labours and inquiries of that extraordinary genius juſt mentioned. By innumerable experiments, he in a great meaſure filled up thoſe plans and outlines of ſcience, which his predeceſſors had ſketched out. His life was ſpent in the purſuit of nature, through a great variety of forms and changes, and in the moſt rational as well as devout adoration of its divine author." *Franceſco Redi*, in one of his *letters* publiſhed in the *fourth volume* of his works at *Florence* 1724, in 4to, expreſſes the higheſt eſteem and veneration for him, and aſſerts, that *he was the greateſt man who ever was, and perhaps ever will be, for the diſcovery of natural cauſes.* But the juſteſt and completeſt character of him is given us by the learned and ingenious Dr. *Shaw*, in the *General Preface* to his *Abridgment of Mr.* Boyle's *Philoſophical Works*, where he tells us [z], that thoſe works *have, from their firſt appearing in public, done an honour to his country, and procured him a general eſteem in the world. The novelty, the variety, the dignity, and the uſefulneſs of the ſeveral ſubjects he treats, with the eaſy and familiar manner wherein they are handled, recommend his performances to the whole body of mankind. Mr.* Boyle *accommodates himſelf as well to the unlearned and the novice, as to the philoſopher and the ſcholar; his whole*

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[w] *Budgell*, p 144, 145.

[x] *Methodus diſcendi medicinam.*

[y] Spectator, No. 54. Vol. VII.

[z] P 1. 2d edit. *London* 1738, in 4to.

ſcope and deſign being, with the utmoſt candor and ſimplicity, to communicate thoſe numerous and important diſcoveries, which coſt him infinite pains and application to make. He afterwards remarks[a], that Mr. *Boyle* takes up his reader at the elements or fundamental principles of things, and with exquiſite judgment conducts him through all the regions of nature, to furniſh him with objects whereon to exerciſe his faculties; and being firſt ſolicitous to make him a general philoſopher, leaves him prepared for any further inquiry he ſhall think fit to make into the works of nature or art. That as the beſt and only ſolid foundation for philoſophy is its uſefulneſs to mankind, and the only way to gain it the reputation it deſerves, muſt be to manifeſt the great advantages that attend the ſtudy thereof; ſo Mr. *Boyle* has been every where careful to ſhew, that nothing can be more ſerviceable and beneficial in life than experimental philoſophy in general. "The men of wit and learning have, *continues Dr.* Shaw, in "all ages, buſied themſelves in explaining nature by words; but it is Mr. *Boyle* alone "who has wholly laid himſelf out in ſhewing philoſophy in action. The ſingle point "he perpetually keeps in view, is, to render his reader not a talkative, or a ſpeculative, "but an actual and practical philoſopher. Himſelf ſets the example; he made all "the experiments he poſſibly could upon natural bodies, and communicates them "with all deſirable candour and fidelity." He then obſerves[b], that Mr. *Boyle* reſtrains not the buſineſs of experimenting to any particular branch of ſcience, but applies it, in full latitude, to all the elements, and all the bodies they mix with, or go to compoſe; and that the air, earth, fire, and water, are all ſcrutiniſed and tortured by experiments to confeſs their natures, offices, uſes, and the wiſdom and deſign of their creation. That he greatly opens and dilates the mind; gives us noble and generous thoughts of nature, and of our own abilities; manifeſts, that even deſperate things may be attempted with ſucceſs; and ſhews no quarter to the lazy, indolent temper of thoſe, who, from ſhallow notions, and a want of having ſeen the powers in natural bodies, are for diſcouraging all new and grand undertakings; and uniting the knowledge of the philoſopher with all the freedom and addreſs of the gentleman, he renders the moſt daring projects promiſing and advantageous. That he will never allow us to conſider the world as a rude heap of dull inactive matter; but convinces us, that it is a grand and noble machine, continually actuated, informed, and governed by a moſt wiſe and beneficent being, who keeps all the parts thereof in motion, and makes them act upon one another, according to certain laws. "Then bringing "us acquainted with theſe laws, he enables us to make uſe of the ſame ſtratagems "and contrivances which nature herſelf employs; which ſurely are capable of performing the greateſt things when rightly applied; that is, when applied as Mr. "*Boyle*, by his own example and experience, directs us. But he ſhews alſo, that in "order to make the moſt advantageous uſe of theſe powers, a general knowledge "of nature is required. Without large and comprehenſive views, without being well "verſed in mechanical arts, and the ſeveral branches of natural knowledge, ſo as to "make one aſſiſting to another, and all in their turn conſpire to the ſame end, Mr. "*Boyle* encourages no man to be a projector[c]." He tells us[d], that Mr. *Boyle* is particularly cautious to guard his reader againſt every operation that might any ways prove dangerous or hurtful; and to prevent his impoſing upon himſelf by any ſuperficial obſervation, any fallacious or contingent experiment, whether made in his own perſon, or delivered by others. And from an attentive peruſal of his admirable writings, Dr. *Shaw* declares, that in general no author appears to have been more inquiſitive into the foundations and evidence of the narratives he receives from others, or to have ſhewn greater ſagacity and induſtry in diſcovering their weak ſides; always frankly propoſing them, as he received them, to be confirmed or overthrown by farther ſearch and future diligence. For proof of this he appeals to the manner wherein Mr. *Boyle* delivers himſelf upon what are or were eſteemed the moſt exceptionable things in his writings; the ſpecific virtue of the Peruvian bark, rue-leaved whitlow graſs, *ens veneris*, *oſteocolla*, the *ens primum* of balm, *Butler*'s ſtone, the ſympathetic powder, the weapon ſalve, the alkaheſt of *Paracelſus*, the *virgula divina*, the tranſmutation of metals, projection, or the philoſopher's ſtone, *&c.* and he deſires, that Mr. *Boyle* may be compared herein with the moſt approved and judicious authors who have treated of the virtues of remedies, the ſecrets in chemiſtry, and the powers of nature. No doubt (ſays he) Mr. *Boyle* may appear to give more into the belief of ſome extraordinary things than the vulgar, or perſons unacquainted with experimental philoſophy, and the immenſe powers and ſtrange operations of nature and art; but the queſtion is, whether he has here gone further than his evidence carried him. He relates alſo, upon his own knowledge, many things very aſtoniſhing and incredible to minds unprepared by philoſophy to entertain them. But he was ſo happy, as to have ſeen with his own eyes much more of the powers of nature and art, than can eaſily be imagined. And if we conſider the numerous phænomena, which, in a very long ſeries of years ſpent upon natural inquiries, moſt of them ſucceſsful, muſt have been preſented him, by the proceſs of his laboratory, the experiments of the pneumatic engine, his mechanical, optical, hydroſtatical, and other kinds of trials; the correſpondence he happily cultivated with the whole learned world, and particularly with the chemiſts of all nations; the various ſecrets he purpoſely kept to exchange for theirs; we ſhall ſee reaſon enough to perſuade us, that he might well be enabled to deliver things that ſhould ſound odd, ſtrange, and ſhocking to vulgar ears. To render the moſt innocent fluids poiſonous, without altering their colour, taſte, ſmell, or any obvious quality; utterly to eraſe the hand-writing in a ſheet of paper or parchment, without at all unfitting either for ink; to make a baſe metal paſs even upon goldſmiths for gold; not to mention the more ſurpriſing and ſhocking experiments of the like ſtamp, are things that can hardly be thought poſſible or practicable by the vulgar; yet ſome chemical philoſophers could perform them with eaſe. But there are ſome philoſophers, and even chemiſts, who, if you ſhould tell them, that Mr. *Boyle* had a certain liquor with which he could convert gold into ſilver; they would beg to be excuſed believing ſo ſtrange a thing. And why? Merely becauſe they never ſaw the like performed, and had tied themſelves down to a narrow ſtrait-laced philoſophy, that would not ſuffer them to look abroad into what nature and art can produce. *But if Mr.* Boyle *(ſays* Dr. *Shaw) has actually done the thing, and others, men of veracity, have aſſured him they had done it too; is he credulous for believing it? Thoſe rather were ſo who, without ſufficient evidence, raſhly concluded the thing impracticable.* The ſame writer then obſerves[e], that ſince Mr. *Boyle* aſſures us, and gives us ſufficient grounds to believe, that he has rendered ſilver, gold, fixed alkalies, *&c.* volatile; converted acids into alkalies; produced light from a deſpicable dark ſubſtance; immediately reſtored fetid and corrupted water, by precipitating from it a ſubſtance not fetid; diſſolved the hardeſt bodies by innocent and potable liquors without heat; produced cold by a mixture of warm bodies, heat by a mixture of cold ones; warmed cold liquors by ice; inſtantly froze ſpirit of wine; converted that ſpirit into water,

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[a] P. 4. 2d edit. *London* 1738, in 4to.

[b] Ibid. p. 6.

[c] Ibid. p. 8.

[d] Ibid. p. 9.

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[e] Ibid. p. 10.

water and fixed alkalies into earth; and performed numberleſs other ſurpriſing things, that appear ſo many contradictions among the unſkilful; may he not, with good reaſon, ſpeak of them as feaſible, and proper to be tried by others? Or may he not, upon the ſtrength of human teſtimony, upon the relation of his friends and acquaintance, believe, that other men have done the ſame, or even greater things than theſe, though himſelf was not an eye-witneſs of them? If, after having made his liquid *phoſphorus* in *England*, he ſhould be aſſured, that another chemiſt had made a ſolid one in *Germany*; if, after having invented his air-pump, he ſhould have heard, that many unſuſpected diſcoveries were made by means of it; if having converted gold into ſilver, eye-witneſſes ſhould inform him, that ſilver has been converted into gold; or if having himſelf prepared a menſtruum almoſt as ſtrange as the alkaheſt, could he be reproached with credulity for believing, that *Paracelſus* or *Helmont* might poſſibly poſſeſs it in greater perfection; or that the other things were done by others? “ This, “ ſurely, *ſays Dr.* Shaw[f], is not unphiloſophical or irrational. Mr. *Boyle* was too “ wiſe to ſet any bounds to nature; he was not forward to ſay, that every ſtrange “ thing muſt needs be impoſſible, becauſe he ſaw ſtrange things every day; though “ he always acknowledges, and it was a common ſaying with him, that *thoſe who had* “ *ſeen them, might better believe them than thoſe who had not.* He modeſtly thought, “ that all we have to do is to keep our eyes open, and expect what nature and art “ will, upon due application, perform. He was well aware that there are powerful “ agents in the world, whoſe laws and manner of acting we want to be acquainted “ with. A man of a different caſt, or an ignorant Indian, who had never looked “ abroad, and ſeen the common phænomena, would ſcarce credit the moſt faithful “ and exact relation that could poſſibly be made him of the the properties and effects “ of the loadſtone, or of gunpowder, by perſons of the greateſt veracity. But his “ ignorance alone is the cauſe of this incredulity. Propoſe the ſame things to a man “ who never heard of gunpowder or the loadſtone, but is otherwiſe a general philo- “ ſopher, and he will readily and thankfully yield his aſſent, and ſolicit you to ſhew “ him the experiments.” The doctor then remarks[g], that we may certainly depend upon this, that what Mr. *Boyle* delivers as an experiment or obſervation of his own, is related in the preciſe manner wherein it appeared to him: no one ever yet denied, that he was a man of punctual veracity. And what he delivers as received from other hands, if it be any thing extraordinary, the reader will find that he does it with a manifeſt diffidence, and in a quite different manner from that wherein he gives us facts upon his own knowledge; and never entertains it for truth, without producing ſomething analogous or parallel to countenance it from his own obſervations or experience; always reſerving to himſelf the full liberty of believing no more than he ſees reaſon for. And what can a man do more than this, to avoid the imputation of credulity? On the contrary, do not thoſe who, without evidence, and merely from common report, believe Mr. *Boyle* to have been credulous, give us a remarkable inſtance of their own credulity? Wherever he believes more than other men, it is becauſe he had more reaſon for it than they. It is the general way indeed, when a ſtrange thing is related, either poſitively to deny it as impoſſible, or directly to entertain it as a truth. This is making ſhort work of it. But Mr. *Boyle* poſſeſſed in a great degree that noble faculty of ſuſpending his judgment, till by all the enquiries he could any way make he received fuller information; and then the thing he found would determine itſelf.

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[f] Ibid. p. 11.

[g] Ibid. p. 12.

DR. *Shaw* obſerves next[h], that Mr. *Boyle* is altogether taken up with the objects around us, and of which we are or may be put in poſſeſſion; though we remain ignorant of their properties, virtues, and uſes, till he calls them out, ſets them before us, and ſurprizes us with a ſight of our own ignorance of things ſo near us, ſo momentous in themſelves, and ſo neceſſary to our well-being and the true enjoyment of life. There is no profeſſion or condition of men but may be benefited by his diſcoveries. As he had a wonderful comprehenſive genius himſelf, he has improved every part of natural knowledge, and the world is more obliged to this ſingle man, than to a thouſand vulgar philoſophers taken together. It is certain, that he laid the foundations of almoſt all the improvements which have been made ſince his time in natural philoſophy; and actually himſelf performed abundance of thoſe very things, and perhaps in a much better manner too, whereby ſeveral famous men have gained a reputation in putting them off for their own diſcoveries. A very fine collection of uſeful knowledge, publiſhed as the works of a foreign Society, bears a remarkable teſtimony to this truth. The mechanic, the merchant, the ſcholar, the gentleman, all are benefited by Mr. *Boyle*. He ſhews us trades in a new light, and makes them, what they really are, a part of natural philoſophy; and conſidering them accordingly, reveals ſome of their myſteries; all along advancing proper means to encourage, promote, and multiply the arts themſelves. The goldſmith, the lapidary, the jeweller, the refiner, the ſtone-cutter, the dyer, the glaſs-maker, artizans of all kinds, will from him receive the beſt informations as to the working, managing, and employing to advantage their various commodities, materials, engines, and inſtruments. The huſbandman and the diver are here inſtructed in their arts; and the mineraliſt, the miner, and aſſayer, to find and ſeparate their ore to the greateſt profit; to increaſe the quantity; to meliorate, improve, and enrich their metals; to purify and fine them, and accurately to diſtinguiſh the genuine and pure from the adulterate, baſe, and counterfeit. The architect and builder are ſhewn how to chooſe the beſt materials for their ſeveral purpoſes; the painter to make, to mix, and improve his colours; and no part of mankind is neglected by Mr. *Boyle*. But he ſhews a more particular regard to thoſe profeſſions, wherein the health of the ſpecies is nearly concerned. The phyſician, the anatomiſt, the apothecary, and the chemiſt, are moſt highly indebted to him. He has conſidered and improved the art of medicine in all its branches. We owe to him the beſt ways we have of diſtinguiſhing genuine drugs from adulterate; the diſcovery and preparation of ſeveral valuable medicines, with the manner of applying abundance to good advantage. He has ſhewn us the way wherein ſpecifics may act; how to judge of the wholeſomeneſs and unwholeſomeneſs of the air, water, and of places; and how to examine and make choice of mineral ſprings. In a word, there is ſcarce an art, or natural production known, but he makes ſome uſeful diſcovery or improvement in it.

HIS temper, according to Dr. *Shaw*[i], was moſt open, candid, generous, and communicative. He endeavours to make all the things he treats of plain, eaſy, and familiar. There is no deep knowledge in mathematics or algebra previouſly requiſite to underſtand him fully; no tedious ſyſtems need be read, to prepare us for reaping all the advantages of his philoſophy. He brings us at an eaſy rate acquainted with the moſt uſeful things in nature; and this all the world agrees to be the higheſt excellence in a philoſophical writer. His memory will be dear to poſterity, whilſt that of myſtical and enigmatical writers, ſuch as ſtudy to make things difficult, and envy mankind their knowledge, will meet with the diſregard and contempt they deſerve. What principally recommends him, and diſtinguiſhes him from the vulgar herds of chemiſts, naturaliſts, and philoſophers, is this humane temper, this open, candid, generous, and beneficent diſpoſition. He was at immenſe pains and charge in making his enquiries; he ſpared no time, no money, no diligence, in purſuing diſcoveries for the public advantages, without any ſordid view to increaſe his own fortune, which he happily thought ſufficient of itſelf. His beneficent and public ſpirit made the world a generous preſent of all the fruits of his labours, without the leaſt expectation of reward from them. His ſoul was as great and noble as his genius was comprehenſive, or his invention fruitful. And what ſhews him in the moſt amiable point of light, he was far above the ſelfiſh pleaſure of being admired for a genius, or raiſing a reputation by his diſcoveries. Though he wanted no capacity or abilities to have worked up a glorious ſyſtem, and erected a more pompous, oſtentatious, and perhaps a more durable ſtructure of natural and chemical philoſophy than had ever appeared in the world before, he nobly deſpiſed this poor ſatisfaction and mean gratification, telling us plainly and expreſsly, that, notwithſtanding all he had done, all the labour, pains, and expence beſtowed in a life of natural enquiries, notwithſtanding the vaſtly numerous and important obſervations and diſcoveries he made, he ſaw nothing but the firſt dawnings of ſcience; has drawn only the rudiments of natural knowledge, and leaves it in charge to poſterity, for their own ſakes, to conſider him but as a beginner, and to purſue philoſophical enquiries in general, without ſtopping to raiſe petty ſyſtems by the way. "Here, *concludes Dr.* Shaw[k], was a noble ſoul! "This the deſirable character! A true philoſophical mind, well ſeaſoned with humanity, beneficence, goodneſs! After he had led us through all the regions of nature; "conſidered her various productions; ſhewed us their uſes, and the manner of converting them to our ſeveral purpoſes; convinced us, that we live in a world moſt "wiſely contrived, wherein numberleſs grand deſigns are at once carried on with unceaſing variety; and manifeſted, that all the beings, and all the bodies we know, "jointly conſpire, as one whole, in bringing about the great ends of nature; he bids "us not ſtop here, but leaves us full of aſſurance, that the further we enquire into "the works of the univerſal architect, the more beauty and harmony, the greater uſe "and ſatisfaction we ſhall find among them; and this as long as the frame of the "world endures. Theſe noble, manly, and generous notions, are what the reader "will find inculcated through the philoſophical writings of Mr. *Boyle*; whoſe principal aim and great delight it was to benefit mankind by them."

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[h] Ibid. p. 13.

[i] Ibid. p. 14.

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[k] Ibid. p. 15.

APPENDIX

APPENDIX

TO THE

LIFE of the Honourable *Robert Boyle*.

NUMBER I.

The Charter of the Corporation for propagating the Goſpel in *New England* and the Parts adjacent in *America*, of which Mr. *Boyle* was the firſt Governor. *Life*, p. lxviii.

CHARLES the Second by the grace of God, king of *England*, *Scotland*, *France* and *Ireland*, defender of the faith, *&c.* To all, to whom theſe preſents ſhall come, greeting. Whereas by the ſeveral navigations, diſcoveries, and ſucceſsful plantations of divers of our loving ſubjects of this our realm of *England*, the empire and dominion of us and our royal progenitors and predeceſſors, hath, by the bleſſing of Almighty God, been augmented and enlarged, as well upon the main land and continent of *America*, as upon ſeveral iſlands and promontories thereof, and the trade and commerce between *England* and thoſe colonies and plantations hath of late years been very much increaſed, and by reaſon thereof, and of the pains and induſtry of certain *Engliſh* miniſters of the Goſpel, and others reſiding in or near our colonies and plantations in *New England*, who having attained to ſpeak the language of the heathen natives in thoſe parts, have, by their teaching and inſtructions, brought over many of them from the power of darkneſs, and the kingdom of Satan, to the knowledge of the true and only God, and to an owning and profeſſing of the proteſtant religion (whereof we have of late been more fully informed by the humble petition of divers miniſters and others our loving ſubjects now reſiding in this our kingdom of *England*). And although a large door of hope be hereby opened to us for the glorifying of the name of Jeſus Chriſt, and the further enlargement of his church, yet unleſs ſome due and competent proviſion be made to lay a foundation for the educating, clothing, civilizing and inſtructing the poor natives; and alſo for the ſupport and maintenance of ſuch miniſters of the Goſpel, ſchoolmaſters, and other inſtruments, as have been, are, or ſhall be ſet apart and employed for the carrying on of ſo pious and Chriſtian a work, the ſame may be much retarded, and a work, ſo happily begun, diſcouraged; thoſe planters, who firſt began, and contributed largely thereunto, being of themſelves unable to bear the whole charge thereof. And whereas we are reſolved, not only to ſeek the outward welfare and proſperity of thoſe colonies by putting an induſtrious people into a way of trade and commerce, that they may be employed and improved, for their own and the common benefit of theſe our kingdoms, but more eſpecially to endeavour the good and ſalvation of their immortal ſouls, and the publiſhing the moſt glorious Goſpel of Chriſt amongſt them; and to the end that ſuch our loving ſubjects, as either have already been aiding herein, or as ſhall hereafter be willing to contribute hereunto, may not be diſcouraged in their intended charity, for want of ſufficient authority and patronage from us faithfully to order and diſpoſe all and every ſum and ſums of money, goods, chattels, lands, tenements, or hereditaments, that have been, or ſhall or may be given for the purpoſes aforeſaid; know ye, that we, of our princely piety, and for the further propagation of the Goſpel of Jeſus Chriſt, amongſt the heathen natives in or near *New-England* and the parts adjacent in *America*; and for the better civiliſing, educating and inſtructing of the ſaid heathen natives in learning, and in the knowledge of the true and only God, and in the proteſtant religion, already owned and publicly profeſſed by divers of them; and for the better encouragement of ſuch others of them as ſhall embrace the ſame, and of them and their poſterity after them, to abide and continue in, and hold faſt the ſaid profeſſion; of our eſpecial grace, certain knowledge, and mere motion, and for divers other good and pious cauſes and conſiderations us thereunto eſpecially moving, do, for us, our heirs and ſucceſſors, will, ordain, conſtitute and declare, by theſe preſents, that there be, and for ever hereafter ſhall be, within this our kingdom of *England*, a ſociety, or company, for propagation of the Goſpel in *New England*, and the parts adjacent in *America*. And further, we do, for us, our heirs, and ſucceſſors, will, ordain, conſtitute, and appoint, that our right truſty and right well beloved couſin and counſellor, *Edward*, earl of *Clarendon*, lord chancellor of *England*, our right truſty and right well beloved couſin and counſellor, *Thomas*, earl of *Southampton*, lord high treaſurer of *England*, our right truſty and well beloved counſellor, *John*, lord *Roberts*, lord privy ſeal, our right truſty and right well beloved couſin and counſellor, *George*, duke of *Albemarle*, our right truſty and right well beloved couſin and counſellor, *James*, duke of *Ormonde*, our right truſty and right well beloved couſin and counſellor, *Edward*, earl of *Mancheſter*, lord chamberlain of our houſhold, our right truſty and right well beloved couſin and counſellor, *Arthur*, earl of *Angleſey*, our right truſty and well beloved counſellor, *William*, viſcount *Say* and *Seale*, our well beloved *Francis Warner*, alderman of *London*, *Eraſmus Smith*, Eſq; *Henry Aſhurſt*, *Richard Hutchinſon*, *Joſhua Woolnough*, *George Clarke*, *Thomas Speed*, *Thomas Bell*, *John Rolfe*, citizens of *London*, our truſty and well beloved *Robert Boyle*, Eſq; Sir *William Thompſon*, Sir *William Bateman*, Sir *Anthony Bateman*, Sir *Theophilus Biddolph*, Sir *Lawrence Bromfield*, Knights, *Tempeſt Milner*, *William Love*, *William Peake*, alderman of *London*, *Thomas Foley*, Eſq; *Thomas Cox*, *John Micklethwait*, *Edmund Trench*, doctors in phyſic, and our well beloved *Charles Doyley*, *Thomas Staynes*, *John Jurian*, *William Antrobus*, *John Bathurſt*, *Harman Sheafe*, *Thomas Gillibrand*, *James Hayes*, *John Benbowe*, *Lawrence Brinſley*, *Barnabas Meares*, *John Acrod*, *John Dockett*, *Edward Boſcowen*, and *Martin Noell*, citizens of *London*, to be the firſt members and perſons, whereof the ſaid company ſhall conſiſt. And that they, the ſaid *Edward*, earl of *Clarendon*, *Thomas*, earl of *Southampton*, *John*, lord *Roberts*, *George*, duke of *Albemarle*, *James*, duke of *Ormonde*, *Edward*, earl of *Mancheſter*, *Arthur*, earl of *Angleſey*, *William*, viſcount *Say* and *Seale*, *Francis Warner*, *Eraſmus Smith*, *Henry Aſhurſt*, *Richard Hutchinſon*, *Joſhua Woolnough*, *George Clarke*, *Thomas Speed*, *Thomas Bell*, *John Rolfe*, *Robert Boyle*, Sir *William Thompſon*, Sir *William Bateman*, Sir *Anthony Bateman*, Sir *Theophilus Biddolph*, Sir *Lawrence Bromfield*, *Tempeſt Milner*, *William Love*, *William Peake*,

Peake, *Thomas Foley*, *Thomas Cox*, *John Micklethwait*, *Edmund Trench*, *Charles Doyley*, *Thomas Staynes*, *John Jurian*, *William Antrobus*, *John Bathurſt*, *Harman Sheafe*, *Thomas Gillibrand*, *James Hayes*, *John Benbowe*, *Lawrence Brinſley*, *Barnabas Meares*, *John Acrod*, *John Dockett*, *Edward Boſcowen*, and *Martin Noell*, and their ſucceſſors, to be hereafter choſen into the ſaid company, and in ſuch manner, as is hereafter in theſe preſents directed, for ever hereafter be, and ſhall be by virtue of theſe preſents, one body corporate and politic, in deed and in name, and ſhall have continuance for ever, by the name of the company for propagation of the Goſpel in *New England* and the parts adjacent in *America*. And them the ſaid *Edward*, earl of *Clarendon*, *Thomas*, earl of *Southampton*, *John*, lord *Roberts*, *George*, duke of *Albemarle*, *James*, duke of *Ormonde*, *Edward*, earl of *Mancheſter*, *Arthur*, earl of *Angleſey*, *William*, viſcount *Say* and *Seale*, *Francis Warner*, *Eraſmus Smith*, *Henry Aſhurſt*, *Richard Hutchenſon*, *Joſhua Woolnough*, *George Clarke*, *Thomas Speed*, *Thomas Bell*, *John Rolfe*, *Robert Boyle*, Sir *William Thompſon*, Sir *William Bateman*, Sir *Anthony Bateman*, Sir *Theophilus Biddolph*, Sir *Lawrence Bromfield*, *Tempeſt Milner*, *William Love*, *William Peake*, *Thomas Foley*, *Thomas Cox*, *John Micklethwait*, *Edmund Trench*, *Charles Doyley*, *Thomas Staynes*, *John Juriam*, *William Antrobus*, *John Bathurſt*, *Harman Sheafe*, *Thomas Gillibrand*, *James Hayes*, *John Benbowe*, *Lawrence Brinſley*, *Barnabas Meares*, *John Acrod*, *John Dockett*, *Edward Boſcowen*, and *Martin Noell*, and their ſucceſſors, by the name of the company for propagation of the Goſpel in *New England*, and the parts adjacent in *America*, we do, for us, our heirs and ſucceſſors, fully and really create, erect, make, ordain, eſtabliſh, conſtitute, and appoint, to be one body corporate and politic, to have continuance for ever to them and their ſucceſſors. And that by the ſame name they, and their ſucceſſors, ſhall and may have perpetual ſucceſſion, and from time to time (as occaſion ſhall require) aſſemble, and meet together, in ſome convenient place, within the city of *London*, for the ends aforeſaid. And that they and their ſucceſſors, by the name of the company for propagation of the Goſpel in *New-England*, and the parts adjacent in *America*, be for ever hereafter perſons able and capable in the law to purchaſe, take, have, hold, receive, and enjoy any manors, lands, tenements, liberties, privileges, juriſdictions and hereditaments whatſoever, of what kind, quality, or nature ſoever they be, ſituate and being either within our kingdom of *England*, or elſewhere, within any other of our dominions and territories to them, and their ſucceſſors, in fee and perpetuity, or for term of life, or lives, or years, or otherwiſe, in what ſort ſoever; ſo as the ſame exceed not, in lands and hereditaments of inheritance, the clear yearly value of two thouſand pounds; the ſtatute for not putting of lands or tenements in mortmain, or any thing therein contained, or any other act, or ſtatute, to the contrary, notwithſtanding. And alſo all manner of goods and chattels, ſum and ſums of money, and other things whatſoever, of what nature, or quality, ſoever they be. And alſo to give and grant, demiſe, let, aſſign, alien and diſpoſe of all or any of the ſaid manors, lands, tenements, or hereditaments, goods or chattels. And alſo to do, perform, and execute, all and every other lawful act and acts, thing and things, whatſoever, by the name of the company for propagation of the Goſpel in *New-England*, and the parts adjacent in *America*. And further, that the ſaid company, and their ſucceſſors, by the name aforeſaid, ſhall and may be perſons able and capable, in the law, to plead, and to be impleaded, to anſwer, and to be anſwered unto, to defend, and to be defended, in what court, or courts, ſoever, and before any judges, or juſtices, and other perſons, and officers of us, our heirs and ſucceſſors whatſoever, in all and ſingular actions, pleas, ſuits, plaints, matters, and demands, of what kind, nature, or quality, ſoever they be, or ſhall be, in the ſame, and in the like, and in as ample manner and form, as any other the people of this our kingdom of *England*, or any other body, corporate or politic, within the ſame our kingdom, may or can have, hold, take, purchaſe, poſſeſs, enjoy, retain, give, grant, demiſe, let, alien, diſpoſe, aſſign, plead, and be impleaded, anſwer, and be anſwered unto, defend, or be defended, do, perform, or execute. And that they the ſaid company for propagating of the Goſpel in *New England* and the parts adjacent in *America*, and their ſucceſſors, ſhall and may for ever hereafter have a common ſeal, to ſerve and uſe, for all cauſes, matters, things, and affairs whatſoever, of them and their ſucceſſors. And that it ſhall and may be lawful to and for any thirteen, or more of them, whereof the governor of the ſaid company, for the time being, to be one, to appoint, alter, and make new the ſaid ſeal, from time to time, at their wills and pleaſures, as they ſhall think fit. And further, we will and ordain, and by theſe preſents, for us, our heirs and ſucceſſors, do declare and appoint, that there be, for the better ordering and managing of the affairs and buſineſs of the ſaid company, and their ſucceſſors, and for ever ſhall be, one of the members of the ſaid company, and their ſucceſſors, who ſhall be, and ſhall be called the governor of the ſaid company; and to hold the ſaid office and place of governor of the ſaid company and their ſucceſſors, ſo long as he the ſaid governor ſhall well demean himſelf in the ſaid office and place, and that the ſaid governor ſhall have hereby power, from time to time, to ſummon, or cauſe to be ſummoned, any courts or meetings, of the ſaid company, as often as occaſion ſhall require. And we do, for us, our heirs and ſucceſſors, aſſign, name, conſtitute, and appoint the aforeſaid *Robert Boyle*, to be the firſt and preſent governor of the ſaid company and their ſucceſſors, to hold and exerciſe the ſaid office, for ſo long time as he the ſaid *Robert Boyle* ſhall well behave himſelf therein. And therefore we will, and by theſe preſents, for us, our heirs and ſucceſſors, do give and grant unto the ſaid company for propagation of the Goſpel in *New-England* and the parts adjacent in *America*, and their ſucceſſors, that upon the death or removal of the ſaid governor, for the time being (whom for evil government, frequent neglect of attendance, or any other juſt and reaſonable cauſe, we will, and by theſe preſents do, for us, our heirs and ſucceſſors, declare, ſhall, and may, from time to time, be removed by the ſaid company, or any thirteen of them) it ſhall and may be lawful to and for the ſaid company, or any thirteen of them, as aforeſaid, at their wills and pleaſures, to elect and chuſe any other members of the ſaid company, for the time being, into the place of ſuch governor, as ſhall be dead, or removed as aforeſaid, to hold and exerciſe the ſaid office, or place, whereunto he ſhall be ſo elected and choſen as aforeſaid, for ſo long time as he ſhall and do reſpectively well demean himſelf therein. And alſo, in caſe of the abſence of the governor, for the time being, whereby either the meeting of the ſaid company cannot well be ſummoned, or held, or being ſummoned, cannot proceed, or act therein; that then the treaſurer of the ſaid company, for the time being, to be choſen, as hereafter is expreſſed, ſhall, and hath hereby power and authority to ſummon ſuch meetings; and then any five or more of the ſaid company, ſo meeting together, ſhall and may have power to appoint one of the ſaid members, ſo meeting, to ſupply the place and office of the governor, for the preſent. And to the end alſo the ſaid company may have a continual ſucceſſion, and may be the better ſupplied, from time to time, hereafter, with fit and able perſons to be members of the ſaid company, in the place and places of ſuch of the preſent members of the ſaid company herein above named, or any others that ſhall hereafter be choſen to be members of the ſaid company, and ſhall hereafter happen to die, or, as ſhall, by reaſon of their own affairs, or otherwiſe, deſire to be diſcharged, or otherwiſe ſhall be removed from being any longer members of the ſaid company, we do likewiſe, for us, our heirs and ſucceſſors, by theſe preſents, give and grant unto the ſaid company for propagation of the Goſpel in *New-England* and the parts adjacent in *America*, and their ſucceſſors, that it ſhall and may be lawful to and for the ſaid governor, for the time being, and company, and their ſucceſſors, or any thirteen or more of them, whereof the governor, for the time being, to be one, to diſcharge or remove any ſuch perſon or perſons, from being any longer member or members, of the ſaid company as aforeſaid; and by like order, at their wills and pleaſures, to admit into the ſaid company, in the place of any of the ſaid member or members, that ſhall be dead or removed, as aforeſaid, any other, or ſo many, fit and diſcreet perſon and perſons, as they ſhall ſee cauſe; ſo as the number of the ſaid perſon and perſons, to be at any time hereafter newly admitted, and to be members of the ſaid company, together with the then remaining members of the ſaid company, for the time being, do not in the whole exceed the number of five and forty perſons. And further, we will and ordain, and, by theſe preſents, for us, our heirs and ſucceſſors, do give and grant unto the ſaid company for propagating of the Goſpel in *New-England* and the parts adjacent in *America*, and their ſucceſſors, that they, or any thirteen or more of them (whereof the governor of the ſaid company, for the time being, to be one) ſhall and may elect and chuſe one or more fit and able perſon and perſons, member or members, of the ſaid company, to be treaſurer or treaſurers, of the ſaid company, for the time being; and one fit perſon to be clerk of the ſaid company, to write and ſerve for the affairs of the ſaid company and their ſucceſſors; and all and every ſuch other fit perſon and perſons, and officers, as they ſhall find meet and neceſſary, to be ſerviceable and uſeful to them, in ſuch matters and affairs, touching and concerning the ſaid company and their ſucceſſors, as he, or they, ſhall be employed in, and as they ſhall ſee cauſe, to allow them ſuch ſalaries and allowances, in execution of the ſaid reſpective places, as they ſhall think meet. And the ſaid treaſurer and treaſurers, clerk, and all other officers, as aforeſaid, to be ſo choſen by them, for reaſonable and juſt cauſe, to diſplace and remove out of their place or places, and other meet perſons, in their or any of their places, at the pleaſure and diſcretion of the ſaid company, and their ſucceſſors, or any nine or more of them, ſo met and aſſembled, to chuſe and elect. And we do likewiſe, for us, our heirs and ſucceſſors, by theſe preſents, further give and grant unto the ſaid company, and their ſucceſſors, that the ſaid company, and their ſucceſſors, or any nine or more of them (whereof the governor of the ſaid company, for the time being, to be always one) ſhall and may have full power and authority, by virtue of theſe preſents, to make, ordain, conſtitute, appoint, and ſet down, from time to time, ſuch reaſonable acts, orders, inſtructions, and conſtitutions, in writing, as to them, or any nine, or more of them (whereof the governor of the ſaid company, for the time being, to be always one) ſhall ſeem fit, good, wholeſome, profitable, honeſt, neceſſary and convenient, according to their ſound diſcretions, for the ſupport of the ſaid company and their ſucceſſors; and for directing how and in what manner the ſaid governor, treaſurer, and treaſurers, clerk, and other officers and perſons, for the time being, truſted, and to be truſted and employed by the ſaid company and their ſucceſſors, ſhall and ought to demean, bear, and carry themſelves in their offices, places, and truſts, reſpectively; and for and concerning the managing, ordering, and diſpoſing of all and every the manors, lands, leaſes, tenements, hereditaments, goods, chattels, money, or ſtock of the ſaid company and their ſucceſſors, and for and concerning all and every other matters and things, incident

cident unto, or that ſhall or may concern the ſame, or any other the matters, or affairs, of the ſaid company and their ſucceſſors; and the ſame acts, orders, inſtructions, and conſtitutions, ſo made, and to be made, to revoke, alter, or change, and others, or new ones, to agree, make, ordain, conſtitute, appoint, and ſet down, in writing, as aforeſaid, as they, or any nine or more of them, as aforeſaid, ſhall ſee cauſe; ſo as the ſaid acts, orders, inſtructions, and conſtitutions, or any of them, be not repugnant or contrary to the laws and ſtatutes of *England*, but thereunto conformable and agreeable. Nevertheleſs, our intent and meaning is, that the governor of the ſaid company, for the time being, with any five or more of the other members of the ſame, ſhall have power, from time to time, upon all emergent occaſions, to allow of all incident charges, for and touching the meetings of the ſaid company, and other neceſſary expences, for the better managing of the affairs of the ſaid company. And to the end, that the yearly revenues, iſſues, and profits of all and every the manors, lands, leaſes, tenements, and hereditaments, and alſo the goods, chattels, money, and ſtock, of the ſaid company, and their ſucceſſors, may, from time to time, be faithfully laid out, diſpoſed, employed, and applied, for the promoting and propagating of the Goſpel of Chriſt unto and amongſt the heathen natives, in or near *New-England* and parts adjacent in *America*; and alſo for civilizing, teaching and inſtructing the ſaid heathen natives, and their children, not only in the principles and knowledge of the true religion, and in morality, and the knowledge of the *Engliſh* tongue, and in other liberal arts and ſciences; but for the educating and placing of them, or their children, in ſome trade, myſtery, or lawful calling, we do ordain, and for us, our heirs and ſucceſſors, do, by theſe preſents, give and grant full power and authority to the ſaid governor, or any thirteen or more of the ſaid company, for the time being, under the common ſeal of the ſaid company and their ſucceſſors, from time to time, to nominate, conſtitute, and appoint, ſuch and ſo many fit and meet perſon and perſons, reſiding in or near any of the colonies or plantations in *New-England* aforeſaid, and parts adjacent in *America*, to be commiſſioners for and on the behalf of the ſaid company and their ſucceſſors, to treat, contract, and agree with ſuch miniſters, ſchoolmaſters, and others, reſiding, and to reſide in any the parts aforeſaid, for ſuch ſalaries, allowances, and recompences, to be from time to time made, given, and paid to them, and every of them, for their labour, pains, and induſtry, to be taken by them and every of them, in the duties and employments aforeſaid. And alſo to treat, contract, and agree with any other perſon or perſons, there, for clothes, books, tools, implements, and other neceſſaries, for the civilizing, employing, educating, or placing out any of the ſaid natives, or their children, that ſhall own and profeſs the proteſtant religion, in Engliſh families, or with and under Engliſh maſters there, or otherwiſe, in ſuch manner, as they, the ſaid commiſſioners, in their good diſcretions, ſhall, from time to time, think fit, and to content, pay, and ſatisfy all ſuch contracts, bargains, and agreements, and all ſalaries, wages, and allowances, to ſuch miniſters, ſchoolmaſters, and officers, as they ſhall ſo contract and agree with and for the clothing and apparelling of any of the ſaid natives, or their children, and for books, tools, implements, and other neceſſaries, for them, and for educating or placing them, or any of them, with, or under, any Engliſh maſter or maſters, there, in any trade, myſtery, or lawful calling, out of ſuch monies, goods, and chattels, as ſhall be, from time to time, ſent, or made over, unto the ſaid commiſſioners, ſo to be appointed as aforeſaid, or any of them, out of *England*, or any other the dominions of us, our heirs and ſucceſſors, by the ſaid company and their ſucceſſors, unto *New-England* aforeſaid, or any the parts adjacent in *America*, as aforeſaid; and alſo to do, perform and execute, all and every other act and acts, matters and things, which ſhall, or may, any way tend or conduce to the ends aforeſaid, in ſuch manner, and according to ſuch orders and inſtructions, as the ſaid commiſſioners, ſo to be employed and intruſted, ſhall, from time to time, receive from the ſaid governor, or any other nine or more of the ſaid company, for the time being; which commiſſioners, ſo to be appointed, ſhall, from time to time, give an account, in writing, of their proceedings herein, ſo often as they ſhall be thereunto required, by the ſaid governor, or any other nine of the ſaid company. And that a ſupply of foreign coin or monies, according to the abilities of the ſaid company and their ſucceſſors, may not be wanting, to be employed for and towards the ends and purpoſes aforeſaid, we do, by theſe preſents, for us, our heirs and ſucceſſors, give and grant free licence, and full power and authority to the ſaid company for propagating of the Goſpel in *New-England*, and the parts adjacent in *America*, and their ſucceſſors, by direction, from time to time, of the governor, and any five or more of the ſaid company, for the time being, to ſhip and carry, or cauſe to be ſhipped and carried, in any Engliſh ſhip or veſſel, within any the ports of this our kingdom of *England*, any quantity or quantities, of Spaniſh, or other foreign ſilver coin, not exceeding in the whole, in any one year, the value of one thouſand pounds, without any charge or cuſtom, for the ſame, ſo as, before ſuch ſhipping, the ſame, or any part thereof, to be ſo carried as aforeſaid, the quantity and quantities, and the value and values, of the ſaid foreign coin, ſo to be ſhipped and carried, be firſt duly entered in the cuſtom-houſe of us, our heirs and ſucceſſors, of ſuch our port or ports, of this our kingdom of *England*, where the ſaid foreign coin ſhall be ſo ſhipped, to be carried to *New-England* and parts adjacent in *America*, for any the ends or purpoſes abovementioned; any law, ſtatute, act, ordinance, proviſion, proclamation, or reſtraint, to the contrary, in any wiſe, notwithſtanding. And to the end, that what the ſaid company ſhall be ſeized, intereſted, or poſſeſſed of, may, from time to time, be faithfully improved, applied, and diſpoſed, for the ends, intents and purpoſes herein before declared, and for the neceſſary affairs and buſineſs of the ſaid company and their ſucceſſors only, and no other, we will, and, by theſe preſents, for us, our heirs and ſucceſſors, do, require and ordain, that the ſaid company and their ſucceſſors do yearly, and every year (if they ſhall be thereunto required) by and upon the order and warrant of the chancellor, or keeper of the great ſeal of *England* of us, our heirs and ſucceſſors, and of the treaſurer of *England* of us, our heirs and ſucceſſors, and of the chief baron of the court of exchequer of us, our heirs and ſucceſſors, at *Weſtminſter*, for the time being, or any two of them, make, deliver, and declare, a true and perfect account, before the ſaid chancellor or keeper of the great ſeal, treaſurer and chief baron, for the time being, as aforeſaid, or any two of them, of all and every the goods, chattels, and ſtock of the ſaid company and their ſucceſſors, and alſo of the rents, iſſues and profits of all and every the manors, lands, leaſes, tenements, and hereditaments, and alſo of all and every ſum and ſums of money, received, iſſued and paid, by or for the uſe of the ſaid company and their ſucceſſors; which ſaid account and accounts, as alſo the account of all and every perſon and perſons heretofore employed for the receiving or diſpoſing any rents, profits, or ſums of money, limited or appointed for propagating the Goſpel in *New-England*; we do, for us, our heirs and ſucceſſors, authoriſe and require the ſaid chancellor, or lord keeper, treaſurer, and chief baron, for the time being, or any two of them, to hear, determine, and declare; and, if they ſhall find juſt cauſe, fully to ratify, confirm, and allow of, all and every the receipts, payments, and diſburſements, in every ſuch account and accounts, to be continued, made, and applied to or for the uſes, intents and purpoſes aforeſaid; and after ſuch declaration and allowance of every ſuch account and accounts, to ſign the ſame, under their or any two of their hands, and to deliver, or cauſe to be delivered, the ſaid account and accounts, ſo to be declared, determined, and allowed of, unto the remembrancer of us, our heirs and ſucceſſors, of the ſaid exchequer, for the time being, to remain on record, in the cuſtody of the ſaid remembrancer, for our uſe and information in the premiſes, without any fee or reward, to be given, or paid, by the ſaid company and their ſucceſſors to the ſaid remembrancer, or any other perſon or perſons, for the receiving and ſafe keeping of the ſaid account and accounts, or any of them, or otherwiſe than for the entry and writing thereof. And laſtly, we will, and, by theſe preſents, for us, our heirs and ſucceſſors, do grant unto the ſaid company for propagating the Goſpel in *New-England* and the parts adjacent in *America*, and their ſucceſſors, that theſe our letters patents, or the inrollment, or exemplification of the ſame, and all and every the matters and things, powers, licences, and authorities, in the ſame contained, ſhall be, from time to time, good, valid, ſufficient, and effectual in the law, in and by all things, according to the true intent and meaning thereof; and ſhall be conſtrued, adjudged and taken, as well in all our courts within *England*, or elſewhere, moſt beneficially and largely, and for the benefit and advantage of the ſaid company and their ſucceſſors, any law, ſtatute, act, ordinance, proviſion, proclamation, or reſtraint, to the contrary heretofore had, made, ordained, provided, or publiſhed, or any other matter or thing whatſoever to the contrary in any wiſe notwithſtanding. In witneſs whereof we have cauſed theſe our letters to be made patent; witneſs ourſelf, at *Weſtminſter*, the ſeventh day of February, in the fourteenth year of our reign.

By writ of privy ſeal.

HOWARD.

NUMBER II.

An exact copy of the laſt will and teſtament of the honourable *Robert Boyle*. *Life*, p. cxxxiii.

IN the name of God, Amen. I *Robert Boyle*, of *Stalbridge*, in the county of *Dorſet*, Eſq; youngeſt ſon of the late right honourable *Richard*, earl of *Corke*, deceaſed, being, God be praiſed, of good and perfect memory, and taking into due and ſerious conſideration the certainty of death, and the uncertainty both of the time and manner of it; being likewiſe deſirous, when I come to die, to have nothing to do but to die Chriſtianly, without being hindered by any avoidable diſtractions from employing the laſt hours of my life in ſending up my deſires and meditations before me to heaven, do, this eighteenth day of July in the third year of the reign of our ſovereign lord and lady *William* and *Mary*, by the grace of God King and

Queen of *England*, *Scotland*, *Frvnce*, and *Ireland*, defenders of the faith, *&c.* and in the year of our Lord God one thouſand ſix hundred ninety and one, make and ordain this my laſt will and teſtament in writing, in manner and form following.

FIRST and chiefly, I commend my ſoul to Almighty God, my Creator, with full confidence of the pardon of all my ſins in and through the merits and mediation of my alone Saviour Jeſus Chriſt; and my body I commit to the earth, to be decently buried within the cities of *London* or *Weſtminſter*, in caſe I die in *England*, without eſcutcheons, or unneceſſary pomp, and without any ſuperfluous ceremonies, and without the expence of above two hundred and fifty pounds, and alſo without being unneceſſarily diſſected or diſembowelled; and if it ſhall appear neceſſary, that my body be opened, then my will is, that it be performed very privately, and in the preſence of none but the phyſician, or chirurgeon, and his attendants, and of my domeſtics, or very near relations.

AND as touching my temporal eſtate, wherewith God of his goodneſs hath been pleaſed to endow me, I diſpoſe thereof in manner and form following; that is to ſay;

I GIVE and bequeath unto my dear ſiſter, the lady *Catharine*, viſcounteſs *Ranelagh*, a ſmall ring, uſually worn by me on my left hand, having in it two ſmall diamonds, with an emerald in the middle; which ring being held by me, ever ſince my youth, in great eſteem, and worn for many years for a particular reaſon, not unknown to my ſaid ſiſter, the lady *Ranelagh*, I do earneſtly beſeech her, my ſaid ſiſter, to wear it in remembrance of a brother, that truly honoured, and moſt dearly loved her.

ITEM, I give to the ſaid lady *Ranelagh* all my manuſcripts and collections of receipts, whether of my own hand-writing, or others, unleſs I do otherwiſe diſpoſe of all or any of them before my deceaſe; beſeeching her to have a care, that they or any of them come not to the hands or peruſal of any, to whom ſhe thinks, that if I were alive, I ſhould be unwilling to have them communicated. And I do likewiſe deviſe unto her the ſum of 300 *l.* (and all intereſt) which I formerly lent her upon enlarging the dwelling-houſe in the *Pall-Mall*, which ſhe now lives in.

ITEM, whereas by my deceaſe the greateſt part of my intailed lands in *Ireland*, will come to my dear and eldeſt brother, *Richard*, now earl of *Burlington* and *Corke*, I do hereby give and bequeath unto him a Sardonyx ſeal ring, which I uſually wear on my little finger; beſeeching him to accept of it, and wear it for my ſake, as a teſtimony of my unfeigned affection, and of my ſenſe of his great kindneſs and many favours towards me; and beg of him to believe, that it doth not afflict me, that I have not children of my own to inherit my intailed lands, ſince they are, by that defect, to return to him, the truly honourable head of our houſe and family.

ITEM, I give and bequeath unto my dear brother, the lord viſcount *Shannon*, the beſt watch I ſhall die poſſeſſed of, to put him in mind of my conſtant kindneſs and affection, which I endeavoured to expreſs by my voluntary yearly expence in keeping up the manor houſe of *Stalbridge*, without intending to live in it, for his ſake.

ITEM, I give and deviſe unto my worthy friend *Richard Newman*, of *Weſtminſter*, in the county of *Middleſex*, Eſq; a piece of plate of the value of 10 *l.* ſterling, as a teſtimony of my remembrance of his kindneſs, which I deſire him to continue by affording his aſſiſtance, if there be occaſion, to my executors hereafter named.

ITEM, I give and bequeath unto my faithful friend *John Nichalls*, Gent. ſteward of the courts of my manor of *Stalbridge*, a piece of plate of the value of 10 *l.* ſterling, to be within the ſpace of one year next after my deceaſe delivered unto him by my executors, as a teſtimony of my remembrance and affection; hereby making him the ſame earneſt requeſt, that I have hereby made to Mr. *Richard Newman*.

Item, I give and deviſe unto the right reverend father in God *Gilbert*, lord biſhop of *Saliſbury*, my great Hebrew bible with ſilver claſps, as a ſmall token of my great reſpect for him, and ſenſe of his favours to me.

Item, I give and deviſe unto *Thomas Smith*, now my ſervant, thirty pounds, in caſe he be with me, or in my ſervice, at the time of my death; but in caſe of his departure or removal from me, before that time, then my will is, that he ſhall have 20 *l.* and no more.

Item, whereas my ſervant *John Warr* the younger is indebted unto me in the ſum of 50 *l.* by bond or bill obligatory, my will is, that his ſaid debt be remitted and diſcharged.

Item, whereas I had ſet apart, among other things, the ſum of 400 *l.* for certain pious uſes; and whereas his late Majeſty King *Charles* the ſecond having, by his ſpecial grace and favour, without my ſeeking, or knowledge, been pleaſed to conſtitute me governor of the corporation for propagating of the Goſpel amongſt the heathen natives of *New-England* and other parts of *America*, hath thereby given me opportunity to diſcern that work to be unqueſtionably pious and charitable; and whereas I have given and paid the ſum of three hundred pounds towards that piety; I do hereby give and deviſe the ſum of one hundred pounds more to the ſaid corporation (though by reaſon of ſickneſs and infirmity I have reſigned the office of governor) to be ſet aſide and employed as a ſtock for the relief of poor Indian converts, which I hope will prove of good effect for the advancement of the pious work, for which they are conſtituted, and in which I heartily pray him, whoſe glory the work itſelf tends unto (and I hope the perſons intruſted with it aim at) to give them a proſperous ſucceſs.

Item, to the Royal and learned Society for the advancement of experimental knowledge, wont to meet at *Greſham* college, I give and bequeath all my raw and unprepared minerals, as ores, marchaſites, earths, ſtones (excepting jewels) *&c.* to be kept amongſt their collections of the like kind, as a teſtimony of my great reſpect for the illuſtrious Society and deſign, wiſhing them alſo a moſt happy ſucceſs in their laudable attempts to diſcover the true nature of the works of God, and praying, that they and all other ſearchers into phyſical truths may cordially refer their attainments to the glory of the Author of Nature, and the benefit of mankind.

Item, to my worthy friend and phyſician, Sir *Edmund King*, Knt. I give and bequeath a ſilver ſtandiſh of the value of 30 *l.* ſterl. as an unfeigned, though ſlight teſtimony of the juſt eſteem I have of his worth and ſkill, and the ſenſe I have of his particular care of me and kindneſs for me.

Item, I give to Mr. *Robert Hooke*, now profeſſor of mathematics in *Greſham* college, my beſt microſcope and my beſt loadſtone, which I ſhall have at the time of my death.

Item, I give and bequeath unto Mr. *John Dwight* and Mr. *John Whittane*, once my ſervants, each of them a ring of 5 *l.* price.

Item, I give and bequeath unto Mr. *Chriſtopher White*, once my ſervant, 5 *l.* ſterling.

Item, I give and bequeath unto Mr. *John Milne* and Mr. *Hugh Gregg*, once my ſervants, each of them a ring of 6 *l.* price.

Item, I give unto *Nicholas Watts*, my bailiff of the manor of *Stalbridge*, a piece of plate of the value of 10 *l.* or 10 *l.* ſterl. in money, in lieu thereof, to be at his own choice.

Item, I give unto my aforeſaid ſervant *John Warr*, 40 *l.* and the one half of my wearing apparel and linen, in caſe he be with me at the time of my death; but otherwiſe to have no ſhare of my apparel or linen.

And I give to *Robert St. Claier*, my ſervant, the ſum of 15 *l.* and the other half of my apparel and linen, in caſe he continues to be my ſervant at the time of my death: if he doth not, then I give to him a ring of 6 *l.* price only. And to Dr. *Frederick Slare*, late my ſervant, a ring of 8 *l.*

And I make and ordain my ſaid dear brother, *Richard* earl of *Burlington* and *Corke*, and my ſaid dear ſiſter, the lady *Ranelagh*, and *John Warr* the younger, gent. my preſent ſervant, ſole executors of this my laſt will and teſtament.

Item, I give and deviſe to my ſaid executors all my eſtate, right, title and intereſt of, in, and to a certain leaſe, bearing date in or about the month of   made by the corporation of mines royal, or their authority, unto *William* lord viſcount *Brounker*, Sir *Robert Moray*, Knt. late lord *Brereton*, and myſelf, of a mine or mines in or near the pariſh of *Aſhbury* in *Cheſhire*, for 41 years, or thereabouts.

Item, Whereas the executors of *James Watſon*, late alderman of the city of *Dublin*, and *Abraham Richens*, late of the ſame city, merchant, or ſome one of them, ſtand indebted to me in nine hundred and ſixty pounds, or thereabout, towards ſatisfaction whereof I have received the ſum of fourſcore pounds, or ſomewhat more, for a houſe in *Dame-ſtreet*, *Dublin*, ſold by me to my once worthy friend, Sir *John Temple*, Knt. deceaſed, formerly maſter of the rolls; which houſe was made over unto me, amongſt other things, towards ſatisfaction of the ſaid debt, upon a decree which I obtained in the then called the court for the adminiſtration of juſtice ſitting at *Dublin* aforeſaid. And whereas the ſaid debt of 960 *l.* or thereabouts, ſaving and except the 80 *l.* or ſomewhat more, ſo as aforeſaid beforementioned to be received, is ſtill due and owing; I give and deviſe the ſame, with all writings of what kind ſoever thereunto belonging, and the proceed of compoſition for the ſaid debt, whenſoever it ſhall happen, to be made unto my executors; and I do hereby will and declare, that all and every of my bequeſts aforeſaid unto the ſaid *Richard* earl of *Burlington*, the lady *Ranelagh*, and *John Warr*, my ſervant, jointly, as executors, and in that capacity, ſhall not be for their own private uſe, but only in truſt for and towards the payment of my debts and legacies hereby deviſed, and towards and for performance of this my laſt will and teſtament, and to and for no other uſe or uſes whatſoever.

And as touching the diſpoſition of all and ſingular my caſtles, manors, lordſhips, meſſuages, lands, tenements, and hereditaments in the kingdom of *Ireland*, which I have power to diſpoſe of, this is the laſt will and teſtament of me the ſaid *Robert Boyle* made and declared the day and year firſt above written, *viz.*

I give and deviſe unto the right honourable *Richard* earl of *Burlington* and *Corke*, my dear nephew the honourable *Henry Boyle*, Eſq; of *Caſtle-Martyr* in the county of *Corke*, and Sir *Robert Southwell* of *Kingſale* in the kingdom of *Ireland*, all my eſtate, right, title, intereſt, and demand whatſoever of, in, and unto the caſtle, town, manor, and lands, being nine plow-lands and half, or thereabouts, of *Buttevant*, and the moiety of the lands called *Buttevant* and *Rices* lands in the county of *Corke*, ſeveral years ſince mortgaged to my father and unto me for the ſum of 2000 *l.* ſterling, or thereabouts, and after my father's deceaſe leaſed by me under certain covenants and conditions for thirty-one years unto lieutenant colonel *Agmondeſham Muſchampe*, for about the ſum of ſix ſcore pounds *per Ann.* and now or lately in leaſe to *Denny Muſchampe*, Eſq; and alſo all my eſtate, right, title, intereſt, claim, and demand whatſoever, of, in, and to all and ſingular the towns, lands, tenements, and hereditaments of *Rathenege*, and four pounds chief rent iſſuing out of the lands of *Turmore* and *Ballytramly* by the name of Five Caſtles, all lying in the aforeſaid county of *Corke*, and all my eſtate, right, title, intereſt, and demand whatſoever of, in, and to the impropriate rectory of *Adare* alias *Athdare*, in the county of *Limerick* in *Ireland*; and alſo all my eſtate, right, title, intereſt, and demand whatſoever of, in, and to the caſtles, towns, and lands, containing two plow-lands and half (be it more or leſs) of *Ballydangan*, *Killcroyne*, and *Ballybrittas*, heretofore mortgaged to my ſaid father for the ſum of 1000*l.* or thereabouts, to have and to hold all and ſingular the ſaid caſtles, manors, lordſhips, meſſuages, lands, hereditaments, and premiſes of *Buttevant*, *Rices* lands, *Rathenege*, and the 4*l.* a year chief rent of impropriation of *Adare*, the two plowlands and half of *Ballydangan*, *Killcroyne*, and *Ballybrittas*, and all other the premiſes and ſum or ſums of money therefore or thereout due and payable unto me, with all my eſtate, right, title, intereſt, claim, and demand whatſoever in and to the premiſes, and every part, member, and appurtenances of them, every or any of them; and alſo all deeds, evidences, and writings whatſoever for or concerning the premiſes or any part thereof, to them the ſaid *Richard* earl of *Burlington*, the honourable *Henry Boyle*, Eſq; and Sir *Robert Southwell*, and their heirs and aſſigns for ever, as fully and in as large and ample a manner, as the premiſes or any part thereof are or inveſted in, deſcended, come, given, deviſed or bequeathed unto me the ſaid *Robert Boyle*, upon ſpecial truſt and confidence notwithſtanding, and to and for the only uſes, intents, and purpoſes in and by this will and teſtament limited and declared, and to and for no other uſe, intent or purpoſe whatſoever: that is to ſay, that they the ſaid *Richard* earl of *Burlington*, the honourable *Henry Boyle*, Eſq; and Sir *Robert Southwell* and their heirs, ſhall, after my death, by and with the advice of my executors, ſell the ſaid reſpective premiſes, and pay the money thereby raiſed, as alſo the profits until ſale, unto my ſaid executors, to and for the uſes herein expreſſed.

Item, I give unto my ſaid executors all and every ſuch ſum and ſums of money, as ſhall be raiſed out of or by ſale of my ſaid lands; and alſo all my printed books, goods, chattels, plate, monies, jewels, and credits whatſoever, and herein before bequeathed, limited, or appointed unto them, to and for the only uſes, intents, and purpoſes hereafter alſo in and by theſe preſents limited, expreſſed, and declared, and to and for no other uſe, intent, or purpoſe, whatſoever: that is to ſay, to the intent and purpoſe, that the ſaid *Richard* earl of *Burlington*, lady *Ranelagh*, and *John Warr*, their heirs, executors, adminiſtrators, or aſſigns, ſhall with what convenient ſpeed they can, after my deceaſe, by ſale or compoſition, (though upon terms advantageous to the buyers) ſell and make money of all the ſaid caſtles, lands, mortgages, goods, and chattels, cattle, plate, jewels, and credits by this my laſt will and teſtament bequeathed unto them, or to my ſaid truſtees, *Richard* earl of *Burlington*, the honourable *Henry Boyle*, Eſq; and Sir *Robert Southwell*; and ſhall, in ſome convenient time after my deceaſe, out of the proceed of the ſaid ſale or compoſitions duly and fully ſatisfy, pay, and diſcharge as well all the ſeveral debts and ſums of money (at preſent not amounting to much) which I now do, or at the time of my deceaſe ſhall owe to any perſon or perſons whatſoever, and the charges of the funeral of my body; and alſo all ſuch legal engagements, as I have undertaken to perform to any perſon or perſons whatſoever; eſpecially to my tenants of my manor of *Stalbridge*: and from and immediately after my debts and funeral charges, and all ſuch legacies, as I have above by this my will given and bequeathed, ſhall be diſcharged and paid, and my aforeſaid engagements made good; then my will is, that the reſt, reſidue, and remainder of the proceed of the ſale of, or compoſition for all the ſaid lands, mortgages, and chattels hereby deviſed and bequeathed unto the ſaid *Richard* earl of *Burlington*, lady *Ranelagh*, and *John Warr*, my ſaid executors, ſhall be by them ſummed up: and that in the firſt place they do diſtribute among the poor (having therein a ſpecial regard unto the poor of the pariſh of *Stalbridge*, and of the pariſh of *Fermoy*, in the county of *Corke*, in the kingdom of *Ireland*, and other pariſhes in that kingdom, where any of my lands do lie) the ſum of 300*l.* ſterl. and next after the diſtribution thereof the ſum of 200*l.* ſterl. more amongſt the much diſtreſſed perſons of all qualities that have been forced or frighted out of *Ireland*, and that ſhall happen to be in or near *London*, *Briſtol*, *Cheſter*, or *Liverpool*, at the time of my deceaſe; which ſums with reſpect chiefly to ſome things that occurred in a gracious deliverance, that was vouchſafed me in great diſtreſs, I think fit to charge my executors with the payment of after my deceaſe. And alſo, in the next place, that after the diſtribution of the ſums of 300*l.* and 200*l.* as aforeſaid, and alſo after the payment of the ſum of 66*l.* 4*s.* to the abovementioned Sir *Robert Southwell*, to be diſtributed by him amongſt the incumbents, reſidents, and widows and children of ſuch as have been incumbents and reſidents in the ſeveral pariſhes of which I have the impropriations in the kingdom of *Ireland*, and to ſuch of them, and in ſuch proportion, as the ſaid Sir *Robert Southwell* ſhall think moſt needing and deſerving of the ſame; that then they pay to my friends and kindred, named in the ſchedule to this my will annexed, the reſpective ſums therein expreſſed, in the whole amounting to the ſum of four hundred pounds; and after payment thereof I will and deviſe all the reſidue and remainder of my goods and chattels, real and perſonal, and of the money to be raiſed by the ſales aforeſaid by this my laſt will above directed to my ſaid dear ſiſter, the lady *Ranelagh*, with this deſire nevertheleſs, and my will is, that in caſe the ſaid reſiduary part of my ſaid eſtate ſhall amount (as I verily believe it will by far) to more than 800*l.* ſterl. over and above the legacies and bequeſts above deviſed and diſpoſed of, that then my ſaid dear ſiſter do retain and enjoy, for her own uſe, the ſum of 200*l.* ſterl. over and above the 300*l.* debt above remitted to her (my intention being, that ſhe ſhall be benefited in the total to the value of 500*l.* ſterl. by this my laſt will and teſtament) and this legacy being from the beforementioned reſiduary part of my eſtate, then my will is, that the overplus be laid out by my ſaid dear ſiſter, in caſe ſhe ſurvive me, and in caſe of her death, by my executors, in ſuch manner, as by my codicil or other writing under my hand I ſhall hereafter direct; and for want of ſuch direction, for charitable and other pious and good uſes at her or their direction. But I do chiefly recommend unto her and them the laying of the greateſt part of the ſame for the advance or propagation of the Chriſtian religion amongſt infidels; and I hope my relations and friends will believe my intentions to be as good towards them now, as when I thought I had eſtate more conſiderable to diſpoſe of than now I have (eſpecially having of late years loſt, by the breaking of goldſmiths and others, above 1500*l.* and ſince that, by the unfaithfulneſs of my receiver in *Ireland*, above 900*l.* and ſince that alſo by the deſtructive inſurrections and war that hath happened in *Ireland*, the whole income for above two years laſt paſt of my eſtate there) and will therefore accept in good part of what legacies ſhall be preſented unto them by my executors, conſidering that it is the duty of every honeſt man to prefer doing acts of juſtice before thoſe of kindneſs, when he is not ſure he is able to do both at once.

And whereas divers lands and tenements, by this my will above directed to be ſold, were originally mortgaged eſtates, to the end therefore that the heirs of the mortgagers may have no juſt cauſe to complain, nor the buyers be miſtaken in their purchaſes, I do hereby declare, direct, and appoint, that in caſe my couſin , the heir of Sir *Piercy Smith*, long ſince deceaſed, ſhall deſire, within the ſpace of one year after notice given him by my executors, to redeem the impropriate rectory of *Adare*, which was mortgaged by the ſaid Sir *Piercy Smith* for 1000*l.* and ſhall not do the ſame, that then, upon his releaſing of all his right and equity of redemption, the ſum of 66*l.* ſhall be paid to him out of the proceed and ſale of the ſaid rectory, as a gift or legacy from me to him. But my will is, that the ſaid ſum of 66*l.* ſhall not be paid unto him, unleſs he releaſe as aforeſaid within one year after the notice aforeſaid. And whereas the caſtle, town, and lands of *Ballydangan*, *Killcroyne*, and *Ballybrittas* aforementioned were heretofore transferred by me to my honoured couſin *Michael*, archbiſhop of *Armagh*, and primate of all *Ireland*, to have and to hold to the ſaid archbiſhop and his heirs, from and after the determination of divers years yet to come and expire: and whereas ſome years ſince, when the ſaid lands were ready to come into my hands, I made a promiſe to leaſe the ſaid premiſes to my abovenamed dear brother the earl of *Burlington*, for all my term and intereſt therein under the rent of 60*l. per Ann.* or thereabouts *de claro*, I do hereby ratify and confirm my ſaid promiſe, and do will and appoint, that he ſhall hold and enjoy the ſame under the rent of 60*l. per Ann.* during all the reſidue of my ſaid term and intereſt, which ſhall be to expire at the time of my death.

Lastly, I do hereby earneſtly deſire my ſaid executors to ſee this my will, and my truſts in them repoſed, faithfully performed, as they hope or deſire to have their own wills faithfully performed. But for as much as by reaſon of the uncertainties of the times it may happen (which God forbid) that my eſtate may meet with ſome further misfortunes, whereby it ſhall be ſo weakened as not to be ſufficient to diſcharge all the legacies by this my will bequeathed; then my will is, that after the payment of 300*l.* by me deviſed to the poor, and the ſum of 200*l.* by me deviſed to the much diſtreſſed perſons of all qualities, *&c.* as aforeſaid, mentioned to be in reſpect of a deliverance from a diſtreſs (which I hereby will ſhall be obſerved) that then my other legatees ſhall each of them be reſpectively abated their legacies, in proportion to what my ſaid eſtate ſhall fall ſhort of my ſaid other legacies, above by me deviſed.

In witneſs whereof, I the ſaid *Robert Boyle*, revoking and diſannulling all other and former wills, teſtaments, and bequeſts, have to every ſheet of this my laſt will and teſtament, contained in ſeven ſheets of paper, ſet my hand, and to the laſt of them my ſeal, the day and year firſt above written.

Robert Boyle.

Signed, ſealed, publiſhed, and declared by the abovementioned *Robert Boyle*, as his laſt will and teſtament, the day and year firſt abovewritten in the preſence of us,

Iſaac Garnier,
John Caddick,
James Oglebey,
William Johnſon.

A ſchedule in my will referred unto, for the diſpoſition of 400*l.* which I will ſhall be paid to the perſons herein after ſet down, in ſuch proportion to each perſon, as is herein expreſſed: *viz.*

IMPRIMIS, to my honoured and dear nephews, the earl of *Barrimore*, 30*l.* the earl of *Ranelagh*, 30*l.* *Charles* lord *Clifford*, 30*l.* Capt. *Robert Fitz-gerald*, 30*l.* Capt. *Henry Boyle*, 30*l.*

ITEM, to my honoured and dear nieces, the counteſs dowager of *Thanet*, 30*l.* the counteſs dowager of *Clancarty*, 30*l.* the lady viſcounteſs of *Powers-Court*, 30*l.* the lady *Frances Shaen*, 30*l.* the lady *Catherine Fitz-gerald*, 30*l.* and to Mrs. *Elizabeth Melſter*, not to make a difference between her and my other nieces in my affection, no more than there is in their relation to me, but becauſe of her peculiar circumſtances, I give 100*l.*

Robert Boyle.

THIS is the ſchedule referred to and mentioned in the fifth ſheet of this the laſt will and teſtament of the honourable Mr. *Robert Boyle*, as a part thereof, and was by his appointment annexed hereunto, and, at and before the enſealing and publiſhing this his will, ſigned by him in the preſence of us,

James Garnier,
John Caddick,
James Oglebey,
William Johnſon.

WHEREAS an act of parliament in *Ireland*, intitled, *An Act for the better Execution of his Majeſty King Charles the Second's gracious Declaration of the Settlement of his Kingdom of Ireland, and Satisfaction of the ſeveral Intereſts of Adventurers and Soldiers and others the Subjects there*, it was enacted (amongſt other things) that I *Robert Boyle*, my executors, adminiſtrators, and aſſigns, ſhould and might, for and during the term of thirty-one years, have, hold, and enjoy all and ſingular the impropriations of or belonging to the reſpective abbies late diſſolved, monaſteries, religious houſes, priories or pariſhes of *Ballytubber*, in the county of *Mayo*, *Knockmoy-Kireleulta Iran*, *alias* St. *Mary*'s *Athenree*, and *Dunmore* in the county of *Galway*, and *Tybone* in the county of *Tipperary*, or any of them, together with all the impropriate tythes and rectories and appurtenances of the ſaid impropriations, tythes, and rectories, or belonging thereunto, which belong unto, or by that act were veſted in his late Majeſty to any of the uſes therein before mentioned, according to the tenor and effect of ſuch grant or grants as had, or then were, or then after ſhould be paſſed unto me thereof by his late Majeſty's letters patents in that behalf, I or they paying yearly for the ſame double the exchequer or crown rent reſerved thereupon in the year 1641: now I the ſaid *Robert Boyle* do hereby give and deviſe to my dear brother *Richard* earl of *Burlington* and *Corke* in *Ireland*, the right honourable *John* lord viſcount *Maſſerine*, *Clotworthy Skeffington*, Eſq; in the county of *Antrim* in *Ireland*, and Sir *Peter Pett*, Knt. their executors, adminiſtrators, and aſſigns, the fourth part (the whole in four parts to be divided) of the ſaid impropriations and premiſes in and by the ſaid act granted, limited, or appointed by me to be held and enjoyed during the ſaid term of one and thirty years upon truſt, to be by them employed and diſpoſed for ſuch good and pious uſes, as I ſhall from time to time declare or appoint it, or in default of ſuch appointment;

appointment, after the death of me the ſaid *Robert Boyle*, then for ſuch pious and good uſes, as my ſaid truſtees in their beſt judgments and conſciences ſhall think to be moſt agreeable to my intent. And I deviſe the one moiety or half part of the other three parts of the premiſes unto the executors or aſſigns of my late dear ſiſter-in-law *Margaret* counteſs dowager of *Orrery*, for all the reſidue of the term that ſhall be to come at the time of my death. And as to the other moiety of the ſaid three parts, I deviſe the ſame to the right honourable *Elizabeth* counteſs dowager of *Angleſey*, her executors and aſſigns, for all the reſidue of the term that ſhall be to come at the time of my death; and in caſe of her dying before me, I deviſe the ſame to her executors, adminiſtrators, and aſſigns.

In witneſs whereof I have hereunto ſet my hand and ſeal the 25th day of July, in the year of our Lord God 1691.

Robert Boyle.

Signed, ſealed, and publiſhed by the abovenamed *Robert Boyle*, as part of his laſt will, in the preſence of us,

Iſaac Garnier,
Robert St. Claire,
William Johnſon,
James Oglebey.

Memorandum, That whereas I had thoughts of diſpoſing to good uſes of what money came to my hands on the ſcore of my ſhare of the grants mentioned in the codicil, whereunto this paper is annexed, to be granted to me by his late Majeſty King *Charles* the Second, in the act of ſettlement of his kingdom of *Ireland*: and whereas I myſelf living in *England*, found it difficult to meet with perſons every way qualified to be employed in the laying out of ſuch money to the uſes I deſigned, by which means there remains yet undiſpoſed of the ſum of 200*l.* which is now in the hands of the Eaſt-India company of merchants of the city of *London*, for ſecurity whereof I have their writing or bill under their common ſeal, dated the 20th day of May 1681; which ſaid writing or bill, together with the benefit, intereſt, and increaſe thereof, I have, by indenture of aſſignment aſſigned, transferred, and ſet over unto Sir *Henry Aſhurſt*, of *London*, Bart. by the name of *Henry Aſhurſt*, of *London*, Eſq; in truſt (as by the ſaid indenture, dated the 9th day of January 1684, relation being thereunto had, may appear) to and for ſuch uſes, intents, and purpoſes, as I ſhould at any time afterwards direct or appoint: and I having hitherto made no direction or appointment, do hereby, in caſe I diſpoſe not otherwiſe of it, or any part of it, during my life-time, deviſe, leave, and bequeath the ſaid ſum of money, with the benefit, intereſt, and increaſe thereof, to my truſtees *Richard* earl of *Burlington* and *Corke* in *Ireland*, the right honourable *John* lord viſcount *Maſſereen*, *Clotworthy Skeffington*, Eſq; and Sir *Peter Pett*, their executors, adminiſtrators, and aſſigns, to be by them received and diſpoſed of to the ſame uſes, for which I intruſt them with my ſhare of the impropriations of *Ballytubber*, *Knockmoy*, *&c.* mentioned in the foregoing codicil. And in caſe that within ſome reaſonable time after my deceaſe my title to the premiſes, or any of them, or to the money received by me on the ſcore of any of them, ſhall not be queſtioned, or like to be called in queſtion, to the prejudice of my executors, then my deſire is, that the truſtees may diſtribute the ſaid ſum, according as their conſciences and diſcretion ſhall direct them, among the preaching miniſters (whether parſons, rectors, vicars, or curates) that have beſtowed their pains as incumbents in the pariſhes or churches granted to me by the aforeſaid act, ſince they came into my quiet poſſeſſion, or to their poor widows or children, the ſaid truſtees being deſired to have regard in the diſtribution to the piety and orthodoxneſs of the parſons, to their pains and faithfulneſs in performing the work of their miniſtry, to the ſmallneſs of the worth of their livings, and to the conſiderableneſs of income they yield the impropriation. And by this direction I deſire and charge my ſaid truſtees to take their meaſures in diſpoſing of the future income or proceed of all that I have intruſted them with; only in caſe it ſhall appear to them for weighty reaſons, that ſomewhat may be ſpared for ſome other eminently good and pious uſe or uſes, they are left at liberty to uſe their diſcretion therein; provided the main part of the abovementioned money and future income be employed for the encouragement of pious preaching miniſters in the ſaid impropriations.

WHEREAS I have an intention to ſettle in my life-time the ſum of 50 *l. per ann.* for ever, or at leaſt for a conſiderable number of years, to be for an annual ſalary for ſome learned divine, or preaching miniſter, from time to time, to be elected and reſident within the city of *London*, or circuit of the bills of mortality, who ſhall be injoined to perform the offices following, *viz.* firſt to preach eight ſermons in the year for proving the Chriſtian religion againſt notorious Infidels, *viz.* Atheiſts, Theiſts, Pagans, Jews, and Mahometans, not deſcending lower to any controverſies, that are among Chriſtians themſelves; theſe lectures to be on the firſt Monday of the reſpective months of January, February, March, April, May, September, October, November, in ſuch church, as my truſtees herein named ſhall from time to time appoint.

SECONDLY, to be aſſiſting to all companies, and encouraging of them in any undertaking for propagating the Chriſtian religion to foreign parts.

THIRDLY, to be ready to ſatisfy ſuch real ſcruples, as any may have concerning theſe matters, and to anſwer ſuch objections or difficulties, as may be ſtarted, to which good anſwers have not yet been made.

AND whereas I have not yet met with a convenient purchaſe of lands of inheritance for accompliſhing ſuch my intention, I do therefore will and ordain (in caſe it ſhall pleaſe God to take me hence before ſuch ſettlement be made) that all my meſſuage or dwelling-houſe in St. *Michael's Crooked-Lane*, *London*, which I hold by leaſe for a certain number of years yet to come, ſhall ſtand and be charged during the remainder of ſuch term as ſhall be to come and expire at the time of my deceaſe, with the payment of the clear yearly rent and profits, that ſhall from time to time be made thereof (ground-rent, taxes, and neceſſary reparations being firſt to be deducted) to be paid to ſuch learned divine, or preaching miniſter, for the time being, by quarterly payments, that is to ſay, at Midſummer, Michaelmas, Chriſtmas, and Lady-Day, the firſt payment to begin at ſuch of the ſaid feaſts, as ſhall firſt happen next, after my deceaſe, and ſhall be made to ſuch learned divine, or preaching miniſter, as ſhall be in the employment at the time of my death, during his continuance therein. And I will, that after my death Sir *John Rotheram*, ſerjeant at law, Sir *Henry Aſhurſt*, of *London*, Knt. and Bart. *Thomas Teniſon*, doctor in divinity, and *John Evelyn*, ſenior, Eſq; and the ſurvivor or ſurvivors of them, and ſuch perſon or perſons as the ſurvivor of them appoint to ſucceed in the following truſt, ſhall have the election and nomination of ſuch lecturer, and alſo ſhall and may conſtitute and appoint him for any term not exceeding three years, and at the end of ſuch term ſhall make a new election and appointment of the ſame or any other learned miniſter of the Goſpel reſiding within the city of *London*, or extent of the bills of mortality, at their diſcretions.

tions. And I do hereby will and ordain this my codicil to be a part of my laſt will and teſtament. Witneſs my hand and ſeal, this 28th day of July in the year of our Lord 1691.

Robert Boyle.

Signed, ſealed, publiſhed, and declared by the honourable *Robert Boyle*, to be a codicil to and part of his laſt will and teſtament, in preſence of us,

Iſaac Garnier,
Robert St. Claire,
James Oglebey,
William Johnſon.

Whereas ſince the making and executing of my laſt will and teſtament, ſeveral perſons are brought to my remembrance, whom I ſhould have made legatees in my ſaid will, had I thought upon them before my finiſhing thereof; and being unwilling, that they ſhould be wholly deprived of my intended kindneſs to them; wherefore I have thought fit to inſert their ſeveral names and legacies in this my codicil, which I hereby declare to be a part of my ſaid will.

Imprimis, I give and bequeath unto my couſin Mrs. *Catharine Molſter*, daughter of my niece Mrs. *Elizabeth Molſter*, the ſum of 100 *l.* to be paid unto her, when ſhe ſhall have attained to her age of 21 years, or upon the day of her marriage, which ſhall firſt happen.

Item, I give and bequeath unto Sir *Robert Southwell* the ſum of twenty pounds.

Item, I give and bequeath unto Sir *Henry Aſhurſt*, Knt. and Bart. the ſum of twenty pounds.

Item, I give and bequeath unto Sir *William Aſhurſt*, Knt. and alderman of the city of *London*, the ſum of twenty pounds.

Item, I give and bequeath unto Sir *John Rotheram*, ſerjeant at law, the ſum of twenty pounds.

Item, I give and bequeath unto my ſervant *William Johnſon*, the ſum of fifteen pounds.

Item, I give and bequeath unto ſuch a number of the French proteſtant refugees inhabiting in and about the cities of *London* and *Weſtminſter*, as my executors ſhall think fit to pitch upon, and ſhall judge to be moſt neceſſitous, the ſum of 100 *l.* to be divided amongſt them, according to the diſcretion of my ſaid executors.

And whereas *Nicholas Courtney*, in the *Inner-Temple*, Eſq; is and hath been for ſeveral years indebted unto me in the ſum of 150 *l.* principal money, beſides intereſt for the ſame during the time he hath had the ſaid money; now in conſideration, that the ſaid *Nicholas Courtney* hath done ſome buſineſs for me in my life-time, I do hereby remit and fully diſcharge him, his executors, and adminiſtrators of and from all the intereſt money, which now is or ſhall be due unto me from him or them at the time of my deceaſe. And I do hereby declare, that this codicil ſhall be added to my laſt will and teſtament, and be a part thereof. In witneſs whereof I have hereunto ſet my hand and ſeal, the 30th day of July, in the third year of the reign of our ſovereign lord and lady, King *William* and Queen *Mary*, over *England*, *&c. Annoque Domini* 1691.

Robert Boyle.

SIGNED, ſealed, publiſhed, and declared by the honourable *Robert Boyle*, Eſq; as a codicil to be added to his laſt will and teſtament, and to be a part thereof, in the preſence of us,

Iſaac Garnier,
Robert St. Claire,
Jo. Caddick.

WHEREAS I have in and by my laſt will and teſtament impowered my moſt honoured and deareſt brother the right honourable *Richard* earl of *Burlington* and *Corke*, the honourable *Henry Boyle*, Eſq; and Sir *Robert Southwell*, my truſtees, to grant, bargain, ſell and diſpoſe of to the beſt advantage, immediately after my deceaſe, all that the manor and lands of *Buttevant* and *Rices* lands in the county of *Corke* within the kingdom of *Ireland*, with all and ſingular their rights, members, and appurtenances for and during ſo long time and term, as there ſhould be then to come and unexpired in the ſaid premiſes, in caſe the ſame ſhould not be redeemed; and have therein declared, that the money to be raiſed by the ſale or redemption thereof ſhould be by my ſaid Truſtees paid unto my executors: and having for a long time determined to make ſome real expreſſions of the great reſpects and kindneſs, which I have for and bear to my aboveſaid brother, and to the right honourable *Charles* lord *Clifford* (ſon and heir apparent of my ſaid brother) my honourable nephew, by making ſome gift or preſent to the off-ſpring of my ſaid brother: now in proſecution of my ſaid determination, I do hereby publiſh and declare, that my executors ſhall make an even and equal dividend of the ſaid purchaſe or redemption money, when paid to them by my ſaid truſtees as aforeſaid, and one moiety or half part thereof, my will is, that my executors ſhall pay back unto my ſaid brother the earl of *Burlington* and *Corke*, upon ſpecial truſt and confidence, nevertheleſs, and to the uſe, intent, and purpoſe herein after mentioned and declared; that is to ſay, that he the ſaid earl ſhall apply, lay out, pay, and employ the ſame moiety or half part of the ſaid money within three months after his receipt thereof for the uſe and beſt advantage of ſuch one or more of the younger children now unmarried of the ſaid lord *Clifford*, and in ſuch proportion, if to more than one, as to him the ſaid earl, his executors, adminiſtrators, or aſſigns, ſhall ſeem fit; and under this further truſt, that in caſe he the ſaid earl, his executors, adminiſtrators, or aſſigns, ſhall fail to ſettle and diſpoſe of the ſame, according to the ſaid truſt, within three months after the ſame is paid to him by my executors as aforeſaid, that then he the ſaid earl, his executors, adminiſtrators, and aſſigns, ſhall ſtand poſſeſſed of the ſaid moiety or half part of the purchaſe or redemption money, ſo paid to him as aforeſaid, in truſt for all the younger children now unmarried as aforeſaid, of him the ſaid lord *Clifford*, ſhare and ſhare alike. And as for the other moiety or half part of the ſaid purchaſe or redemption money, which will remain in the hands of my executors, I do hereby publiſh and declare ſhall be employed by them to the uſe or uſes in my ſaid laſt will declared and appointed. And this codicil I do hereby declare ſhall be added to my laſt will and teſtament, and be a part thereof. In witneſs whereof I have hereunto ſet my hand and ſeal the firſt day of Auguſt in the third year of the reign of our ſovereign lord and lady King *William* and Queen *Mary* over *England*, *&c. Annoque Domini* 1691.

Robert Boyle.

Signed, ſealed, publiſhed and declared by the honourable *Robert Boyle*, Eſq; as a codicil to be added to, and be a part of his laſt will and teſtament in the preſence of us,

Iſaac Garnier,
Robert St. Claire,
William Johnſon,
James Oglebey.

Whereas I have in and by my laſt will and teſtament ſettled and appointed the ſum of 200 *l.* ſterling to be paid and diſtributed by my executors to and amongſt ſuch diſtreſſed Iriſh proteſtants of all qualities, as have been either forced or affrighted out of the kingdom of *Ireland* ſince the late calamities there, and that ſhould happen to be in or near *London*, *Briſtol*, *Cheſter*, or *Liverpool*, at the time of my deceaſe: now in regard of the preſent exigencies and preſſing neceſſities of ſeveral of thoſe perſons, I have thought fit to allot and give out myſelf as a part of that my intended the ſum of 50 *l.* to be divided and diſtributed amongſt ſuch of them, as I do judge moſt neceſſitous for their preſent relief; ſo that now my will is, that my executors ſhall be obliged to pay and diſtribute to and amongſt the ſaid diſtreſſed Iriſh proteſtants in my laſt will mentioned as aforeſaid, no more than the ſum of 150 *l.* (being the reſidue thereof) for the reaſon aforeſaid. And this my codicil I do hereby ſignify and declare ſhall in order thereto be added to and be a part of my laſt will and teſtament. In witneſs whereof I have hereunto ſet my hand and ſeal this fifth day of Auguſt in the year of our Lord God 1691.

Robert Boyle.

Signed, ſealed, publiſhed, and declared by the honourable *Robert Boyle*, Eſq; for and as a codicil to be added to and be a part of his laſt will and teſtament, in the preſence of us,

Robert St. Claire,
Thomas Smith,
William Johnſon,
James Oglebey.

Whereas I am juſtly ſenſible of the great trouble, that I ſhall give my deareſt and moſt honoured brother and ſiſter, the earl of *Burlington* and *Corke*, and the lady viſcounteſs *Ranelagh*, by my having conſtituted and ordained them to be two of the executors in truſt of my laſt will and teſtament; which, conſidering their age and quality, they may be no way fit to undergo; to the intent therefore, that they may be eaſed, as much as in me lies, from all the avoidable trouble of that office, I do hereby declare, that I have engaged my other executor in truſt, Mr. *John Warr*, jun. my preſent ſervant, to take upon him more immediately the troubleſome part of the execution of my ſaid laſt will and teſtament, which he is to proſecute by and according to the direction and appointment of my other two executors, and not abſent himſelf from the ſame during the term or terms herein after mentioned, for which he ſhall happen to be employed in the execution of the ſaid truſt, by travelling above one day's journey from *London*, unleſs it ſhall be on extraordinary occaſion, and with the conſent of my other two executors. In conſideration whereof I do hereby give, deviſe, bequeath, and ſettle upon him, the ſaid *John Warr*, the yearly ſum of 40 *l.* for his care and pains in the execution of the ſaid truſt; which, my will is, ſhall be continued to him as a yearly ſalary for the ſpace of three years from the time of the proving my ſaid will, provided the diſcharge of my ſaid truſt require ſo long time;

otherwiſe my will is, that this ſalary ſhall be continued but for the ſpace of one year, or at moſt but in proportion for ſuch further time, as ſhall be neceſſary for the diſcharge of the then remaining part of the ſaid truſt. And my further will is, that beſides the ſalary abovementioned, my ſaid executor *John Warr* ſhall be allowed all ſuch neceſſary and reaſonable charges, as he ſhall from time to time expend or be put unto in the performance and execution of the ſaid truſt. Laſtly, my will is, that this codicil ſhall be added to and be a part of my laſt will and teſtament. In witneſs whereof I have hereunto ſet my hand and ſeal the eleventh of September in the third year of the reign of our ſovereign Lord and Lady *William* and *Mary* by the grace of God of *England*, *Scotland*, *France*, and *Ireland*, King and Queen, defenders of the faith, *&c.* and in the year of our Lord God 1691.

Robert Boyle.

SEALED, publiſhed, and declared by the honourable *Robert Boyle*, Eſq; for and as a part of his laſt will and teſtament, in the preſence of us,

Robert St. Claire,
Thomas Smith,
Jo. Caddick,
William Johnſon.

WHEREAS in and by my laſt will and teſtament bearing date on or about the 18th day of July laſt paſt I did conſtitute and appoint my deareſt ſiſter *Ranelagh* one of my executors in truſt: and whereas it hath pleaſed God ſome few days ſince to take unto himſelf my dear ſiſter; I do therefore in her ſtead and place hereby make, ordain, and appoint my very loving friend Sir *Henry Aſhurſt* of the pariſh of St. *Sepulchre*'s in the county of *Middleſex*, Knt. and Bart. to be one of the executors in truſt of my ſaid laſt will and teſtament. And I do hereby deſire and impower my ſaid executor, together with my other two ſurviving executors in truſt in my ſaid laſt will named and appointed, to take ſpecial care to diſcharge the truſts by me therein in them repoſed, and more eſpecially that part of my will, that I had particularly recommended to the care of my deareſt ſiſter *Ranelagh* deceaſed. And I do hereby give and bequeath unto the ſaid Sir *Henry Aſhurſt* the ſum of 20 *l.* to be laid out by him in a piece of plate. And I do hereby confirm all the appointments of my ſaid laſt will and teſtament, excepting only what therein relates to my ſaid deceaſed ſiſter *Ranelagh*, which I do hereby revoke. And I do hereby alſo confirm all other codicil or codicils, that have been made by me ſince the date of my aboveſaid laſt will and teſtament. In witneſs whereof I have hereunto ſet my hand and ſeal the 29th day of December, in the third year of the reign of our ſovereign Lord and Lady King *William* and Queen *Mary* over *England*, *&c. Annoque Domini* 1691.

Robert Boyle.

SIGNED, ſealed, publiſhed, and declared by the honourable *Robert Boyle*, Eſq; as a codicil to and a part of his laſt will and teſtament, in the preſence of us,

Iſaac Garnier,
Robert St. Claire,
Tho. Smith,
William Johnſon,
James Oglebey.

NUMBER III.

Letters relating to the edition of the Iriſh Bible printed and diſtributed at the expence of Mr. *Boyle*. See *Life*, p. cxxxix. and Works, Vol. VI. p. 592——610.

I.

Biſhop of Meath *to Mr.* Boyle.

Honourable Sir, *Dublin*, Aug. 4, 1680.

THE incloſed, from my good friend Dr. *Sall*, ſhould have been (as you find) ſooner with you, but that I made bold to reſerve the paſſing it to you by my good lord the earl of *Ranelagh*, as now; deſigning in both, to be repreſented, and ſo better known to you, than by a bare letter from a ſtranger, I mean myſelf.

By Dr. *Sall* I underſtand, Sir, your zeal for having this poor ignorant people, the *Iriſh*, informed in the true knowledge of God in their own language, by reprinting the New Teſtament, and Common Prayer in Iriſh, theſe being now out of preſs, and the books of them extant hardly found.

In addition whereunto is herein offered, the publiſhing of the Old Teſtament alſo in Iriſh, together with the Pſalms in metre fitted to our ordinary church tunes, tranſlated by means and procurement of that learned and pious prelate, Dr. *Bedell*, ſometime biſhop of *Kilmore* in *Ireland*, and provoſt of *Trinity* college in *Dublin*; whoſe heart God had ſtirred up thereunto zealouſly. This was by that learned biſhop himſelf compared with the original, and finiſhed anno 1640. After whoſe death it came into mine hand, where it now continues in ſheets, and MSS. This compleating the bible in Iriſh, and added to what is already printed, would be a work greatly to God's glory, in bringing, by his grace, many from darkneſs to light, and of deſerved praiſe to the happy undertaker.

I had once thoughts of repreſenting this to our next parliament here, hoping for public allowance, and ſupplies thereby, toward it. But in diſcourſe with ſome concerning it, I found it almoſt a principle in their politics, to ſuppreſs that language utterly, rather than in ſo public a way to countenance it. This occaſioned what I have ſometime written of that, in an epiſtle to the earl of *Eſſex*, then lord lieutenant of *Ireland*, prefixed to a ſmall tract, which is therefore herewithal for your peruſal; accompanied with another on another occaſion, both under, Sir, your cenſure.

As for the Iriſh letters ſtamped for the firſt printing here of the Common Prayer, and New Teſtament; they had paſſed from hand to hand of many his Majeſty's printers in *Dublin* ſucceſſively, until by covetouſneſs of one, into whoſe hands they fell unhappily, they were by the Jeſuits gotten away, and are now at *Doway*, for Iriſh prints; ſome of which I have ſeen, to my grief, ſent hither, further corrupting the people. So as there is nothing left of what was formerly, towards the printing of Iriſh here, if it ſhould be required.

I have dealt with our preſent provoſt of this college of *Dublin*, that he, according to what was ſometimes by Dr. *Bedell*, his predeceſſor, practiſed, would encourage the reading Iriſh; and that Iriſh prayers, *&c.* as others, might be publicly uſed in the college, for thereby fitting out labourers for that harveſt of ſouls, which may, by God's bleſſing ſuch endeavours, be hopefully expected. But even for that, are books wanting, or not ſufficient to be found for it. Therefore the reprinting what is now out of preſs, if there be no more, will be for that good work neceſſary.

I REFER all, Sir, to your conſideration, whoſe heart God hath, I find, enlarged in this, as otherwiſe eminently, to your laſting remembrance, and praiſe every way. And deſiring God's bleſſing you in all your ways, I reſt,

Honourable Sir,

Your moſt humble ſervant,

Henry Midenſis.

II.

Mr. Boyle *to the biſhop of* Meath.

My Lord,

I HOPE your lordſhip did long ſince receive the letter I had the honour to write to you, when I ſent you the forms of ſubſcription, ſucceſsfully employed here, in carrying on the publication of the Welch bible. And though I have not, ſince I ſent that packet, which was above two months ago, received any intimation, that it came ſafe to your hands, yet I thought it unfit to trouble your lordſhip with another letter ſince that time, becauſe I could not give you in it any good account of the progreſs of our Iriſh teſtament; the preſs, that was to print it, having, as well as others here, been ſo taken up with pamphlets of ſeveral ſizes and kinds, that till of late our work was almoſt quite laid aſide; but now, for want of other employment, I hope the printer will reſume it, and vigorouſly purſue it, partly becauſe he has promiſed to do ſo, and partly becauſe he is ſo far advanced, that he ſays, there is but five or ſix ſheets remaining to be printed off. But I fear the preface will loſe as much time, becauſe the moſt intelligent divine I have adviſed with here about it is earneſt to have the preface, that the Janſeniſts have premiſed to their tranſlation of the new teſtament; which being a piece of great learning and piety, and much eſteemed by the better ſort of the Romaniſts themſelves, it is judged, that if it be publiſhed in Engliſh with no more alterations and additions, than the exigency of our deſign requires, it may very much recommend the introduction of the Iriſh teſtament to the better ſort of Papiſts themſelves, for whoſe benefit it was chiefly made. Mr. *Reily*, that corrects the preſs for the Iriſh teſtament, having been bred in *France*, undertakes the turning this preface into Engliſh; which, as ſoon as he has done, for it is no ſhort one, I ſhall, God permitting, ſend it into *Ireland* to be peruſed by your lordſhip and my worthy friend Dr. *Sall*, and to be made ſuch uſe of, as you ſhall upon the place think fit. I formerly gave notice, that I had ordered my agent in *Munſter* to return up to *Dublin* 50 *l.* in lieu of a ſubſcription towards the printing of the Old Teſtament in Iriſh; but Sir *John Champanty* having more than once taken notice to me, that the money lies in his hands, I now think it neceſſary to advertiſe your lordſhip of it, and that a direction to pay it in, when your lordſhip ſhall call for it, for the uſe newly mentioned, is intended to be, by the next poſt, renewed to him by,

My Lord,

Your Lordſhip's moſt humble

[April 8, 1681.]

and moſt obedient ſervant.

III.

Mr. Boyle *to Sir* John Champanty.

SIR, *London*, April the 9th, 1681.

FINDING by my ſiſter *Ranelagh*, that you have been pleaſed to receive fifty pounds for my uſe, and are deſirous I ſhould diſpoſe of it as ſoon as there is occaſion; I think myſelf obliged to return you my humble thanks for the favour of letting it lie in ſuch ſafe hands as yours, and to inform you, that I intend to employ it by way of contribution to the printing of the Old Teſtament in the Iriſh tongue, which is carrying on by the right reverend the lord biſhop of *Meath*, and ſome other worthy perſons in your parts. And therefore if his lordſhip, or any duly authoriſed by him, ſhall call to you for the money to be employed to that purpoſe, I deſire you would be pleaſed upon his acquittance to let him forthwith have it; whereby you will further oblige,

SIR,

Your moſt humble ſervant,

Robert Boyle.

The ſuperſcription,
These to my honoured Friend Sir John Champanty,
preſent at Dublin.

IV.

Biſhop of Meath *to Mr.* Boyle.

Honourable Sir, *Dublin*, May 3, 1681.

I HAVE now two of yours (of the 8th and 19th paſt) and thereby underſtand my loſs of a packet not received, which I greatly deſire may be retrieved. (There is nothing from you not deſirable.)

As to that preface of the Janſeniſts, premiſed to their tranſlation of the New Teſtament, propoſed to be prefixed to our Iriſh New Teſtament; conſidering their refined principles, and even that very deſign of their publiſhing the holy ſcripture in a vulgar tongue; and that that preface paſſeth among the better ſort of Romaniſts themſelves; ſo rendering all, probably, more acceptable to that ſide; it having alſo that approbation you mention of thoſe eminent among us; on all, although I have not yet ſeen it, yet can I not but well approve of your thoughts for tranſlating, printing, and affixing that to our Iriſh new teſtament alſo. Dr. *Sall* is now here with me, to whom I imparted your mind in this, in which is his concurrence alſo.

I HAD, from Dr. *Sall*, ſome time ſince, my firſt intimation of your ſetting yourſelf on that good work of reprinting our Iriſh New Teſtament, which I then underſtood to have been intended of both, the Common Prayer Book and New Teſtament alſo; both being neceſſary, and out of preſs, and hardly found. But now I find it from yourſelf (in yours of the 19th) that you had not then ſo much as knowledge of ſuch a thing in being, as the Common Prayer in Iriſh, and publiſhed.

So as our work herein is thereby enlarged; both for printing the Old Teſtament in Iriſh, which hath not been done hitherto; and for reprinting the Common Prayer alſo; that ſo all (with what you are now doing) may be complete.

YOUR letters for the work in hand will ſo far eaſe that charge; and what you propoſe of ſubſcription for printing the Old Teſtament will take in all.

THAT, which leads to this, would be the digeſting that propoſal for ſuch a ſubſcription: that it be not overcharging as to particular ſums; and that it be ſufficient in the total to anſwer the whole work. It may be, what hath been already ordered that way, as to the Welſh Bible (which I have not ſeen) may be for this alſo. In ordering which, as to *Ireland*, for procuring ſubſcriptions, from our clergy in our ſeveral dioceſes, and from others here, and in the ſeveral provinces, as it ſhall require;) this ſhall be (if you pleaſe) my charge herein; if ſome other be at leiſure for furthering ſomething that way there alſo. The ſooner that the ſcheme for this is conſidered and prepared, the better; that by that time that the New Teſtament and preface to it (which will take up ſome time) may be perfected, your letters for that work may be free for this, to be done here; or this here to be ſent thither, for being ordered by the ſame preſs and overſeer, as is the New Teſtament at preſent: in which Mr. *Reily*, your overſeer, well deſerves.

As for your 50 *l.* left in Sir *John Champanty*'s hand, in way of ſubſcription toward the printing the Old Teſtament, I deſire, that this may be yet under your own ordering, until that work ſhall be under a fuller proſpect, one way or other. In the mean time, and ever, you have your praiſe of piety, bounty, and beneficence, among all good men, in this, and other your good every way manifeſted, and not to be forgotten.

I HAVE communicated to the worthy provoſt of this our college, what you were pleaſed to declare of your intentions for encouraging our Iriſh reader and inſtructor here: which is thankfully accepted, and in your ſo taking notice of that work, will it be much advanced. Be pleaſed, that what in that kind is deſigned may be rather in money than otherwiſe; of which the quantum will be ſufficient, as it is propoſed: that alſo this be conſigned to the provoſt to be ordered accordingly.

As to the teaching and learning Iriſh; that perſon hath read (I hear) a public lecture in the college hall: to which, of the ſtudents, about 80 already reſort: divers alſo of the fellows, and chief of that ſociety, frequent his chamber privately for inſtruction in that kind. And even the provoſt himſelf (not very many years ſince tranſplanted out of *Oxford* hither, and not many months ſince his promoting this good work within this his charge) is become ſo great a proficient already in the knowledge of the Iriſh, that to the teacher's admiration (from whom I have it) he writes the Iriſh perfectly; and is with his own hand tranſcribing the Iriſh grammar, intended for your peruſal and farther conſideration. Of our having ſuch a grammar in our hand, ſome intimation hath been formerly given you. There is, I hear, an Iriſh dictionary in *Bodley*'s library, which the provoſt hath ſent for, for preſent uſe, and to be reſtored.

I SHALL ſhut this up, with what I have in my laſt given you joyfully, and with what is ſince then, of the progreſs of Iriſh preaching in this college chapel. The firſt Sunday in each month is deſigned for that. The firſt of that (as you heard) was on Eaſter-day laſt (a day, you well obſerve, for that, and I hope, auſpicious). The next was on that day month, May-day laſt (the ſecond of that kind) when the auditory was ſo enlarged, that the whole area of the chapel, with rooms adjoining above and below, were unuſually thronged. Among thoſe were the lord viſcount *Dillon*, and other eminent perſons, and others of the Romiſh profeſſion: which we take for a preſage of yet greater good to the church and kingdom at large, from this happy ſeminary. The lord lieutenant intends to afford his preſence at this exerciſe ſometime, for farther countenance, and encouragement to it. Dr. *Sall* deſigns to preach there next in Iriſh. Let God's bleſſing be on this, and on all, whoſe hearts are ſtirred up by his ſpirit, in this great work; in which you have, I doubt not, a double portion, who firſt promoted it in what you are there doing happily. To his grace and goodneſs I leave you; and reſt,

Honourable Sir,

Your moſt humble and real ſervant,

Henry Midenſis.

Having ſome curioſity for obſerving the late comet, I gave what was on that collected to the provoſt here; from whom I ſend in return the incloſed paper, with which I preſent you, as well knowing what uſe to make of it. He was well fitted for ſuch obſervation by inſtruments which he had, and none other that I know hereabout.

V.

Biſhop of Meath *to Dr.* Narciſſus Marſh, *provoſt of* Trinity *college in* Dublin.

Reverend Sir, *Oſterſton*, Aug. 1, 1681.

I RECEIVED yours of the 25th paſt, for which I thank you. My leaving *Dublin* was (I found) about the time of your greateſt trouble, the commencement; and my retiring then into the country, where now I am, was by reaſon of ſome indiſpoſition attending me in that city: both which occaſioned my not waiting on you on my going thence, whereby we might have diſcourſed more freely on the matters in Mr. *Boyle*'s letter, which I ſent you for your peruſal; that occaſioning yours to me mentioned. Nor could I well return him an anſwer to his, until I had firſt adviſed with you concerning it.

In former letters from that worthy perſon, Mr. *Boyle* (like whom, in his way, the age affords not any) I found him zealouſly intent on prefixing the Janſeniſts late prefaces of the New Teſtament in French, to the Iriſh New Teſtament now under his hand in the preſs; hoping thereby (I conceive) to gain more in the world, in joining ſo far with thoſe of that perſuaſion; they drawing toward us, although not cloſing with us. Hereunto, on that conſideration, I then alſo inclined, although I had not yet ſeen what thoſe prefaces imported.

Since then, I had, by the hand of Dr. *Sall*, the book of the New Teſtament in French, with thoſe Janſeniſts prefaces prefixed. Which their prefaces, for my better information, I had tranſlated, being myſelf a ſtranger to the French.

In reading which I obſerved harſh reflections on ſome of the more eminent and leading proteſtant reformers; alſo other paſſages concerning the difficulty of tranſlations out of the original, on the reaſons therein mentioned: this taking up much paper, and more time and labour than could be now well ſpared in tranſlating that again out of French into Iriſh.

On which I preſume to offer mine own thoughts, that I know not how convenient would be our tacit owning ſuch reflections on ours as juſt; and whether that might not ſtick with ſome, and not pleaſe others? Or profeſſing to give the world a true tranſcript of thoſe the Janſeniſts prefaces, whether it would be ingenuous by picking and chuſing, to omit any part thereof, on a private account; I mean, the omitting (if that ſhould be) thoſe reflecting paſſages, which might offend? And whether ſuch omiſſions might not diſguſt the Janſeniſts themſelves, whom we would indulge? And whether, either in omitting or not omitting thoſe reflecting paſſages on ours, we ſhould not equally gratify the common adverſary?

In this, I would willingly adviſe with you, Sir, but know not how you can give any judgment herein at preſent, without the firſt peruſal of thoſe prefaces, which you cannot now compaſs, the book being locked up in my ſtudy in *Dublin*, and not to be there readily found but by myſelf: which yet ſhall not hinder, if you pleaſe to command my going hence to *Dublin*; which on that occaſion I ſhall do, on intimation by you therein given.

As for myſelf: conſidering what hath been before offered; as alſo the delays to the principal part of the work, the publiſhing of the New Teſtament in Iriſh; of which there is ſuch uſe and need at this time; on theſe conſiderations, I ſay, I confeſſed myſelf much biaſſed to the laying aſide the tranſlating thoſe French prefaces in Iriſh; at leaſt for the preſent. And if on farther conſideration it ſhould be after found notwithſtanding adviſable to take notice of them, that they be prefixed to the Old Teſtament, or to the Common-Prayer-Book in Iriſh, when publiſhed.

As to the printing of the Old Teſtament in Iriſh; that being a work of charge and time, toward which, not ſo much as a propoſal is yet prepared, but in its time expected; I ſhould offer, that this alſo be reſpited for a while; and that the Common-Prayer-Book in Iriſh, formerly printed, and now out of preſs, ſhould be firſt reprinted: which, with the New Teſtament, ſhortly expected, may well forward that your pious work, in which you have ſo merited of this church and nation.

In all which, I humbly ſubmit to better judgments and to yours in a ſpecial manner; deſiring to hear from you, if you pleaſe; that in my return to Mr. *Boyle*, the whole may be repreſented to him, and alſo left to his ordering.

Thus, deſiring God to bleſs you in that your great charge and work, I reſt,

Reverend Sir,

Your very affectionate

and humble ſervant,

Henry Midenſis.

VI.

Biſhop of Meath *to Mr.* Boyle.

Honourable Sir, *Dublin*, Sept. 3, 1681.

THIS ſeems a tardy return to yours, two months paſt; but the incloſed ſhews me not ſo forgetful:

IN which you find my communicating yours to my good friend Dr. *March*, the worthy provoſt of the college here; whoſe hand in furthering that by you begun, for the good of the Iriſh, in their own language, hath been happy, and every day more ſucceſsful.

HIS thoughts, with mine, you have, in the incloſed, concerning the Janſeniſts prefaces to the New Teſtament in French, deſigned for ours alſo in Iriſh; to which I refer, and ſubmit all, howſoever, to what you ſhall judge of it. Dr. *Sall* ſpeaks of preparing a preface, he hopes, fit for our purpoſe; which, when peruſed and allowed by you, ſhall be after tranſlated into Iriſh.

Some of your copies of the New Teſtament reprinted would be very acceptable here, where they are wanting, and neceſſary. Theſe, if you pleaſe, may be conſigned to the provoſt for public uſe.

I AM on the way for procuring one of the Iriſh Common-Prayer-Books; and ſhall find the means for conveying it to you. I ſuppoſe your preſs will be fitter for reprinting that, than any here, ſtrangers to that character and language. But whether there or here, and how to be carried on, I may after underſtand.

I AM ſurpriſed, in miſſing in that our Common-Prayer-Book in Iriſh the vulgar tranſlation of the pſalms; which until now I never obſerved; it having been a ſtrange omiſſion, that the pſalms, of ſuch daily uſe in reading, ſhould have been neglected. But, I ſuppoſe, that what of the pſalms we have in the body of the Old Teſtament in the Iriſh MS. (which I have) may ſupply that want, although it be not according to the vulgar, and may be added in the Common-Prayer-Book as the other.

I PERCEIVE by your laſt, that what of yours miſcarried in ſending, was the project concerning your Welſh Bible, intended as leading to ours of the Old Teſtament in Iriſh, when it ſhould be conſidered. But that ſo loſt may be (I doubt not) retrieved ſeaſonably, before we can be ready for the other. For I underſtand by Dr. *Sall*, and others verſed in the Iriſh, that it may be neceſſary, that the Iriſh verſion of the Old Teſtament in MS. ſhould be firſt reviſed, before it be expoſed to public view. This will take up time; and will require charge alſo, in procuring aſſiſtance, and labours in that kind of ſuch as are of approved abilities for it: among whom I wiſh, if it might be, that Mr. *Reily* might be one. Of this you ſhall hear farther hereafter.

WHAT you had in my former concerning the late comet, was of Dr. *Marſh*, our provoſt's obſervations, which being by him ſent me, I preſented them to yourſelf, who are generally known to be in the recondits of nature, above others, critical.

You find alſo ſomething of his in Dr. *Plott*'s Natural Hiſtory of *Oxfordſhire*, page 289——concerning muſic; to which I refer. This I mention, that the genius of that worthy perſon may be better known, his natural modeſty giving him otherwiſe to lie hidden.

I SHALL no farther interrupt your more ſerious thoughts at preſent, but wiſhing you all happineſs, and the reward of your good to all, reſt,

Honourable Sir,

Your affectionate and humble ſervant,

Henry Midenſis.

VII.

Mr. Boyle *to Dr.* Henry Jones, *Biſhop of* Meath.

My Lord,

I MUST begin this letter with my humble thanks for the honour of your lordſhip's of the 3d of this month, which, together with the two incloſed papers, came the ſlowlier to my hands, becauſe they found me not in *London.* As to the verſion of the Janſeniſts preface, I muſt ingenuouſly confeſs to your lordſhip, that the book itſelf, to which it is prefixed, having been got from me, by a perſon of quality, before I had read more than here and there ſome paſſages of the preface, I did, in recommending it, rely leſs upon my own judgment, than that of a very learned and famous divine, and ſome other perſons of eminent parts. And though I was aware of ſome of the objections, that I find in the two judicious papers, and had with Mr. *Reily* taken order to have them obviated in his tranſlation into Iriſh, yet I meet with ſome weighty ones that I did not (and indeed could not) well know of; and therefore, as your lordſhip may remember, that in propoſing this preface, I was ready to ſubmit to the better judgment of your lordſhip and your learned friends upon the place, ſo now, upon freſh accounts, I fully refer to you the prefixing what preface ſhall be thought moſt ſuitable to the deſign of the publication, and the particular circumſtances of it and the verſion: in which work I am very glad your lordſhip will have ſo good an aſſiſtant as Dr. *Sall*, who is ſo able a man, and ſo well acquainted with the humour and opinions and prejudices of his countrymen, that I doubt not but he will ſkilfully accommodate his diſcourſe to them. Only, perhaps, it may be thought fit to make uſe of ſome paſſages of the Janſeniſts preface, not as it is a preface, but as it contains the public avowed ſenſe, and eloquently exhibits the reaſons of famous and eminent divines of the Roman church for the tranſlating and ſtudying the New Teſtament in vulgar tongues. I am, as well as your lordſhip, much ſurpriſed, that the reading pſalms ſhould be omitted in the Iriſh Common-Prayer-Book; but perhaps it will not be for the worſe, ſince it will be neceſſary to ſupply that omiſſion out of your Iriſh Old Teſtament, in which it is hard if the pſalms be not better tranſlated out of the original, than if they had been out of the vulgate. I perceive, that, God permitting, there will be time to retrieve the papers that I ſent your lordſhip, and that miſcarried, about the method uſed in getting ſubſcriptions for the Welſh Bible; but by reaſon of the abſence of the manager of that impreſſion, I cannot yet get copies of his papers. But I hope your lordſhip, and the other well-wiſhers to the printing of the Old Teſtament, will not ſtay long for the reviſal of the tranſlation. For though it ſeems very fit that one ſhould be heedfully made, yet I ſee no neceſſity that the work ſhould be thereby retarded. For ſome good progreſs may be made in the review, before it will be poſſible to get the firſt ſheet printed; and then it cannot be very difficult for the reviſers to correct at leaſt as faſt as I fear the printer will work off. Whether the edition will be fitter to be made here (as your lordſhip ſeems inclined to think) or at *Dublin*, I wholly leave to be determined by your lordſhip and your friend: and whether here or there, the letters I cauſed to be caſt will be equally at your lordſhip's ſervice. I lately expoſtulated with Mr. *Reily* about the ſhameful tardineſs of the edition of the New Teſtament; for which he much excuſes himſelf, laying the whole blame upon the printer, and promiſing, that if the Old Teſtament be to be wrought off here, he will tie him up very ſtrictly to diſpatch. All which, notwithſtanding I know not, as I was lately intimating, whether *London* or *Dublin* be the fitteſt to be pitched upon for this work; eſpecially ſince here, in caſe Mr. *Reily* ſhould die, or fall ſick, the whole work muſt be at a ſtand, there being none (that I can hear of) in this place fit to correct the preſs. Thoſe ingenious papers about the comet, were communicated to a perſon that I hope will make good uſe of them; and I am glad to know, that I was not miſtaken in thinking they came from Dr. *Marſh*, that I may upon the new ground rejoice, that the college of *Dublin* is happy in ſo learned and worthy a provoſt. Before the receipt of your laſt packet, I was altogether of your lordſhip's mind, that it is not fit all the exemplars of the New Teſtament ſhould ſtay for the preface, (whatever that be) and accordingly I cauſed copies to be bound the ordinary way, beſides much more handſomely bound, for the preſent uſe of the members of the college, and ſome other perſons that your lordſhip or Dr. *Marſh* ſhall think fit to ſend any to. And becauſe to have them tranſported by long ſea (though I were adviſed to that, as much the cheapeſt way) would certainly take up time, and might prove hazardous, I reſolved to diſpatch them by land to *Cheſter*, to the poſt-maſter of which place I got them particularly recommended by Mr. *Dowlin*, poſt-maſter of *Dublin*; where, I hope, they will ſpeedily come ſafe, and be delivered to your lordſhip, whoſe long life, health, and ſucceſs in this good work, is heartily prayed for by,

My Lord,

Your lordſhip's moſt faithful

and moſt humble ſervant.

VIII.

Biſhop of Meath *to Mr.* Boyle.

Honourable Sir, *Dublin*, Novemb. 5, 1681.

YOUR laſt of the 27th of September, came to hand as I was then entering on a journey into the country, where my preſence was neceſſary, which ſo far occaſions this delay in this return to yours.

Therein you mentioned the ſending hither Iriſh Teſtaments, ſome (about 50) bound, and others (about 350 in ſheets) then on their way hither from *London*.

Thereupon I attended our principal officers at the Cuſtom-houſe here, deſiring, when thoſe books arrived, they might be ſafely depoſited under their care, until my return out of the country; the provoſt alſo of the college being then for ſome time in the county of *Kerry*, inſpecting college affairs there, and thereabout.

ON his and my return hither, we found thoſe books here arrived, and laid up accordingly for us about ſix weeks, which, and the bringing them from the Cuſtom-houſe to the college, (where they are laid) was with ſome, but little charge.

IN that your letter mentioned, was alſo intimated ſomething farther expected from you for ordering that parcel of thoſe books which were not bound; of which nothing hath hitherto come to my hand here, that I can underſtand. Therefore are they laid by, not delivered to any book-binder, until further orders concerning them appear. Theſe are depoſited in the college under the provoſt's care, which I conceived the place proper for their ſafe keeping, and after diſperſing them on occaſion.

As to thoſe books ſent over bound, 40, according to your directions, are left with the provoſt for the preſent uſe of the college.

SOME alſo of the books of the better ſort of binding, were preſented as from yourſelf to thoſe great perſons in yours named. Only I preſumed to preſent one of them to the lord lieutenant the duke of *Ormonde*, as from yourſelf (although not mentioned) and found it well accepted by his lordſhip. I conceived this neceſſary, for having his lordſhip's countenance in that great buſineſs when occaſion ſhould require, there being many adverſaries.

THAT which gives greateſt hopes of ſucceſs in this, is our good provoſt's care and zeal in training up the preſent youth in the college in reading the Iriſh, which by the books from you now in their hands is much forwarded.

THESE may be a ſeed plot for the church; the harveſt is great, and labourers few; therefore is the Lord of the harveſt to be earneſtly deſired to prepare and ſend forth more labourers for the great harveſt of ſouls, which we hope for.

WE have received your tranſlation of the Janſeniſts prefaces, and you will ſhortly hear from Dr. *Sall* of his preface alſo. I deſired him firſt to ſend you a copy of that in Engliſh for your approbation, before he entered on his tranſlating it into Iriſh.

I SHALL forthwith put into his hand the Iriſh Old Teſtament in manuſcript, to be by him reviſed. You will alſo have from him the Iriſh Common Prayer-Book for farther conſideration.

As for the Welſh model for ſubſcriptions, which may direct in our proceeding and forwarding this great work, in that we depend on your procuring it, as you ſhall find your opportunities for it. God bleſs you in all. I reſt,

Honoured Sir,

your moſt humble and real ſervant,

Henry Midenſis.

I CONTINUE to mind you of what I have ſome time ſince written concerning my kinſwoman Mrs. *Loane*, tenant to the earl of *Burlington*, for renewing her leaſe now expiring. I am juſt now ſolicited in this, which occaſions your trouble in it.

IX.

Dr. Anthony Dopping *Biſhop of* Meath, *to Mr.* Boyle.

Honoured Sir, Aug. 3, 1682.

I MUST needs acknowledge it as a great omiſſion in myſelf, that I have ſo long delayed an anſwer to your kind and obliging letter, which I had the honour to receive a conſiderable time ſince: and as I deſire not to make any apology for my neglect, ſo I hope you will be inclined to pardon it, upon my promiſe of offending no more in this nature. As for the deſign on foot about the Iriſh Old Teſtament, it goes on, though by ſlow and leiſurely ſteps; but I doubt not within a ſmall time to procure (from ſome well affected perſons to the work) ſuch ſubſcriptions as may add little more of life and briſkneſs to the undertaking; and I hope we may effect this, without putting you to the trouble of ſending the formularies of ſubſcriptions for the Welſh Bible: but I preſume our ſubſcribers here will expect a book when it is printed; and if ſo, I am afraid that the printed copies deſigned may but anſwer the number of ſubſcriptions, and ſo the publickneſs of the work be prejudiced by it. How to obviate this, I leave it to your prudence and judgment to conſider, and contrive an expedient; to whom, as the glory of the undertaking is entirely due, ſo I ſhould think myſelf very criminous, if you were not made acquainted with all the hazards that fœtus muſt run through, which owes its birth to your care and charity. Thus with my humble reſpects I remain,

Honoured Sir,

your moſt humble and obedient ſervant,

Anth. Midenſis.

X.

Biſhop of Meath *to* * * *.

Reverend Sir, December 14, 1685.

YOUR letter, which I received yeſterday, would have been very obſcure to me, if you had not ſent me Mr. *Boyle*'s incloſed in it, as a key to open the ſenſe of ſome things in it. As to the title page, mentioned in his letter, I can ſay nothing to it, being a ſtranger to the tranſactions between him and you concerning it: as to the preface, if by it be meant that which I ſent you, I am much of Mr. *Boyle*'s opinion, that it is improper to have it in Engliſh, and that I intended it to be tranſlated into Iriſh, had not your letter to me imported that the preſs ſtayed for it. But ſince it is now out of my hands, and Mr. *Reily* hath obtained a deſerved character for his knowledge in the language of his own country, I know of no perſon more fit to be employed in it than himſelf.

As for an hiſtorical account of the verſion, I dare not undertake to tell the world, that it is impartially done from the original, having no knowledge of the language of my own country: all that I can add concerning it, is only this, (which, if Mr. *Boyle* thinks fit, may be farther enlarged in its exterior dreſs, but very little as to the ſubſtance) that in the convocation held at *Dublin* 1634, there were no ſmall debates about the verſion of the Bible and the liturgy of the church into the Iriſh tongue, for the benefit and inſtruction of the natives; Dr. *Bedell*, biſhop of *Kilmore*, being for the affirmative, and Dr. *Bramhall*, biſhop of *Derry*, oppoſing it. The reaſons of the former were drawn from the principles of theology, and the good of ſouls; of the latter, from politicks and maxims of ſtate, and eſpecially from an act of parliament, paſſed in this kingdom in the reign of King *Henry* VIII. for obliging the natives to learn the Engliſh tongue. However, the reaſons of biſhop *Bedell* were thought ſo ſatisfactory (eſpecially being countenanced by the authority of primate *Uſher)* that the convocation thought fit to paſs two canons concerning it; the one, that the miniſter ſhould read the liturgy in Iriſh, where moſt of the people were ſo, can. 8. the other, for the pariſh clerk to accompany the miniſter in reading his part of the ſervice in Iriſh, can. 66.

UPON theſe foundations the pious biſhop *Bedell* reſolved to make farther ſuperſtructures; and accordingly ſet himſelf to the verſion of the Old Teſtament into the Iriſh tongue (for the New had been done before at the expences of Sir *William Uſher*, clerk of the council of *Ireland*, and by the care of Dr. *Daniel*, who was after made archbiſhop of *Tuam in Ireland)* taking to his aſſiſtance one Mr. *King* and Mr. *Dennis Sheridan*, both Iriſhmen and clergymen, and excellently ſkilled in the language of their own country, whoſe office it was to tranſlate the then Engliſh verſion into Iriſh, whilſt the biſhop (who was excellently learned in the Hebrew and the Iriſh languages) reviſed the whole work, comparing it with the original, and either expunged or added, as he ſaw it nearer or more remote from the original. The work, thus happily finiſhed, was left by biſhop *Bedell* with Mr. *Sheridan* the tranſlator, (who ſurvived him) and was by him delivered to the late biſhop of *Meath*, Dr. *Henry Jones*, by him communicated to Dr. *Andrew Sall*, from whom I received it before his death, and gave it to your predeceſſor Dr. *Marſh*; and what fate it hath met with ſince, he and others, whoſe hands it hath paſſed, can beſt relate.

SOME part of this narrative I have read in the life of biſhop *Bedell*, lately publiſhed by one *Clogy*, who is ſomewhere beneficed in *England*, (if he be alive) and married the ſaid biſhop's daughter, and may be more particular if conſulted. If I have miſtaken any thing, (as perhaps I may, as to the name of *King*, for I have not the book by me) that book will rectify it: the remainder, relating to Mr. *Sheridan*, you may receive a more ample account of from the biſhop of *Kilmore*, who is his ſon. I have ſent you Mr. *Boyle*'s letter, conceiving, that it may import you more than me. I ſuppoſe the title page ought to be in Iriſh characters firſt, and then in Engliſh; with this inſcription in both: *The books of the Old Teſtament tranſlated into Iriſh, by the Care and Diligence of Dr.* William Bedell, *late Biſhop of* Kilmore *in* Ireland, *and now printed for the public good of that Nation.* London.

IF Mr. *Boyle*'s modeſty would permit, I judge it neceſſary, that at the end of the preface ſomething be inſerted concerning his bounty in the printing it at his own charges, that the nation may know their obligation to ſo worthy a benefactor. I remain,

SIR,

Your moſt humble ſervant,

Anthony Midenſis.

XI.

PREFACE to the IRISH NEW TESTAMENT.

To the Chriſtian people of *Ireland*.

HERE you have, dear countrymen, the New Teſtament of our bleſſed Lord and Saviour Jeſus Chriſt preſented to you, in your country garment, tranſlated unto the Iriſh tongue; and therein the moſt noble, the moſt important, and, to well diſpoſed minds, the moſt pleaſant reading that poſſibly can be offered to their view. I ſay the moſt noble, whether you conſider the Author of it, being the very wiſdom of God, his divine Son Jeſus, and his holy diſciples, taught by himſelf, and by the Holy Ghoſt guiding their pens; whether you look upon the ſubject of this ſacred book, which is the knowledge of divine myſteries, and of the way leading to your everlaſting happineſs; in which conſideration you find alſo the ſupreme importance of this ſacred reading, as guiding you to the kingdom of heaven. And what availeth you to purchaſe all that the world can afford, if you ſhould fall ſhort of that glorious enjoyment? And if God hath commanded his people to read continually the law which he hath given them, and to meditate therein day and night; and if ſocieties, aſpiring to ſome peculiar degree of perfection, do believe they are obliged to read always the rules they have taken to that end; we that are by God's mercy *called unto the fellowſhip of his Son Jeſus Chriſt our Lord*, how ſhall not we think it our moſt neceſſary duty to read, and continually to meditate in the law he has left us as a teſtament and pledge of his inheritance?

1 Corin. i. 9.

FINALLY, the unſpeakable pleaſure of this ſacred reading, was well experienced and frequently atteſted by thoſe godly ſouls who happily have exerciſed themſelves in it. Them you may conceive ſpeaking to the Author of this book, in the Pſalm cxviii. 103. *How ſweet are thy words unto my taſte! yea ſweeter than honey to my mouth*; and in that of *Canticles* iii. *Thy ſpeech is comely.* St. *Clement Martyr* declares, that whatſoever may be deſired for harmleſs pleaſure or profit in profane books, the ſame may be found far more advantageouſly in the ſacred ſcripture, of which he maketh himſelf a large and fair induction, running upon the contents of ſeveral books of the Holy Bible. And St. *Auguſtin* ſays, that *as the ſtore of gold, ſilver and garments, which the Iſraelites brought out of Egypt, was but mean, in compariſon of what they found after in Jeruſalem, as may appear by the wealth of King Solomon: ſo any purchaſe of knowledge got in the books of the heathens, is nothing, being compared to the ſcience of ſacred ſcripture.*

Clement. lib. 1. conſtitut. apoſtolic. c. 7.

Aug. lib. 2. de doctrin. Chriſtian. c. 41.

AND this being true enough, ſpeaking of ſcripture in general, is ſingularly evinced in this divine volume of the New Teſtament of our Lord and Saviour Jeſus. For if you are for the pleaſure of hiſtory, relating wonderful ſucceſſes, other records may repreſent to you men overcoming other men, cities, or wild beaſts: but here you ſhall find a divine man ſubduing the world without worldly arms; the ſea and winds, and all the elements, even the devils, obeying his commands; death itſelf, which trampleth all human power, overcome by him, raiſing others and himſelf from under the power of it, and mounting with his own proper power over the clouds and ſkies unto the higheſt heaven. In the Acts of the Apoſtles you ſhall meet with the like prodigies wrought by his diſciples, illiterate ſimple men triumphing over the ſwelling ſcience of the ſages of this world, kingdoms and nations obeying their word. If you deſire moral inſtructions for ruling your life, and ſteering the courſe of it to everlaſting reſt, you have in the ſermons and precepts of our Saviour ſpread over the Goſpel, and in the epiſtles of his bleſſed apoſtles, the very treaſure of knowledge given to you by God for that purpoſe, and the moſt excellent ſyſtem of ethics and theology that ever was ſeen in the world, all virtues heretofore known to men, there commended and exalted, and others more ſublime never before practiſed in the world, taught by our Saviour and his diſciples in their precepts, and moſt powerfully by their examples. St. *Chryſoſtom*, gloſſing upon the admirable meekneſs of *David*, ſparing his inveterate enemy in the cave of *Engeddi*, 1 *Sam.* xxiv. exhorteth all men to print in their hearts that paſſage; *for it is impoſſible*, ſaith he, *that a mind converſant in ſuch*

Chryſoſt. homil. 2.

kind of-hiſtories ſhould be overcome by paſſions. And is it poſſible, that a man, reading in the Goſpel and in the Acts of the Apoſtles, ſo many prodigious examples of humility, meekneſs and patience, practiſed by our Saviour and his bleſſed diſciples, ſhould be overcome by pride, vanity, or impatience? When he finds the Son of God chearfully bearing affronts offered to him by vile men; when he ſees the holy apoſtles rejoicing, that *they were counted worthy to ſuffer ſhame for his name*, Acts v. 41. ſhall he be ſtartled at every ſmall injury received?

de David & Saul.

AND laſtly, if you are for a ſublime extatic contemplation of heavenly myſteries, you have in the venerable prophetic deepneſs of the Apocalyps ſparkles of divine light, which in pious and humble ſouls do work a profound reſpect of God's infinite grandeur and majeſty.

IF any ſhould ſay, that this precious treaſure ſhould be reſerved for the learned in the original or ſchool languages, and not expoſed to popular eyes in the vulgar ones; whatever be the weight of this conſideration, it has been over-ruled already by all Chriſtian nations, *Italy*, *Spain*, *France*, *Germany*, *England*, and all other kingdoms and countries of the Chriſtian world, do enjoy the glorious light of divine ſcripture in their native idioms. The learned doctors in divinity of the univerſity of *Paris*, have reprinted of late the New Teſtament in the French language, following therein the examples of the doctors of *Louvain* in the ſame faculty, who had ſet forth the whole Bible in the ſame language, and bemoaning in the preface of their edition the ſad effects of the common neglect of reading the holy ſcripture in theſe latter times of Chriſtianity, far otherwiſe than it was in the more holy primitive ages of it. All the writings of the holy and learned fathers of the church are repleniſhed with marks of their reſpect to the word of God delivered to us in the holy ſcripture, exhorting all men to the reading and knowledge of it. The Goſpel, ſay they, is the mouth of Chriſt: he ſits in heaven, but ſpeaks continually on earth. How then dares that man call himſelf ſervant of Chriſt, who cares not to know what he ordains him to do? Or how can he obey him, if he ſlights to hear him? The precepts of the Goſpel, ſays St. *Cyprian*, are the foundation of our hope, and the nouriſhment of our heart. It is in this reading we find light to guide us, ſtrength to uphold us, and remedy to heal us. This made *Cæſarius*, biſhop of *Arles*, to ſay, that thoſe very men, who are not able to read, may not be excuſed from attaining to a competent knowledge of the ſoul-healing doctrine of the Goſpel; for if even ſuch men do find means to learn idle and lewd ſongs and romances, with more reaſon they ſhould apply themſelves to learn the heavenly doctrine of ſcripture. And that they may the better do it, the foreſaid learned doctors of *Paris* and *Louvain*, as alſo other pious and learned men in all nations of Chriſtendom, have taken care to tranſlate the holy ſcripture into the vulgar language of their countries, that the divine doctrine of it may more eaſily come to the knowledge of all ſorts of men. And that *Ireland*, ſometimes inferior to few of the nations in piety and learning, might not be deprived of the like happineſs, has been the pious zeal and care of worthy patriots, whoſe names ought to be with praiſe continued in the memory of poſterity. Such we find to have been *John Kearney*, ſometime treaſurer of St. *Patrick*'s church in *Dublin*; *Nicholas Wailſh*, biſhop of *Oſſory*; *Nehemiah Odonelan* and *William Odonel*, both archbiſhops of *Tuam*, by the help of others ſkilful in the *Iriſh* tongue, the province of *Conaught*, and Sir *William Uſher*, clerk of the council, bearing the charge of the firſt edition of the New Teſtament in Iriſh.

AND now again, that edition being worn out in time, and few copies of it to be found, God has raiſed up the generous ſpirit of *Robert Boyle*, Eſq; ſon to the right honourable *Richard* earl of *Corke*, lord high treaſurer of *Ireland*, renowned for his piety and learning, who hath cauſed the ſame book of the New Teſtament to be reprinted at his own proper coſt. And as well for that purpoſe, as for printing the Old Teſtament, and what other pious books ſhall be thought convenient to be publiſhed in the Iriſh tongue, has cauſed a new ſet of fair Iriſh characters to be caſt in *London*, by a ſkilful artiſan, and an able printer to be inſtructed in the way of printing this language; and having appointed five hundred copies of this new edition to be beſtowed on ſuch as ſhall be thought to make the beſt uſe of them, has extended his charitable zeal towards the printing of the Old Teſtament in Iriſh, giving both the uſe of his characters for that purpoſe, and fifty pounds of his money for a beginning to ſubſcriptions for the publiſhing it. We cannot but hope, that God will excite the ſpirits of other pious men to follow the heroic example of this nobleman, in furthering this holy work for the good and comfort of many poor ſouls, pitifully deſolate and blinded, for want of the heavenly light of the word of God to ſhine among them.

If any other ſhould further ſay, that holy ſcripture, tranſlated unto common language, ſhould not be generally permitted to all men's reading, becauſe ſome, by the ill uſe, and wrong underſtanding of it, have raiſed ſchiſms and many diſturbances; it may be anſwered, firſt, that this argument proving too much proves nothing by the ordinary rules of arguing. It proves too much, becauſe churchmen and the more learned ſhould be prohibited the reading of ſcripture more carefully upon this account, whereas *Arius*, *Pelagius*, *Neſtorius*, aad other prime hereſiarchs, were churchmen, and of eminent learning. Secondly, that becauſe children do cut their fingers with knives, and wicked men do murder others with ſwords, knives and ſwords muſt be taken from all upon the like account. The remedy therefore is to be, not to take away knives and ſwords, and leſs the ſcripture, but the evil uſe of them. To avoid this, and find the ineſtimable treaſure encloſed in ſacred ſcripture, let piety, humility, and diſcretion guiding both, be their companions in reading it. Let their piety appear in ſeeking ſincerely the will of God, and the way to him in that holy reading. Let their humility ſhine in not relying too much upon their own judgment in the expoſition of ſcripture, where it is obſcure, but praying to God, and ſeeking the help of the moſt warrantable teachers for the right underſtanding of it; in the choice of which teachers, their diſcretion is to be practiſed, relying more upon the moſt ancient, moſt holy, and moſt generally approved in the Chriſtian church.

And if, after uſing their beſt endeavour to find out the right underſtanding of ſcripture, ſome places of it ſhould continue obſcure to you, take from the learned, humble and holy doctor St. *Auguſtin*, this excellent leſſon for judging of thoſe ſacred books: *Where I underſtand them*, ſays he, *not only nothing can be more wiſe, but alſo nothing ſeems to me more well ſpoken: and where I do not underſtand them, their eloquence is not ſo apparent to me; but, even then, I do not doubt but it is ſuch, as where I do underſtand them.* God were not God, if to be comprehended by human wit. If any of his words ſeem to you not ſo agreeable to reaſon, make ſure account the fault lies in your weak underſtanding; wait patiently, pray and read again, and hope in God's mercy, that he will open your eyes to a right underſtanding; and the more often you read, the more light you ſhall daily receive. Mark what the ſame great doctor St. *Auguſtin* declareth of his own experience. *I do publickly avouch*, ſays he, *the depth of Chriſtian ſcriptures to be ſuch, that I would daily have more and more to learn in them; though from my childhood to decrepit old age, I ſhould with full leiſure, moſt eager ſtudy,*

Aug. lib. ult. de doct. Chr. c. 6.

Aug. epiſt. 3.

and greater wit, endeavour to learn them. But the ſame holy and learned doctor giveth you for comfort what is certainly true, that in what is plain of ſcripture you have all that is neceſſary for your inſtruction, both in belief and in a good life, which made St. *Paul* writing to *Timothy* tell him, that *the holy ſcriptures, which from a child he had known, were able to make him wiſe unto ſalvation.* 2 Tim. iii. 15, 16, 17. Whereof he giveth the reaſon, adding immediately, that *all ſcripture is given by inſpiration of God, and is profitable for doctrine, for reproof, for correction, for inſtruction in righteouſneſs; that the man of God may be perfect, thoroughly furniſhed unto all good works.*

Now therefore the excellency of this holy book being ſo incomparable, and ſo vaſt the importance of it, it is the expectation of the pious and learned men, who have laboured in publiſhing it, and their earneſt requeſt even for Chriſt Jeſus his ſake, that all good Chriſtians capable to make uſe of it, ſhall in their reſpective ſtations reverently frequent the reading of it, for the benefit of their own ſouls, and of others about them, or under their charge; and more eſpecially, that all godly churchmen, who have the cure of ſouls, may read, or cauſe it to be read to their congregations, whereto underſtanders of the Iriſh tongue do reſort, or upon this encouragement may reſort more; conſidering, that no preaching is ſo effective to work upon ſouls, as the very *word* of *God*, which, as St. *Paul* ſays, *is quick and powerful, and ſharper than any two-edged ſword.* Heb. iv. 12. For the ſame reaſon, pious fathers of families ſhould cauſe it to be read on fit ſeaſons to their houſhold, inuring them to a deſerved love of this ſacred reading, in lieu of romances and other idle or noiſome divertiſements. Foreigners alſo, who deſigning to continue their habitation in *Ireland*, will, by the rules of common prudence, deſire to purchaſe a knowledge of the language, may attain it by reading this book, comparing each chapter of it with another of the ſame contents in the language they underſtand; for which purpoſe ſome ſhort rules have been already, and more ſhall be publiſhed, for the right pronouncing and reading the Iriſh tongue.

FINALLY, ſtudents in ſchools and univerſities, who deſign to live by the cure of ſouls in *Ireland*, ſhall upon a ſerious conſideration find it their preciſe duty to procure ſuch knowledge in the language of the natives, as may enable them to help and inſtruct the ſouls committed to their charge, and of which they are to give account to God; for notwithſtanding all the wiſe ſtatutes and endeavours uſed to bring this whole nation to a knowledge of the Engliſh tongue, experience ſhews it could not be effected, too many being unable to give ſuch teaching to their children, or to get it for themſelves; and it is apparent, that in *Ireland* there are many pariſhes, baronies, and whole counties, in which the far greater number of the common people do underſtand no other language but the Iriſh. This being ſo, how can it be preſumed of any godly paſtor of ſouls in ſuch places, that he ſhall not procure the ſpiritual welfare of thoſe men by the ſweat of whoſe brows he hath his bread, enabling himſelf to preach, read to, or converſe with them in a language they can underſtand, that being the way to gain their good-will, and thereby to win their ſouls to God? For very true and experienced is what *Ariſtotle* ſaid, *plurimas amicitias taciturnitas ſola diſſolvit*, that want of communication breeds want of love and union. And to gain their affection is the way to benefit their ſouls, and cure them of the moſt deplorable blindneſs they are in, ſitting long time *in darkneſs and in the ſhadow of death*, for want of ſetting before them the heavenly light of the word of God, which his loving providence hath given as *a lanthorn to our feet*, in this our pilgrimage towards our deſired home of heavenly glory. Pſal. cxix. 104. And now the Lord and giver of it, our dear

Saviour Jeſus, who hath inſpired the thoughts of publiſhing this ſacred book for the good of ſouls, be graciouſly pleaſed to grant all others concerned to make due uſe of it towards the glory of God, and the ſpiritual welfare both of others and their own. Amen.

NUMBER IV.

Letters and papers relating to the diſtributing of the Iriſh Bibles in the *Highlands* of *Scotland*, at the expence of Mr. *Boyle*. *Life*, p. cxxxix.

I.

Mr. Kirkwood'*s letter to Dr.* Wotton, *containing a general account of Mr.* Boyle'*s concern in the edition of thoſe bibles.*

Reverend Sir, June 22, 1702.

I AM glad you are going to publiſh the life of the excellent Mr. *Boyle*, who was ſo great an ornament to his country and to our holy religion. I reckon it one of the bleſſings of my life, to have been acquainted with ſo extraordinary a perſon, whoſe company I found always very delightful and edifying. It was ſoon after our acquaintance was begun, that I had the opportunity of talking with him of the ſad ſtate of religion in the *Highlands* of *Scotland*, where they had neither bibles nor catechiſm in their own language. This gave him an occaſion to tell me of his having cauſed five hundred bibles in Iriſh, in a quarto volume, in the Iriſh character, to be printed for the uſe of thoſe in *Ireland*, who underſtood not the Engliſh. He then offered a few of thoſe bibles to be ſent into *Scotland*, to ſee what reception they might meet with there: a dozen of them was firſt ſent, and afterwards two hundred; which made one for each pariſh. In ſuch places, where theſe books were diſtributed, they had a very good effect, as you may perceive more fully by the papers relating to that affair, which I ſend you. After ſome time, I heard from ſome miniſters in thoſe parts, that it was the earneſt deſire of many who wiſhed well to our religion, to have a new impreſſion of the bible in the Latin character, in a ſmall volume, for the uſe of ſuch perſons in the *Highland* pariſhes, as had been taught to read Engliſh, though they did not underſtand it, and for the advantage of ſuch children, as ſhould be ſent to ſchool, eſpecially thoſe of the poorer ſort, who could not purchaſe books for themſelves. To anſwer the pious deſires of ſuch perſons, endeavours were uſed in *Scotland* to procure another impreſſion; but in this attempt we met not with ſucceſs. The firſt encouragement that was given me to go on with it in this kingdom, was by the worthy Mr. *Boyle*, who told me, he would ſubſcribe for printing one hundred bibles. This example diſpoſed others, whom I acquainted with the deſign, to ſubſcribe very freely and largely. Most of the ſubſcriptions you will ſee in the printed paper I ſend you relating to that affair. I need not mention other particulars, only in ſhort, after ſome few years, this work was happily finiſhed, the books were printed, tranſmitted into *Scotland*, and long ago the greateſt part of them was diſperſed in the ſeveral counties of the *Highlands*. We have had many accounts of the happy effects, which have attended our endeavours, in behalf of thoſe poor people, who have been ſo long neglected and ſuffered to remain in a ſtate of ignorance and barbarity. By the printed paper you will likewiſe ſee, that there were ſix thouſand catechiſms and prayer-books in Iriſh printed at the only charge of Mr. *Boyle*, for the uſe of the Highlanders, which were accordingly ſent down into *Scotland*; many of which have been likewiſe diſperſed among the Highlanders. The catechiſm and prayers were compoſed by Mr. *Charteris*, and were tranſlated into Iriſh by Sir *Hugh Campbell*, of *Caddel*, and afterwards reviſed and corrected by Mr. *Kirk*. There were added to the catechiſm ſome paſſages of ſcripture, containing the principal heads of the Chriſtian religion, to ſerve as a ſhort and plain tract of devotion and Chriſtian morality. It was this catechiſm and prayers, that are mentioned in the biſhop of *Roſs*'s letter. As for the papers I ſend you, which have ſome relation to that great man, I leave it to you to make uſe of them, or any part of them, in ſuch ſort, as you judge may beſt ſerve your deſign. When you have done with them, be pleaſed to return them to me again, to be left for me at Mr. *Milbourn*'s a Watchmaker at the *Blue Boar* in the *Old Baily*, near *Ludgate*, *London*. As for the letter from Mr. *Charteris*, I ſend it you, that you may ſee the high opinion, that pious and primitive perſon had of the famous Mr. *Boyle*. As for Mr. *Boyle*'s own letter, the chief reaſon why I ſend it, is upon the account of ſome few ſeaſonable expreſſions in it about education, which may be very well improved in ſome part or other of his life. As for the letter from Mr. *Kirk*, a Highland miniſter, who was corrector afterward of the preſs, when the Iriſh Bible was reprinted, perhaps it will furniſh you with ſome hints not unuſeful to your deſign. I need ſay no more as to this matter. I heartily wiſh you may have good ſucceſs in what you are about, that you may be able to ſet forth ſo great a pattern and example in ſuch a light, as may move others to have a higher regard for ſolid piety, and uſeful learning. Before I conclude this letter, I muſt acquaint you with another deſign, which has been ſet on foot, in behalf of the miniſters, ſchool-maſters and probationers, in the *Highlands*. The reaſons of this deſign you will ſee in the printed paper I ſend you. They, who are now in the government there, have ſo far eſpouſed it, as to recommend it to the ſeveral preſbyteries to promote it. I know your good affection to religion and learning will diſpoſe you to encourage a work of this tendency, which is likely to prove of very great uſe and advantage. Your neighbour, Mr. *Frank*, can acquaint you with ſome farther particulars relating to this affair. I am,

Reverend Sir,

Your affectionate brother,

and humble ſervant,

JA. KIRKWOOD.

An

II.

An account of the deſign of printing 3000 Bibles in Iriſh, for the uſe of the Highlanders.

THE inhabitants of the *Highlands* of *Scotland* have never had the bible in their own language, which is the Iriſh. Some endeavours have formerly been uſed in *Scotland*, to have it printed in that language, but hitherto they have proved unſucceſsful. Of late the honourable and pious and learned Mr. *Boyle* hath, at great charge, cauſed to be printed 500 Bibles in Iriſh, which were deſigned for *Ireland*; and accordingly moſt part of them, eſpecially the New Teſtaments, were by him ſent thither. Of the books of the Old Teſtament which remained, he ſent above two hundred into *Scotland*, which made one book for each pariſh in the *Highlands*, there being ſo many pariſhes in thoſe parts, which are of very large extent, containing great numbers of people. The ſame excellent and worthy perſon was at the charge of printing, for the uſe of the Highlanders, 3000 catechiſms and prayer-books in Iriſh, with ſome paſſages of ſcripture, containing the principal heads of the Chriſtian religion, they never having had any ſuch helps before. He hath alſo lately given money for to reprint as many more. Several of thoſe bibles and catechiſms have been diſtributed among ſuch pariſhes in the *Highlands* as are near *Edinburgh*, and where the preſent commotions have not prevented their being ſent. The reſt will now be ſent very ſpeedily. In all thoſe places, where they have been ſent, the people expreſs a wonderful joy, and a great deſire to know the word of God; ſo that they, that can read, are at ſome pains to teach others to read likewiſe. And ſuch is their zeal, that they ſend for the bible ſometimes to one part of the pariſh, and ſometimes to another, that they may read on the week days; and then they return it to the church on the Lord's day, that all may hear it read publickly. And it is very remarkable, that amidſt the public commotions in that kingdom, ſcarce any of thoſe Highlanders, who have received bibles and catechiſms, and have been inſtructed in the knowledge of truth, have joined themſelves to the adverſaries of the preſent happy ſettlement. All ſort of people among them expreſs an earneſt deſire to have another impreſſion of the bible in Iriſh for themſelves and for their children, that by reading and hearing of that ſacred book, they may be delivered from ignorance and barbarity. The money collected for carrying on the work is to be depoſited with the reverend doctor *Horneck*, who hath been pleaſed charitably to accept of the trouble of receiving it. Several worthy perſons both of *England* and *Scotland*, have promiſed their concurrence, and ſignified the proportions they intend to give, of whom alſo not a few have already ſent their money to Dr. *Horneck*, at his houſe in *Exeter-ſtreet*, who hath a liſt of their names, with the ſums of money they have ſubſcribed or ſent. They who have not yet ſent their money, are deſired to ſend it as ſoon as may be. The perſon employed for the printing this bible is *Robert Everingham* at the *Seven Stars* in *Ave-mary lane* nigh *Pater-noſter-row*. Mr. *Robert Kirk*, miniſter of *Aberfoyle* in *Monteith* in the *Highlands* of *Scotland*, is come to *London* to attend the preſs. The work is now begun, and will be finiſhed (God willing) before Chriſtmas next. The books, which are ſubſcribed for by ſome charitable perſons of this kingdom, are to be diſtributed (by the advice and with the concurrence of perſons of public authority in the kingdom of *Scotland*) amongſt the poorer ſort of Highlanders. As for the books, which are ſubſcribed for by thoſe of *Scotland*, they are likewiſe to be beſtowed on poor people, ſo far as is needful; and as for the money, which ſhall be got from thoſe Highlanders, who are able to buy books for themſelves, it is to be kept in ſtore, as a fund for another impreſſion of the bible in Iriſh, or for printing ſome ſuch uſeful treatiſe in that language, as ſhall afterwards be thought moſt proper. We have ſufficient reaſon to expect, that ſchools will be ſettled in all pariſhes of the *Highlands*, that thereby this charitable deſign may be rendered effectual. This will be ſhortly recommended to the care of the parliament in *Scotland*. The paper and printing amounts to 275 *l.* at 1 *s.* 10 *d.* per book in quires, the binding (at 8 *d.* per book) amounts to 100 *l.* which in all makes 375 *l.* Towards which we have the following ſubſcriptions, which amount to 244 *l.* 8 *s.* 8 *d.* So that there is wanting to complete the charge of paper and print 30 *l.* 11 *s.* 4 *d.* and for binding 100 *l.* in all wanting 130 *l.* 11 *s.* 4 *d.* There are alſo New Teſtaments to be printed, for the uſe of young children learning at ſchool, the number of which will be proportioned to the money, that ſhall be given.

	l.	*s.*	*d.*
Mr. *Boyle* gave for this charitable uſe	8	6	8
As alſo did ſeveral other perſons the ſum	236	2	0
Total amounting to, with Mr. *Boyle*'s,	244	8	8

III.

An anſwer to the objection againſt printing the bible in Iriſh.

IT is not to be doubted, that a great many who make this objection, do it without any bad deſign, but only through their not conſidering the matter ſufficiently. And therefore to remove their miſtake and prejudice, the following conſiderations are offered unto them, whereby theſe two things will appear. 1ſt, That the extirpation of the Iriſh language, out of the *Highlands* of *Scotland*, is not poſſible in this age. And ſecondly, That it is not probable to be effected in ſucceeding ages. As to the firſt, it may appear ſufficiently to any who will conſider the methods of doing it. As 1ſt, By colonies. 2dly, By ſcattering the Highlanders all over the nation. 3dly, By ſchools. 4thly, By ſending their children to ſerve in other parts of the kingdom, and ſo to learn Engliſh. As to the firſt, if ſeveral colonies ſhould be ſent among them, what could they do towards the extirpating the language, unleſs we ſhould ſuppoſe them more numerous than the inhabitants, which is impracticable? For neither could the reſt of the nation ſpare ſo great a number of people, nor yet could the barren mountains of the *Highlands* afford them the conveniences of life. Beſides, the Highlanders not living in towns, but ſcattered up and down in very ſmall villages and ſingle houſes, the ſettling of colonies among them could not ſignify much to root out their language. As to the 2d of ſcattering the Highlanders all over the nation, it is an idle dream, not at all practicable; for neither would the generality of the Highlanders leave their native ſoil, nor yet would others readily receive them. And, 3dly, As for ſchools, neither can they do it: for firſt we may ſuppoſe, that there will be ſtill too many, who go not to ſchool through the poverty and negligence of the parents, and uncharitableneſs of richer neighbours: now thoſe that tarry at home, muſt needs retain their own language. 2dly, We may very reaſonably ſuppoſe, that many who ſend their children to ſchool to learn to read Engliſh, are not able to keep them ſo long under the care of a ſchool-maſter, as to have time to learn to ſpeak it, though they may have time to read and underſtand it pretty well in books. 3dly, Suppoſe one of ten or five children, who are ſent to ſchool, ſhould learn it perfectly to ſpeak it, and underſtand it when ſpoken, yet what is this to the extirpating the language in this age? What muſt be done with the four or five, or the nine or ten children, not to ſay any thing of aged perſons, who cannot ſpeak Engliſh? Ought not care to be taken, that they periſh not for lack of knowledge? Or ſhall the bleſſed means of heavenly light and comfort be with-held from them, as from a generation, that God hath curſed and forſaken? Far be it from all thoſe that bear the honourable name of Chriſtians, to harden themſelves againſt their brethren, and under pretence of any human policy to fight againſt the kingdom of our God and Saviour. As to the fourth, to wit, ſending children from the *Highlands* to ſerve in thoſe parts, where they ſpeak Engliſh, it can ſignify but very little; for there are not many (in compariſon) who can be ſent into other parts of the kingdom; and it is known that many who have continued for ſome years in thoſe parts where they ſpeak Engliſh, have never acquired ſo much ſkill in that language, as to be able to underſtand a ſermon in it. As to the ſecond thing, that it is not very likely to be effected in ſucceeding ages, may appear from the following particulars. 1ſt, Where is here an inſtance of any ſuch thing, that has been done any where in the world, except in ſuch places where the conquerors have been more numerous than the conquered? It is known to all, who are acquainted with the ſtate of *Europe*, that in moſt kingdoms there are ſome provinces, which ſpeak a different language from what is ſpoken in the reſt of the provinces of the ſame kingdom. 2dly, Many thouſand Highlanders are ſeparated from the reſt of the nation by iſlands, ſo that they have very little converſation or commerce with any but thoſe of their own language; and therefore it is very hard to imagine, how their language ſhould be extirpated. 3dly, It has not been known or heard of in this age, nor for any thing we can learn in ſome paſt ages, that any one pariſh, where they have been wont to preach in Iriſh, has learned ſo much Engliſh, as not ſtill to need a preacher in the Iriſh language. 4thly, In ſeveral pariſhes in the borders of the *Highlands*, where they preach only in Engliſh, there remain ſtill ſome corners, where the inhabitants go to have their children baptiſed, and to hear ſermons in the adjacent pariſhes, where they preach in Iriſh, becauſe they underſtand not thoſe that preach in Engliſh; which plainly ſhews, that the Engliſh language does not prevail ſo much over the Iriſh, as ſome would make the world believe. 5thly, About one half of the miniſters in the *Highlands* (according to our beſt information) preach only in Iriſh, which ſhews, that there is very little Engliſh underſtood in their pariſhes. And where they preach one half of the day in Engliſh, it is in many places of very little uſe to the generality of the people; of whom ſeveral can buy and ſell in Engliſh, who do not underſtand a ſermon in that language. As to the women and children, ſcarce one in twenty can ſpeak Engliſh throughout the *Highlands*. 6thly, It is very conſiderable, that in *Kintyre* (a country in the *Highlands*) whence the Highlanders were expelled, and where others, who ſpoke Engliſh, were planted in their ſtead, in proceſs of time, by frequent converſation with the neighbouring Highlanders, many of them, inſtead of propagating the Engliſh language, have learned Iriſh; ſo that now they preach once a day in Iriſh in the chief churches of the country. To which might be added, 7thly, That the Highlanders, as much as any people, are very much in love with their own ancient language, and jealous of all deſigns againſt it. After all that hath been ſaid, it doth ſufficiently appear, how little ground there is to think it likely, that the language of two hundred thouſand perſons can be extirpated; for by a moderate computation, there can be no fewer in the *Highlands*, where there are about two hundred pariſhes, which makes above a fifth part of the pariſhes of the whole kingdom: and therefore how neceſſary it is to carry on and encourage this deſign for the further reformation of ſo conſiderable a part of the kingdom. When once the children are provided with Iriſh Bibles, and taught to read them, they will be means of inſtructing the families, to which they belong. And beſides the great advantage, which people in general will reap by this charitable work, the miniſters themſelves, and ſtudents in divinity, will thereby receive unſpeakable advantage. It will prevent the miſtakes, which many have been guilty of in tranſlating the holy ſcriptures; and by reading the Bible in Iriſh, which will be no hard matter to do, for thoſe who can read Engliſh, this impreſſion being in the Latin character, they will be enabled in an apt and uniform ſtile (becoming ſoon familiar by uſe) with greater advantage to expreſs their thoughts in their addreſſes to the people, both in public and in private. The ſcriptures are the weapons of the Chriſtian warfare; and ſhall we unchriſtianly and unmercifully deprive our brethren of that which they have ſo great need of in their defence and ſafeguard? How careful has the church been in all ages (except ſince Popery prevailed in the world) to tranſlate the Holy Bible into the language of all nations, which were converted to Chriſtianity! And, not to mention what was done in other countries, *Bede* tells us, that in this iſland the Bible was read in five dialects, then vulgarly uſed, to wit, of the Angles, the Britains, the Scots, the Picts, and the Latins. How juſt a prejudice againſt the Papiſts is their hindering people from reading the holy ſcriptures! They put that light under a buſhel, which God hath appointed to be ſet on a candleſtick.

IV.

Mr. Kirkwood *to Mr.* Boyle.

Honoured Sir, *Aſtwick*, Nov. 5, 1687.

THIS is only to ſhew you, that the biſhop of *Roſs*, with the clergy of his dioceſe ſynodically aſſembled, by one of their number deſires, that thanks may be returned you in the moſt affectionate manner, for that very good work, the making the ſcriptures to be more known, when others have deſign to eclipſe them. The number of the miniſters of that dioceſe are 31. They propoſe this, that if you have ſo many Bibles as may ſerve them, each of them one, they are moſt willing to pay for them. As for the catechiſms, they give this account, that the province is ſufficiently furniſhed with them already in the Iriſh tongue. This is all that I have heard of late. I know not whether theſe catechiſms, which they mention, are printed: if it is ſo, they are both tranſlated and printed very lately. I hope to hear ſhortly from other parts of the *Highlands*, where there is ſtill, for ought I know, need of ſuch helps. I ſhall not fail to acquaint you, whenever I hear from thoſe parts. When theſe ten or twelve bibles (which my friend Dr. *Denvane* told me you intended to give to the Highlanders) are bound, I have deſired a friend of mine to ſend them carefully. If you think fit to ſend ſome dozens more unbound, I ſhall take care, that they be bound in *Scotland*, and diſtributed to the beſt advantage. I have wrote to thoſe who divide them amongſt the Highlanders, to give them chiefly to ſuch miniſters, whoſe pariſhes are in greateſt want of them, with theſe two conditions: 1ſt, that the miniſters read ſome chapters every Lord's day to the people who never heard a chapter read in their lives; and on other days, when they can have occaſion, as at baptiſms, burials, marriages, *&c.* The ſecond is, that the miniſters take care of the Bible, as being for the uſe of the pariſh, that it be not alienated from this deſign; ſo that if a miniſter be removed into another pariſh, or die, yet the Bible may ſtill be kept and preſerved for the uſe of the ſame pariſh. If there are more bibles than ſerve each pariſh, then ſome may be given to the more numerous and conſiderable families, for the benefit of the children and ſervants, who know not the Engliſh tongue. I thought fit to mention the ſending a good many bibles together, becauſe I hope they may be more uſeful now than before, having heard of the good ſucceſs, which ſome zealous perſons have had in preaching amongſt them of late. As to the number of the pariſhes in the *Highlands*, I believe they are not above 200, if you think fit to ſend ſo many. I doubt not but it would be of ſingular advantage towards your deſign, if you wrote a letter, addreſſed in general to the biſhops and clergy of the *Highlands* of *Scotland*, or to the miniſters of the Goſpel in the churches of the *Highlands* of *Scotland*, wherein might be repreſented to them your deſign in cauſing theſe bibles to be printed, and the deſire you have, that they may contribute their aſſiſtance in making your deſign effectual for the purpoſes of the Goſpel. I know your piety and zeal to be attended with ſo much prudence and diſcretion, that you are able to do this in that manner, which will greatly conduce towards the rendering your work, which you have done already, ſo much the more profitable. For the better diſperſing of ſuch a letter (which I very much wiſh) there might be 4 or 500 of them printed: or, if you are not for that, care ſhall be taken to tranſcribe ſeveral of them. I hope you will pardon the freedom I uſe in this matter. If I know how to be ſerviceable to you in any good deſign, you might expect to find me always ready to ſhew you, how much I am,

Honoured Sir,

Your moſt affectionate and moſt humble ſervant,

James Kirkwood.

He whom I have deſired to take care of the bibles, is called *Oſwald*, a bookbinder, at the *Blue Poſts* in *Air-ſtreet*, *Piccadilly*.

I have wrote to *Scotland*, that if it will do well enough to print the little books in the Engliſh character, as I believe it will (for all who can read the one, as I am informed, can read the other) then they may print as many as they think fit at *Edinburgh*, where they can eaſily find an Highlander to overſee the preſs. This, if it ſucceed, will prevent a great deal of trouble. What expence they are at in printing them, may be ſent by a bill of exchange.

V.

Mr. Kirkwood *to Mr.* Boyle.

SIR, *Aſtwick*, Dec. 15, 1687.

YESTERDAY I heard from Mr. *Oſwald* (of whom I wrote to you) that there are 32 bibles ready to be ſent with the firſt occaſion. I have writ to the biſhop of *Roſs* this incloſed, which I thought fit to ſend to you, that if it is not wholly according to your thoughts and deſign (which I have preſumed to gueſs at in this manner) then you may either acquaint me more fully with your mind, or deſire Mr. *Oſwald* to write to the biſhop, unleſs perhaps you may think good to write yourſelf. If you think fit to ſend my letter as it is, be pleaſed to ſeal it up, and let Mr. *Gregg* deliver it to Mr. *Oſwald*, to whom I have written about the bibles, that they may be ſent ſafely to *Scotland*. Since my laſt to you I have heard concerning the Iriſh tranſlation of the prayer-books, *&c.* that they have a great mind, that they be printed in *Scotland*, if it can be got done. I heartily pray for your preſervation, and that God may direct and aſſiſt you in all your ways. I am,

SIR,

Your moſt affectionate and

moſt humble ſervant,

JAMES KIRKWOOD.

VI.

Mr. Kirkwood *to the biſhop of* Roſs.

My Lord, *Aſtwick*, Dec. 14, 1687.

HAVING received a letter from one of my friends in *Scotland*, wherein I was told, that it was your lordſhip's deſire, as alſo of all the clergy of your dioceſe, that thanks might be returned to the excellent Mr. *Boyle*, for his generous and charitable offer of ſome bibles for the uſe of the Highlanders; I did accordingly acquaint him therewith. Since which time he hath been pleaſed to cauſe 200 bibles in the Iriſh tongue to be bound, whereof one is for your lordſhip, and thirty-one for the miniſters of your dioceſe. The reſt are for the uſe of other pariſhes of the *Highlands*. I know Mr. *Boyle*'s deſign in cauſing theſe bibles to be printed; and now his beſtowing them freely where they are needful, is, that he may thereby contribute his aſſiſtance towards the propagating the knowledge of the Goſpel of Chriſt Jeſus, that by theſe ſacred writings, they who are in darkneſs, and ignorant of divine things, may be enlightened, and reſcued from ſin and folly. He deſires no other reward for theſe books, but to hear they produce the effect, which he heartily wiſhes and prays for. I doubt not but that your lordſhip will recommend to your clergy ſuch methods, as in Chriſtian prudence you judge may be moſt proper and effectual for promoting ſo good a deſign. Mr. *Boyle* hopes, that the miniſters will read every Lord's day ſome chapters to the people, or cauſe them to be read by fit perſons, at that time, and at other times, when people come to church. Theſe bibles are intended for the uſe of the pariſhes; and therefore you will be pleaſed to acquaint your clergy therewith, leſt otherwiſe by the death of the miniſters the bibles fall into the hands of thoſe who will not take much care to ſerve this deſign. I am mighty glad, that it hath pleaſed God to raiſe up Mr. *Boyle* to perſue ſo excellent a deſign, as the ſpreading abroad the knowledge of the holy ſcriptures, which he hath done likewiſe in ſome other languages: ſo that he hath not only afforded great light to the world by his excellent philoſophical writings, but, which is far better, hath done his utmoſt to fill ſome of the dark places of the world with the light of divine truth, by theſe uſeful tranſlations of the bible into ſo many languages. How much may ſo great an example make many of our ſacred order to bluſh and be aſhamed, conſidering that we do ſo little good in the world, and concern ourſelves ſo little for the honour of our great maſter; have ſo little love for the ſouls of men, and are ſo barren of good deſigns and works, although we are appointed by God for this very thing, to make it our buſineſs to ſave men's ſouls, to make them wiſe and good, to turn them from darkneſs to light, by the light of pure and holy doctrine, and of an heavenly converſation? And as at all times this ought to be our meat and drink, ſo eſpecially when we ſee how great pains many are at to fill the world with darkneſs, to corrupt men's hearts and lives, to lead them into errors and deluſions. I humbly beg your lordſhip's bleſſing and prayers. I am,

My Lord, *&c.*

JAMES KIRKWOOD.

VII.

Mr. Kirkwood *to Mr.* Boyle.

SIR, *Aſtwick*, Dec. 27, 1687.

AFTER a long and tedious expectation, I have at laſt received a letter from the Highland miniſter, to whom the tranſlation of the ſmall treatiſes was entruſted, giving me an account of his having finiſhed that work, and his having made ſome conſiderable additions for the uſe of the Highlanders. The miniſter's name is Mr. *Kirk*, who is one of very great worth, a learned, pious, zealous man. The tranſlation is at *Edinburgh*. I have wrote to ſome perſons there to take care there be printed about 2000 of them in the Latin character, according to Mr. *Kirk*'s deſire. He tells me, that both miniſters and gentlemen rejoice mightily to think, that there are hopes of their ſeeing the Bible with catechiſms and prayer-books in their own language; and that the miniſters are very willing to read the ſcriptures publickly in their churches. He ſays, a great many miniſters and gentlemen would buy the bibles, if they were ſent down, and the price ſet on them; and that 200 might quickly be ſold: but (he adds) the *Highlands* require 2000: and if the Bible were reprinted (which he ſays were fit to be done in the Latin character) they could ſell them, and get the money very ſoon. He ſays (which is very juſt) it behoved to be well attended, if there were another edition, by a very ſkilful Iriſhman. He hath remarked ſeveral errata in this edition, one letter for another, and ſome words wanting, which he promiſes to ſend me in a ſheet of paper, if ever there be need of it. He ſays, that generally all people in the *Highlands* are overjoyed with the thoughts of this, and are mighty eager to have bibles. He bleſſes the God of heaven and earth, who put this excellent deſign into your breaſt, and adds [who knows what a heavenly flame this ſpark may kindle?] He adds a great many pious and zealous expreſſions, which I have not at preſent time to tranſcribe. I would have ſent you the letter itſelf, but that it is ſuch as I am unwilling to loſe, and I know not, if this ſhall come to your hands. Not many days ſince, I ſent you a letter, with one incloſed to the biſhop of *Roſs*; I know not whether you have received them. Inſtead of this letter, I had a great deſire to have come myſelf, and talked with you of this affair, but that I am not as yet fit to travel ſo far, eſpecially in this ſeaſon of the year. If you think fit to ſend what bibles you have, if you have 200 remaining, you need not be at the trouble to cauſe them to be bound; they may be ſent in ſheets, with thoſe which Mr. *Oſwald* hath received for the dioceſe of *Roſs*. If you judge it proper to put any price upon them, let me know, and I ſhall take care, that the money be received at *Edinburgh*, by thoſe who will ſend it by a bill of exchange faithfully. A part of the price may be employed in printing the little treatiſes. As for the reprinting the 2000, I wiſh it were done. It were a deſign worthy of the concurrence of ſeveral perſons, who have money, charity and zeal together: if there were ſuch an edition of ſo many bibles, I doubt not but the money might be recovered by the ſale of them, if that were needful. For my own part, I would willingly contribute 10 *l.* towards ſuch a work. I cannot but have a great compaſſion for many poor ſouls, which live under a ſad night of ignorance, ſuperſtition, and profaneneſs in thoſe barbarous places. Mr. *Kirk* tells me, he hath drawn up an Iriſh vocabulary, a grammar, and a collection of Iriſh proverbs, which makes him deſirous of a ſmall but competent number of Iriſh types, with the contractions, for which he will ſend money. If you think fit to let him have any of yours, they may be ſent with the bibles, and the price of them (if you let me know it) ſhall be ſent him. He writes of an Iriſh book or two lately printed at *Rome*, which I would buy for him. If you know any Iriſhman at *London*, who deals in books, I make bold to deſire you to cauſe your man, Mr. *Gregg*, to enquire about them, and to buy them for me, that they may be ſent, when other things are ſent. He writes a long account of the reaſon of the great delay in his tranſlation, which is too tedious to tranſcribe. At the ſame time, when I received this letter of Mr. *Kirk*, I received another from the excellent author of the catechiſm and prayer-books, Mr. *Charteris* (formerly profeſſor of theology at *Edinburgh*, but hath lived privately ſince the teſt) who deſires me to ſhew you, that among many others in *Scotland*, he does highly honour and admire you, for your ingenious and learned labours, and your fervent charity and zeal; and wiſhes, that the Lord may long preſerve you, and may ſtir up others to imitate your excellent virtue and piety. I know you value not yourſelf one whit the more, becauſe others do ſo, and therefore I am not afraid to let you know what this excellent perſon did deſire me. I heartily join with my worthy friend in wiſhing, that the Lord may long preſerve you, and that he may direct and aſſiſt you in every good word and work. Whenever I think I may venture upon a journey to *London*, I intend to come and wait upon you. I make bold to deſire you to give my moſt humble ſervice to your excellent ſiſter, my lady *Ranelagh*. I am,

SIR,

Your moſt affectionate

and moſt humble ſervant,

JA. KIRKWOOD.

VIII.

Mr. Boyle *to Mr.* Kirkwood.

Worthy Sir, *London*, March 5, 1687-8.

WHEN I come to look upon the date of your letters, I cannot but be much troubled to find, how long I have been your debtor for an anſwer. And I ſhould be as much aſhamed, as troubled at it, but that it pleaſed God to afflict me with a diſtemper, which, though his goodneſs made very ſupportable to me, yet it has made me ſo weak and tender, that I have not ſtirred out of the houſe theſe two months: during which time I was indiſpoſed to keep a literary correſpondence, even with my near relations; whereto I muſt add, that, during a good part of the time, your letter was unhappily miſlaid. The biſhop and preſbytery of *Roſs* put too high a value upon my weak endeavours in a work, that need not expect a retribution from men. But I am very glad to find by their proceedings, how well they reliſh thoſe things, that aim at the propagation of the Goſpel; and I wiſh, that all, that ought to be equally zealous to promote affairs of this kind, would ſhew themſelves equally concerned for the advancement of this. Some paſſages in your letter oblige me to take the freedom to tell you, that I perceive you have not been rightly informed of my circumſtances in reference to the publication of the Iriſh Bible. For at firſt my aim was, as I thought it became me, to do ſome ſervice to the country, wherein I was born, and have ſome little eſtate, though I have lived a great ſtranger to it; and eſpecially to contribute to the converſion and the inſtruction of thoſe Iriſh natives, that are moſt of them of the Romiſh religion. In purſuit of this deſign, I with much ado, procured a verſion of the New Teſtament, made, as I remember, in ; and finding it to have been many years out of print, the copies having, as I have been informed, been bought up, from time to time, by ſome Romiſh eccleſiaſtics; I cauſed a font of Iriſh letters to be caſt, and the book to be here reprinted; of which I ſent over ſome hundreds, ready bound, to be diſtributed gratis among thoſe to whom they ſhould upon the place be judged the moſt likely to do good. Theſe things I relate, that you may not think it ſtrange, that I have not ſtill by me any number of New Teſtaments in Iriſh; though I ſhould not have ſo much diſfurniſhed myſelf of them, if I had been then ſo informed, as now I am, that this language is ſtill that of a great part of the North of *Scotland*. And my main deſign being, as I ſaid above, to acquaint the native Iriſh with the divine oracles; as ſoon as I had with the ſame characters cauſed an edition to be made of an Iriſh verſion of the Old Teſtament, which the excellent biſhop *Bedell* had long ago procured, and which, after the lately mentioned publication of the New Teſtament, divers of the reverend Proteſtant clergy in *Ireland*, beſides ſome worthy prelates, intended to promote, but by various occurrences were hindered from accompliſhing their good deſires; as ſoon I ſay, as the preſs had diſpatched this book, I ſent a competent number of theſe books into *Ireland*, to be diſtributed gratis by ſome pious and judicious perſons there. By all which particulars you may eaſily diſcern, that it was not, nor well could be, my principal deſign, to provide for the Highlanders and other natives in *Scotland*, that ſpeak *Iriſh*; and that it never was my purpoſe, that any of thoſe, that were printed at my charge, ſhould be ſet to ſale. But yet becauſe it were great pity, that the pious intentions and endeavours of your reverend miniſters ſhould be altogether diſappointed, I ſhall acquaint you with what comes into my mind about it. Before I well knew the great extent of the Iriſh language in *Scotland*, I had cauſed ſeveral Old Teſtaments to be bound for the uſe of your nation, divers of which remain yet in my hands. So that as to the one and thirty you mention, I can ſpare each of them a book, which I think you very diſcreetly adviſe, ſhould not be diſpoſed of by the miniſter at his death, or removed, but be kept for the uſe of the pariſh. And theſe will be accompanied with one with a gilt back, to be preſented to the right reverend biſhop of *Roſs*; which I hope his lordſhip will accept, as an acknowledgment of my juſt ſenſe of his pious forwardneſs to promote the good work I aimed at. I ſhall alſo ſend an hundred more unbound, ſince you deſire to have them ſo, which, with the other thirty-two, ought to be diſpoſed of gratis to ſuch pariſhes as my lord biſhop, who I perceive conſults with his preſbytery, ſhall think fit. With Iriſh New Teſtaments I formerly told you, that I was not furniſhed. And I did not think fit to reprint them with the Old, not only becauſe it was judged needleſs, but becauſe it was ſcarce poſſible to bind them together, without making the volume much too bulky and unwieldy. But having, upon the late peruſal of your letters, cauſed enquiry to be made, I am informed, that a bookſeller, having with my leave printed a ſomewhat greater number of New Teſtaments than I had contracted for, he has yet in his hands about fifty Old Teſtaments, and about one hundred and fifty New ones. He can afford each whole Bible for a mark, provided the whole fifty be taken off his hands; and for the ſupernumerary hundred, or thereabouts, of New Teſtaments, he can part with them at three ſhillings a piece in quires.

IX.

Preſbytery of Roſs *to Mr.* Kirkwood.

Reverend brother, Canonry of *Roſs*, Oct. 15, 1688.

WE the preſbyters of the dioceſe of *Roſs* being this day ſynodically convened, our right reverend biſhop cauſed read publickly a letter from you to his lordſhip, dated June laſt; wherein, to our great ſatisfaction, we heard an account of the pious endeavours of the honourable and excellent Mr. *Boyle*, and were ſurpriſed with the bountiful offer you were pleaſed in his name to make, of a Bible and conſiderable number of catechiſms tranſlated into the Iriſh language, for every pariſh within this dioceſe: for which we can return nothing more ſuitable, than our moſt hearty thanks to the excellent and famous Mr. *Boyle*, and our humble prayer, that God may reward himſelf, and bleſs all thoſe his good deſigns for the benefit of mankind, and withal to return you alſo our thanks for the repreſenting to him our need and deſires for theſe books. We ſhall uſe our utmoſt endeavours to proſecute all his religious deſigns, by following thoſe methods which he by you recommends, as far as the circumſtances of our people can allow; for ſo our reverend biſhop has, with a great deal of affection and reſpect to that worthy perſon, very earneſtly laid upon us. We are alſo uſing all diligence to get that noble preſent of books brought among us; but the diſtance of the place, the unuſual ſtormineſs of the weather, and the confuſions that we are all put into by the march of our pariſhioners, who are commanded to repair unto the King's hoſt under the ſevere penalties of the law, makes us hopeleſs to get them brought to this place for ſome time, till it pleaſes God to calm things, as well as the weather. But the zeal we have to enjoy the benefit of them, will not ſuffer us to be negligent about them. In the mean time, we ſhall be moſt ready to do all ſervices that are in our power to the generous Mr. *Boyle*, and to you, to whom alſo we think ourſelves much obliged, and ſhall never ceaſe to remember you both in our prayers; for we are, and ſubſcribe ourſelves by the clerk of the meeting,

Reverend brother,

Your affectionate brethren to ſerve you,

James Roſs, Cl. Synod.

Mr.

X.

Mr. Kirkwood *to Mr.* Boyle.

SIR, July 13, 1689.

I SHOULD have waited upon you before I went out of town, but I have been obliged to be with a perſon who had the ſmall-pox, and is now dead of them; which makes me that I do not come to wait upon you myſelf, knowing that you have never had that diſeaſe. As for what you mentioned to me, when I was laſt with you, about giving ſome more bibles to the Highlanders, I have conſidered of it with Mr. *Kirk*, and other diſcreet perſons, who do not think it neceſſary at this time, ſince each pariſh hath a Bible already ſent to them, or deſigned for them; and as for others, this impreſſion I hope will be ſufficient for them, for ſome time. We cannot but be more and more ſenſible of your generous charity, for which God will reward you. As for the New Teſtaments, which are not yet diſtributed, if you pleaſe to beſtow of them upon the pariſhes which are largeſt, and have greateſt need of them, it will be a very great and excellent preſent to thoſe poor people. Mr. *Everingham* ſays, he thinks he has but about thirty New Teſtaments belonging to you. If you think fit to beſtow them, they may be ſent with the bibles, which are now a printing, when Mr. *Kirk* returns to *Scotland*. As for the binding of them, we ſhall take care that it be done, without putting you to any more charge, which hath already been very extraordinary. Mr. *Everingham* tells us, that he is obliged to make uſe of two of your Iriſh bibles, which are neceſſary in correcting; which I know you will be very well ſatisfied with. I heartily wiſh you health and all ſort of happineſs. I hope God will, in his good time, reſcue *Ireland* from the hands of wicked men. I am,

SIR,

Your moſt affectionate

and moſt humble ſervant,

James Kirkwood.

XI.

Mr. Spalding *to Mr.* Boyle.

SIR, *Edinburgh*, June 28, 1690.

I AM appointed by the general meeting of the miniſters and elders of this church, as to ſhew the ſenſe they have of your great pains in cauſing emit the Bible in the Iriſh language, ſo to deſire, that theſe two hundred Iriſh bibles already at *Edinburgh*, may be at the diſpoſal of the next general aſſembly of this church, to be held on the third Thurſday of October next; and that they may not be diſperſed, until the ſaid general aſſembly order the viewing of the tranſlation, and their being made uſe of publickly in congregations. This is deſired of you, in the name and by the appointment of the ſaid general meeting, by,

SIR,

Your honour's very humble ſervant, and their clerk,

Jo. Spalding.

DIRECT your return, *To* Thomas Edgar, *chirurgeon, at his ſhop, over-againſt the Croſs, on the north ſide of the ſtreet,* Edinburgh; *for Mr.* John Spalding, *miniſter at* Kirkcudbright.

XII.

Mr. Boyle *to Mr.* Kirkwood.

SIR, *London*, October 18, 1690.

I CONFESS you have exerciſed a great deal of charity (which yet I ſhall not be ſurpriſed to find in you) if you have not cenſured me in your thoughts, for my not having ſent you all this while an acknowledgment of your obliging letter, and the valuable preſent that accompanied it. But though my ſiſter and I were juſtly ſatisfied with the good counſel and reflections contained in your pious and ſenſible book; yet, when I ſhould have returned you my thanks for it, in an anſwer to your letter, I found the care I intended to take of it had not kept it from miſcarrying, as, after a fruitleſs ſearch, I preſumed it had done. But on the Lord's day, turning over ſome papers, for which I had a peculiar regard, I happily lighted on that which I had ſo long miſſed. I need make no return to what you are pleaſed to ſay to my advantage and your own diminution, ſave that I look upon the former as a compliment, and the latter as a dictate of modeſty. But I am altogether of your mind, of what you write about the education of youth; for I think the rectifying of that to be a thing ſo important, that, till it pleaſe God to awaken men to a greater ſenſe than they yet have of the neceſſity and uſefulneſs of that, I ſhall ſcarce expect any ſuch reformation as I wiſh, either of men's principles or their manners. And I think it as much the intereſt, as duty, of thoſe whom it concerns, to liſten to the good advices you give them in your ſerious diſcourſe. Not long after I received the favour of your letter, there came to my hands, out of *Scotland*, a ſhort one (of a long date) from an unknown writer; to whom, becauſe he was ſo, I did not think fit to return an anſwer, till I had conſulted you about the contents. And during the time I could not find your letter; the other that I thought I had carefully put put up, was unhappily miſlaid too. For which reaſon, I muſt beg you to aſſiſt me to apologize for the delay occaſioned by that unlucky accident. But now, having recovered both your's and your countryman's, I muſt deſire your thoughts, what will be fit for me to do upon the latter, whereof a copy is incloſed to you, by,

Sir, your's, &c.

Robert Boyle.

XIII.

Mr. Kirkwood *to Mr.* Boyle.

SIR, *Aſtwick*, Oct. 30, 1690.

YESTERNIGHT I received your very kind and obliging letter, for which I return you my moſt humble thanks. I am glad, that the little plain treatiſe I made bold to ſend you, has met with your approbation. This is the effect of your goodneſs and charity, and of your zeal for any thing, that ſeems to tend to the honour of our great maſter, who accepts of poor and weak endeavours, if they are ſincere.

As to the affair of the bibles, I have not been unmindful of it. I have writ to ſeveral perſons to do what they can to render the deſign effectual; particularly I have writ to one of the miniſters of *Edinburgh*, Mr. *David Blair*, who correſponds with me at preſent about promoting that good work. In my letters to him (the laſt whereof was writ about a month ago) I gave him an account of the bibles and catechiſms you ſent to *Edinburgh*; I ſhewed him how your great charity, in cauſing ſo many bibles to be printed, and your beſtowing 200 of them upon the *Highlands* of *Scotland*, has encouraged ſeveral pious and charitable perſons, chiefly of this kingdom, to give very liberally towards printing 3000 more bibles and 1000 teſtaments, whereof the teſtaments are already bound, as alſo about 2000 of the bibles. I acquainted him likewiſe, that you have been at the charge of printing 3000 catechiſms, beſides the firſt 3000. I deſired him therefore, earneſtly to recommend this buſineſs to the general aſſembly, that they might think of the way, how to tranſport into *Scotland* the bibles, teſtaments and catechiſms, which are now ready at *London*; and to conſider how to diſtribute into the *Highlands* both the books you have given, and ſo many of the reſt of the books, as at preſent are neceſſary to be diſtributed amongſt the *Highlanders*. The great thing, which I deſired him to recommend to the general aſſembly, was, the ſettling of ſchools in every pariſh of the *Highlands*, that the children may be taught to read, and to make uſe of the catechiſms and bibles; by which means they may prove happy means of propagating the knowledge of God in the families to which they belong. Thoſe things I have likewiſe recommended to ſome other miniſters, who, I hope, have been careful to act their part faithfully in promoting this deſign in their general aſſembly. It is likely the miniſters will deſire the concurrence of the parliament in a matter of ſo great importance, which tends ſo much to the benefit both of church and ſtate. I wrote lately to Dr. *Horneck* to propoſe it, by ſome fit perſon, to the King, that he would recommend the encouragement of this deſign to the parliament of *Scotland*, that they may promote it ſo far as they can at this time. I have heard ſince from the doctor, who approves the thing. There ſeems a great deal of reaſon, that the King ſhould appear in a matter, which is ſo much for the honour of the great King. And though the preſent confuſions in the *Highlands* do not permit to do all that we wiſh, yet ſomewhat may be done and projected, till there be an opportunity to do more.

I THOUGHT it would not be improper, in anſwer to ſome part of your letter, to give you this general account of the affair.

You are pleaſed to deſire my thoughts what to do as to Mr. *Spalding*'s letter. It is your humility to propoſe this. Though I am not fit to give advice to a perſon of your wiſdom, yet, in obedience to your commands, as a well-wiſher to the deſign, I ſhall give you my thoughts. If you yourſelf judge it convenient to write, it ſeems not amiſs, in my humble opinion, to ſignify to Mr. *Spalding* your being deſirous, that they, whom he mentions in his letter, would think of fit ways to diſtribute, and to render uſeful the bibles and catechiſms you have beſtowed for the uſe of the *Highlanders*; and that you and a great many more would be very glad to hear of their zealous endeavours to reſcue ſo great a part of the nation from that ſad ſtate of ignorance and barbarity, in which you hear they are. This, when ſuggeſted by you, may prove no ſmall ſpur to them. I know the principle of divine charity, which moved you at firſt to attempt the impreſſion of the Bible in Iriſh, will move you to do any thing you think will tend to render what you have done ſucceſsful and effectual.

WHEN Mr. *Kirk* went to *Scotland*, I wrote to the perſon, with whom your bibles, *&c.* are depoſited, not to give any more of them to any body, but to leave it wholly to the enſuing general aſſembly, to order the diſpoſal of them, according to your intention and deſign; of which I wrote fully to Mr. *Blair*, from whom I expect to hear very ſhortly what the general aſſembly has done in this important affair. I have nothing elſe to add, but my hearty prayers for your ſafety and preſervation. I am,

SIR,

Your moſt obliged and moſt affectionate humble ſervant,

JAMES KIRKWOOD.

XIV.

Biſhop of Roſs *to Mr.* Kirkwood.

Reverend Sir,

I HAD the favour of your diſcreet and obliging letter, dated June the 16th, upon the 24th of Auguſt. You muſt not think it was the fault of your correſpondent, that it came not ſooner to my hand, but the diſtance of the place from *Edinburgh*, and the uncertainty of a ſure bearer. However I thought myſelf extremely obliged by your letter; not only becauſe it gave me an account of the labours of that honourable and excellent perſon, Mr. *Boyle*, but becauſe it gave me alſo an opportunity of a new converſation (at leaſt by letters) with ſo worthy a perſon as yourſelf. I have cauſed tranſcribe your letter, and am to ſend a double thereof to every biſhop any ways concerned with people in his dioceſe that ſpeak Iriſh; judging, that it would better ſpeak forth the learned Mr. *Boyle*'s mind and your's, than any thing I could ſay; and if I cannot get theſe books brought hither before the meeting of my ſynod, I ſhall with them adviſe of the beſt way of doing it. I ſhall alſo recommend to my brethren, and do what is in my power, to proſecute the religious deſign of the author of ſo great a favour as you narrated; though I am much of your mind, that not a few even of the preſbyters muſt begin at the alphabet, that they may but read the Iriſh language in its proper character, there being ſo few books in that language and character paſſing in all this country; and we are removed at ſo great a diſtance from *Ireland* and the *Weſt-Iſles*, that it will oblige the preſbyters to paraphraſe many words in the Iriſh Bible before they be underſtood fully by their people: ſo that I cannot condemn the deſigns of ſome, to have that language quite worn out of this country; and if with it we could alſo put away the barbarity of the manners of ſome of our

Highlanders, I ſhould think it a very good work. I took care, a conſiderable while ago, to cauſe ſome of the preſbyters, who were beſt ſkilled in that language, to tranſlate into Iriſh a catechiſm, which is appointed to be made uſe of in all the dioceſe either in Engliſh or Iriſh, according to the capacities of the perſons to be catechiſed, as it is alſo in ſome other dioceſes; and in my opinion, it ſerves the ends of catechiſing better than that of the aſſembly. I ſend you herewith a double of it in the Engliſh, that at your conveniency I may have your opinion about it, and, if it be poſſible, the judicious ſentiments alſo of the pious Mr. *Boyle*. Care alſo was taken, that there be ſchools for teaching the youth to read in every pariſh; but I confeſs hitherto it has only been in the Engliſh language: and no leſs care is taken, that every miniſter read a portion of holy ſcripture; and in places, where moſt of the people underſtand only the Iriſh, he is at the pains to tranſlate it into that language, for which this noble preſent of the Iriſh Bible will be very uſeful to them, becauſe I have heard ſome of them complain, that they could not get words to expreſs in Iriſh ſome paſſages of holy writ. I cannot but with you regret, that ſo few intend ſo good and catholic a deſign as the worthy Mr. *Boyle* does; but this adds the greater luſtre to his fame. We have many excellent treatiſes written of late by the worthy guides and paſtors of the church of *England*, and ſome alſo by learned and pious laics; all which are very uſeful for aſſerting and improving our holy religion, for which we are extremely obliged to them: but if in this poor church and nation we make fewer public appearances, you are better acquainted with the reaſons of it, than that you need be told them by me. I ſhould think all was well enough with us, if we could truly uſe the antient motto, *non magna loquimur, ſed vivimus*; but that I do not weary you in my firſt letters, I ſhall only ſay, that I have ſeen moſt of the judicious writings of the learned Mr. *Boyle*; and they have raiſed in me, as I think they muſt do in all others, an high but juſt eſteem of him; and for this laſt bounty, by which he has obliged all my charge and me, I know his pious generoſity expects no other reward but our prayers for him, wherein ſhould we be wanting, we were much to blame before God and man. Nor can I cloſe before I return you my own hearty thanks, as well as thoſe of ſuch in theſe bounds, as I have ſpoken with, for your kindneſs in this affair to us all, and in your letter to myſelf. When my brethren meet with me in the ſynod, it is like they will expreſs their own ſenſe of it. Wherefore, intreating, that you may upon occaſion let me know of the health and other good works of Mr. *Boyle*, and what way I can beſt ſerve him or you, I heartily commend you to the grace of God, and beg your prayers for,

Reverend Sir,

Yours, &c.

J. Rossen.

NUM-

NUMBER V.

Letters from Mr. *John Eliot* of *New-England* to Mr. *Boyle*, relating to the ſervices of the latter for the propagation of the Goſpel in *America*. *Life*, p. cxxxix.

I.

Right honourable, *Roxburg*, Sept. 30, 1670.

YOUR conſtant care of, and ſtedfaſt affection unto this Indian work (which the Lord hath, in great undeſerved mercy to me, put under my hand, a weak and unworthy inſtrument therein) do greatly oblige my heart to honour you, and pray, that it may be remembered by the Lord in that great day, when he will ſay [*come ye bleſſed*] unto all the ſincere benefactors unto his people. You have alſo added no ſmall encouragement unto me, in that worthy gift, which your honour is pleaſed to beſtow upon me, *viz. Pool's Synopſis*, or *critica ſacra* upon the whole bible, which, though it be not yet come, is under the care and faithful hand of my worthy and true friend Mr. *Aſhurſt*; for which deſirable gift, I return unto your honour my humble thanks. Touching the preſent ſtate of this work with the Indians, I have written to our worſhipful commiſſioners, who will ſend it unto your honour, governor of the honourable corporation; and therefore I ſhall keep ſilence of that matter here. And whereas your honour will ſee, that I have undertaken and begun a kind of academical reading unto them, in their own language, thereby to teach the teachers and rulers, and all that are deſirous of learning, I find, by experience, that it will be very neceſſary to have ſome entertainment of food, for all the principal men at leaſt, which do come; for many are to come a great way, and had we but food to entertain them, when they come there, it would be ſome encouragement. And I have ſome thoughts, if God give life and means, to read medicine, and call for ſuch roots (for they altogether uſe the root, and not the herb) as they have experience of; eſpecially had I wherewith to recompenſe any, that bring in a deſirable experiment. There hath been a rare work of God this ſummer in a great pond at *Watertown*, where all the fiſh died, and were not willing to die in the waters, but as many as could thruſt themſelves on ſhore, and there died; not leſs than twenty cart load, by eſtimation, lying dead, all at once, round about the pond. An eel was found alive in the ſandy border of the pond, and being caſt into the water, ſhe wriggled out again, as faſt as ſhe could, and died on the ſhore. An inhabitant of the town, living by the pond, his cattle uſe daily to drink there; but then, for three days together, they refuſed there to drink; but after three days they drank of the pond, as they were wont to do. When the fiſh began to come aſhore, before they died, many were taken and eaten, both by Engliſh and Indians, without any hurt; and the fiſh were very good. Now the diſeaſe of the ſtone groweth frequent among the Engliſh, and beginneth among the Indians; which ſtirreth me to ſearch, and I clearly find, that a crude ſtomach provides the matter, and cold in and about the bladder and ureters is the efficient of the ſtone, eſpecially in thoſe whom I have converſed with, as may be demonſtrated. But I am over-bold to preſume to meddle ſo far unto your honour. I therefore ſhall ceaſe to give you any further trouble at preſent. So committing you to the Lord, and to the word of his grace, I remain

Your honour's to ſerve you in the ſervice of the Lord Jeſus,

John Eliot.

II.

Right honourable nurſing fathers, *Roxburg*, Oct. 23, 1677.

THE poor praying Indians do thankfully acknowledge, that (under God our heavenly father, and under Jeſus Chriſt our redeemer, who redeemeth us out of all our troubles) you have been the means and inſtruments in his hand, to ſave and deliver us. God moved your hearts to own us, in that black day, when all were againſt us, and we were almoſt ready to be ſwallowed up in deſtruction; which dark time we ought not to forget, nor your owning kindneſs unto us in that dark day.

AND ſince that, your charity hath greatly revived and refreſhed us. Many of our aged, decrepid, fatherleſs, and widows, ſtill wear the garments, not yet worn out, which your charity did, the laſt winter, clothe us withal. And although we yet know not what our honoured commiſſioners will do for us, whoſe favour we doubt not of; yet underſtanding, that ſome doubt is raiſed about your countenancing and encouraging our rulers, who are of us, and live among us, and without whoſe preſence and aſſiſtance, the Lord's work of ſoul-inſtruction and edification will ſoon faint, ſink, and come to nothing; our humble petition is, firſt to God, that he, who hath hitherunto, would ſtill move your hearts for our good and welfare; and next, our petition is unto yourſelves, that we may have the countenance of your favour, to countenance and own our rulers among us, without whoſe countenance our teachers will be of little power, eſpecially among our youth and riſing generations, who do not yet ſavour the things of the Goſpel of Jeſus Chriſt; and among ſtrangers, who have not yet taſted how good the Lord is, though for their protection and ſafety they have crowded in upon us.

NOBLE-hearted Sir, your gift I do ſtill religiouſly keep, for ſome ſpecial and eminent ſervice of the Lord, in the Lord's time. In our firſt war with the Indians, God pleaſed to ſhew us the vanity of our military ſkill, in managing our arms, after the European mode. Now we are glad to learn the ſkulking way of war. And what God's end is, in teaching us ſuch a way of diſcipline, I know not. By our late eaſtern war it hath pleaſed God to ſhew us our weakneſs by ſea, as formerly by land. The Indians took many of our fiſhing veſſels, and the men that belonged to them, and forced them to ſail whither they deſired: many of the men delivered themſelves and their veſſels; many Indians were ſlain, ſome Engliſh. The hiſtory of theſe actions I have not: others do attend that ſervice, to whom I leave it. The governor of *New-York* ſent a ſtrength this ſummer, and took poſſeſſion of a northern port, where they fixed and fortified themſelves: ſince whoſe coming thither, the Indians have not ſtirred much. Little action hath paſſed, but I hear not of any peace made. The Yorkers have taken in hand a chargeable deſign: what profit will come of it, I know not; time will diſcover that: whether their intention be to promote religion, or only trading, I know not. It pleaſed the Lord, very lately to permit a ſmall handful (not twenty) of the late ſcattered rod to make a ſore direption upon *Hatfield* and *Dearfield*, at *Connecticut*; where about twelve perſons were killed, more than twenty carried away captive, or loſt; ſeven dwellings burned, and ſundry barns full of corn; and ſince they have appeared at *Hadly*, burned the mill. They had parly with them, treated about reſtoring the captives, agreed of a time and place of meeting; but the Indians failed to appear. Theſe laſt actions have very much diſcouraged our people from repairing the deſtroyed towns, which ſome were beginning to do. We had a Sachem of the greateſt blood in the country, ſubmitted to pray to God, a little before the wars: his name is *Wanalaunſet:* in the time of the wars he fled, by reaſon of the wicked actings of ſome Engliſh youth, who cauſeleſsly and baſely killed and wounded ſome of them. He was perſuaded to come in again. But the Engliſh having ploughed and ſown with rye all their lands, they had but little corn to ſubſiſt by. A party of French Indians (of whom ſome were of the kindred of this Sachem's wife) very lately fell upon this people, being but few and unarmed, and partly by perſuaſion, partly per force, carried them all away. One with his wife, child, and kinſwoman, who were of our praying Indians, made their eſcape, came into the Engliſh, and diſcovered what was done. Theſe things keep ſome in a continual diſguſt and jealouſy of all the Indians. I ſhall give your honour no farther trouble at preſent. We intreat your prayers, and commit you to the Lord, and reſt

Your honour's to ſerve you in the Lord Jeſus,

John Eliot.

III.

Roxburg, Nov. 4, 1680.

Right honourable, charitable, indefatigable, nurſing father,

WHEN good works of pure charity are ſown three hundred fold thick, and that by a living hand, Lord what a reaping time or harveſt will there be! Sir, you are eminently mindful of that Goſpel charge, 1 *Tim.* vi. 17, 18, 19. *Charge them, that be rich in this world, that they be not high-minded, nor truſt in uncertain riches, but in the living God, who giveth us richly all things to enjoy.* That they do good, that they be rich in good works, ready to diſtribute, willing to communicate, laying up in ſtore for themſelves a good foundation againſt the time to come; a foundation not of grace unto juſtification by way of merit, but a foundation of degrees of glorification, when God will in free mercy diſtribute his gifts of glory, according to our improvements of our talents in the exerciſe of grace: he that gained ten talents, ſhall have ten cities.

I KNOW it will pleaſe your charitable heart to hear how it fareth with thoſe that are your alumni. We are in great affliction by the Manquaoy Indians: more than 60 at ſeveral times have been killed or captived; a narrative whereof major *Gookins* preſented to lord *Culpepper*, who was affected with it. Alſo he preſented a copy thereof to Sir *Edmond Andros*, who was likewiſe affected with it, though it is ſaid, that he might have prevented it. We hope he will move in it, and our Mr. *Pinchon* is gone up to join with Sir *Edmond* to endeavour a peace. Major *Gookins* intendeth to preſent your honour with a copy of the ſame narrative. The Eaſtern Indians do offer to renew peace with us, and to ſubmit themſelves to be taught to pray unto God. A chief Sachem was here about it, a man of a grave and a diſcreet countenance. Our praying Indians, both in the iſlands, and on the main, are (conſidered together) numerous; thouſands of ſouls, of whom ſome true believers, ſome learners, and ſome are ſtill infant; and all of them beg, cry, intreat for bibles, having already enjoyed that bleſſing, but now are in great want. Your honour's liberality in Engliſh bibles is a great favour, which we all with thankfulneſs receive; but the bible in their own tongue muſt help them to underſtand it. We are at the 19th chapter of the Acts; and when we have impreſſed the New Teſtament, our commiſſioners approve of my preparing and impreſſing alſo the old. Your honour's bounty of 30 *l.* towards our ſending the Goſpel to thoſe remote Indians that ſpeak the language, whereunto the bible is tranſlated, I do religiouſly keep it, to be improved to the ſame end, to which your honour gave it, of which ſervice I am ſtill in hope, having more intelligence that there is ſuch a people.

But by the immaturity of ſome occurrences, and the intentions of the ſhip's ſpeedy ſailing, I cannot give your honour any further diverſion at this time: entreating your prayers, I commit you to God, and reſt,

Your honour's to ſerve you in any ſervice of Jeſus Chriſt,

John Eliot.

IV.

Roxburg, March 15, 1682-3.

Right honourable, charitable, nurſing father,

THIS winter the worſhipful Mr. *Stoughton* (commiſſioner) delivered to Major *Gookins* (a pillar in our Indian work) and to me, the ſum of 6 *l.* as the product of your honour's gift of charity; which we did diligently diſtribute to Chriſtian Indians, two aged blind women, others lame in their limbs, others decrepid with age; all which do bleſs you the giver, and do praiſe God the fountain; and we, your diſpenſers of ſo great charity, do thankfully accept of ſo good an office, as to be the diſpoſers of ſo charitable gifts unto the poor ſervants of Jeſus Chriſt.

The Lord's work ſtill goeth on among them, and though many of the younger ſort, ſince the wars (where their ſouls received a wound) have declined, and too much miſcarried, yet now (through the grace of Chriſt) they are on the repenting and recovering hand; of which your honour may hear more, when the work is proſecuted, and brought unto a good effect.

The great work, that I travel about, is, the printing the Old Teſtament, that they may have the whole bible. They have had the whole in the firſt impreſſion, and ſome of the old they ſtill have, and know the worth and uſe of it; and therefore they are importunately deſirous of the whole. I deſire to ſee it done before I die, and I am ſo deep in years, that I cannot expect to live long: beſides, we have but one man (*viz.* the Indian printer) that is able to compoſe the ſheets, and correct the preſs, with underſtanding. For ſuch reaſons, ſo ſoon as I received the ſum of near 40 *l.* for the bible work, I preſently ſet the work on foot; and one tenth part or near is done: we are in *Leviticus.* I have added ſome part of my ſalary to keep up the work, and many more things I might add, as reaſons of my urgency in this matter. Touching thoſe remote Indians, to the North-Weſt, whoſe language agreeth with ours, ſo that they and we can ſpeak to each other's underſtanding, we have not, as yet, ſo full intelligence of them, as to make a report thereof. But I do both pray and wait for ſome information that way. And for the furtherance thereof, I do carefully reſerve your honour's gift of 30 *l.* to be improved in that ſervice, when the

Lord ſhall pleaſe to open a door thereunto. The Manquaoy Indians have not ſtirred to fall upon us this laſt year; but we are not yet fully ſettled in peace, becauſe they declare the Eaſtern Indians to be their enemies; and the way unto them is thorough us; and our Wameſet Indians, who are our moſt northerly plantation, are in danger to be their thoroughfair. And this putteth us into many fears; but our hope and help is in God, our eyes are unto him; this world is a place and ſtate, wherein God's people muſt expect nothing ſtedfaſt, all things mutable and afflicting. But I ſhall ceaſe to give your honour any farther trouble at preſent; therefore, commending you to the Lord, and to the word of his grace, I reſt,

Your honour's to ſerve you in Chriſt Jeſus,

John Eliot.

V.

Boſton, June 21, 1683.

Right honourable nurſing father,

YOUR hungry alumns do ſtill cry unto your honour for the milk of the word in the whole book of God, and for the bread of life, which they have fed upon in the whole bible, and are very thankful for what they have, and importunately deſirous to enjoy the whole book of God. It is the greateſt charity in the world to provide for their ſouls. Should your honour pleaſe but to change the object of your bountiful charity, from their bodies to their ſouls, here is enough already ſent over to accompliſh the work; they only ſtay for that word from your honour, *fiat*. My age makes me importunate. I ſhall depart joyfully, may I but leave the bible among them, for it is the word of life; and there be ſome godly ſouls among them, that live thereby. The work is under great incumberments and diſcouragements. My heart hath much ado to hold up my head; but doth daily drive me to Chriſt; and I tell the Lord, that it is his word, and your hearts are in his hand. I do therefore commit the whole to the Lord, and leave both it and myſelf to the Lord, who hath not left me wholly deſtitute. But I ſhall give your honour at preſent no farther trouble, for I am ſurpriſed with this opportunity of writing: therefore, committing your honour to the Lord, I reſt,

Your honour's to ſerve you in the Lord,

John Eliot.

VI.

Roxburg, Nov. 27, 1683.

Right honourable, right charitable, and indefatigable, nurſing father,

ALTHOUGH my haſty venturing to begin the impreſſion of the Old Teſtament before I had your honour's (*fiat*) may have moved (as ſome intimate) ſome diſguſt, yet I ſee that your love, bounty and charity, doth ſtill breathe out encouragement unto the work, by ſupplies of 460 *l.* unto the work, for which I do return my humble thankfulneſs to your honour, and take boldneſs to intreat favour for two requeſts.

First, I pray, that you would pleaſe to accept an apology for my haſte. I am deep in years, and ſundry ſay, if I do not procure it printed while I live, it is not within the proſpect of human reaſon, whether ever, or when, or how, it may be accompliſhed. It is Chriſt's work, and for the good of ſouls, which is my charge to attend, and run adventures to accompliſh, eſpecially when divine providence brought into my hand ſome ſmall encouragement to begin. But if this apology be ſhort (though capable of much enlargement) yet then,

My ſecond humble requeſt is, that you would pleaſe to draw a curtain of love over all my failures, becauſe love will cover a multitude of tranſgreſſions. The work goeth on now, with more comfort, though we have had many impediments, partly by ſickneſs of the workmen, for it is a very ſickly and mortal time with us, as alſo the rigour of the winter doth now obſtruct us. The work goeth on, I praiſe God; the ſabbath is ſanctified in many places, and they have ſtill fragments of their old bibles, which they make conſtant uſe of.

I desire to take boldneſs to propoſe a requeſt. A veſſel carried away a great number of our ſurpriſed Indians, in the times of our wars, to ſell them for ſlaves; but the nations, whither he went, would not buy them. Finally, he left them at *Tangier*; there they be, ſo many as live, or are born there. An Engliſhman, a maſon, came thence to *Boſton*, he told me, they deſired I would uſe ſome means for their return home. I know not what to do in it; but now it is in my heart to move your honour, ſo to mediate, that they may have leave to get home, either from thence hither, or from thence to *England*, and ſo to get home. If the Lord ſhall pleaſe to move your charitable heart herein, I ſhall be obliged in great thankfulneſs, and am perſuaded, that Chriſt will, at the great day, reckon it among your deeds of charity, done unto them, for his name's ſake. But I ſhall give your honour no farther trouble at preſent, I humbly requeſt your prayers for me. So, commending you to the Lord, and to the word of his grace, I reſt,

Your honour's to ſerve you in our Lord Jeſus,

John Eliot.

VII.

Roxburg, April 22, 1684.

Right honourable and indefatigable benefactors,

THIS laſt gift of 400*l.* for the reimpreſſion of the Indian Bible doth ſet a diadem of beauty upon all your former acts of pious charity, and commandeth us to return unto your honours all thankful acknowledgments, according to our abilities. It pleaſed the worſhipful Mr. *Stoughton*, to give me an intimation, that your honours deſired to know the particular preſent eſtate of the praying Indians; and alſo, when *Moſes*'s pentateuch is printed, to have ſome copies ſent over, to evidence the real and good progreſs of the work.

Your honours intimation hath the force of a command upon me, and therefore I ſhall briefly relate the religious walking and ways of the praying Indians. They do diligently obſerve and keep the ſabbath, in all the places of their public meetings to worſhip God. The example of the Engliſh churches, and the authority of the Engliſh laws, which Major *Gookins* doth declare unto them, together with ſuch mulcts as are inflicted upon tranſgreſſors; as alſo and eſpecially, the clear and expreſs command of God, which they and their children learn and rehearſe daily in their catechiſms; theſe all together have fully poſſeſſed and convinced them of their duty, to keep holy the ſabbath day. So that the ſanctifying of the ſabbath is a great and eminent part of their religion. And though ſome of the vain and carnal ſort among them are not ſo girt to it, as were to be deſired, yet the grave and religious ſort do conſtantly worſhip God, every ſabbath-day, both morning and evening, as the Engliſh do.

THE acts of worſhip, which they perform in their public meetings, are as followeth.

THE officer beginneth with prayer, and prayeth for all men, rulers, miniſters, people, young, old, ſick, well, Engliſh or Indians, *&c.* according to that word, 1 *Tim.* ii. 12. *I will that firſt of all prayers be made*, &c. I ſay, the officer beginneth with prayer, *viz.* where they have an officer ordained, as it is almoſt in all the churches. But we have more public aſſemblies, that meet every Lord's day, to worſhip God, than we have churches. There is not yet a church gathered in every place, where they meet to worſhip God and keep the ſabbath; but where it is ſo, they chuſe ſome able godly man (the beſt they can) to manage the worſhip among them: him they call their teacher, and he beginneth with prayer, *&c.* When prayer is ended, they call forth ſuch as are to anſwer the catechiſm; and though this is ſometimes omitted in ſome places, yet that is the way they walk in, and it is often practiſed. When catechiſm is ended, a chapter is read, ſometimes in the Old Teſtament, and ſometimes in the New; and ſundry of the young men are trained up, and called forth to this ſervice, ſometimes one, ſometimes another.

WHEN the chapter is read, a pſalm is ſung, which ſervice ſundry are able to manage well.

THAT finiſhed, the preacher firſt prayeth, then preacheth, and then prayeth again. If it be the day for the Lord's Supper to be celebrated, the church addreſs themſelves unto it, and the miniſter doth exactly perform it, according to the ſcriptures. When that ſervice is done, they ſing a pſalm, according to the pattern of Chriſt; then he bleſſeth the church, and ſo finiſheth the morning ſervice.

IN the afternoon they meet again, and perform all the parts of worſhip, as they did in the morning; which done, if there be any infant to be baptiſed, they perform that ſervice according to the ſcriptures; which done, the deacon calleth for contributions; which done, if there be any act of public diſcipline (as divers times there is, there being many failures among us) then the offender is called forth (being with care and diligence prepared) and is exhorted to give glory to God, and confeſs his ſin; which, being penitent, they gladly accept him, forgive him, and receive him. If it be not a ſatisfactory confeſſion, they ſhew him his defect, they admoniſh and exhort him to a more full confeſſion; and ſo he is left to ſome other time. This finiſhed, he bleſſeth the church, and ſo diſmiſſeth the aſſembly.

MOREOVER, major *Gookins* hath dedicated his eldeſt ſon, Mr. *Daniel Gookins*, unto this ſervice of Chriſt; he is a pious and learned young man, about thirty-three years old, hath been eight years a fellow of the college; he hath taught and trained up two claſſes of our young ſcholars, unto their commencement; he is a man whoſe abilities are above exception, though not above envy. His father, with his inclination, adviſed him to *Sherborn*, a ſmall village near *Natik*, whoſe meeting-houſe is about three miles, more or leſs, from *Natik* meeting-houſe. Mr. *Gookins* holdeth a lecture in *Natik* meeting-houſe once a month; which lecture many Engliſh, eſpecially of *Sherborn*, do frequent. He firſt preacheth in Engliſh, to the Engliſh audience, and

then

then the ſame matter is delivered to the Indians by an interpreter, whom, with much pains, Mr. *Gookins* hath fore-prepared. We apprehend, that this will (by God's bleſſing) be a means to enable the Indians to underſtand religion preached in the Engliſh tongue, and will much further Mr. *Gookins* in learning the Indian tongue. Likewiſe major *Gookins* holdeth and manageth his courts in the Engliſh tongue; which doth greatly further the Indians in learning law and government in the Engliſh tongue; which is a point of wiſdom, in civilizing them, that your honours have manifeſted your deſires that it might be attained.

The places where the Indians meet to worſhip God, and ſanctify the Sabbath, are many; the moſt are ſtated places, others are occaſional. The ſtated places, in the *Maſſachuſets*, ſince the wars, are contracted into four, *Natik*, *Poukipoy*, *Wameſut*, and *Chachaubunkkakowok*. The occaſional meetings are at places of fiſhing, hunting, gathering cheſnuts in their ſeaſons. Alſo ſince the wars, the Manquaoys, making incurſions upon the praying Indians, did cauſe to make divers forts, to live ſafely in, and then they did there meet to worſhip God, and keep the Sabbath.

In *Plymouth Patent* there are about ten places, where they meet to worſhip God.

An intelligent perſon, of *Martyn*'s vineyard, reckoned up unto me ten places, where God is worſhipped every Lord's day in that iſland.

At *Nantukket* there be about five places of prayer and keeping ſabbath.

The reaſon of this diſperſion of places of public meeting to worſhip God, is this; there is but here and there a ſpot of good land, fit for planting corn, with accommodation of fiſhing; theſe ſpots of good land lie at a great diſtance from each other; ſome four or five miles, ſome eight or nine miles, ſome ten or twelve miles: ſo that it is impoſſible for them, eſpecially with women and children, to meet at one place; therefore all that live together at one place, meet to worſhip God on the ſabbath day.

Thus I have briefly repreſented before you, right honourable, at your command, the preſent eſtate of the praying Indians, in reſpect of their religion. And what I have here expreſſed, for the ſubſtance of the things, I know them to be true, and I have often ſo practiſed among them.

By this it appeareth, that they are, in ſome good meaſure, able (by the light of the ſcriptures, and by the example of the churches of Chriſt, and by ſuch inſtruction as they have had) to practiſe and manage the whole inſtituted public worſhip of God among themſelves, without the preſence or inſpection of any Engliſh among them, which is no ſmall addition and advancement of the kingdom of Chriſt; and I doubt not but it ſhall add much comfort and joy to your ſouls here, and ſhall add much weight of glory to your ſouls hereafter, who have been ſo diligent, liberal and conſtant in your ſupplies for the encouragement of this work of Chriſt.

And it is no ſmall comfort to me, whom divine providence and grace hath made one of the poor inſtruments, to inſtruct and manage them unto this eſtate in Chriſt Jeſus, whereunto they have attained.

As for the ſending any numbers of *Moſes*'s Pentateuch, I beſeech your honours to ſpare us in that; becauſe ſo many as we ſend, ſo many Bibles are maimed and made incomplete, becauſe they want the five books of *Moſes*. We preſent your honours with one book, ſo far as we have gone in the work, and humbly beſeech, that it may be acceptable, until the whole be finiſhed; and then the whole impreſſion (which is two thouſand) is at your honours command. Our ſlow progreſs needeth an apology. We have been much hindered by the ſickneſs this year. Our workmen have been all ſick, and we have but few hands, one Engliſhman, and a boy, and one Indian; and many interruptions and diverſions do befal us; and we could do but little this very hard winter. But I ſhall give your honours no further trouble at this time; only requeſting the continuance of your prayers and protection. So I remain,

Your honours

to ſerve you in our Lord Jeſus,

John Eliot.

POSTSCRIPT.

THE people of *Natik* have procured ſome friend of *Sherborn*, to draw up a letter to me, which I make bold to preſent to your honours view, being here incloſed. If I have been over-bold herein, I beſeech your honours to pardon me.

IX.

Roxburg, Auguſt 29, 1686, in the third month of our overthrow.

Right honourable unweariable nurſing father,

I HAVE nothing new to write but lamentations, and I am loth to grieve your loving and noble ſoul.

OUR Indian work yet liveth, praiſed be God; the Bible is come forth, many hundreds bound up, and diſperſed to the Indians, whoſe thankfulneſs I intimate and teſtify to your honour. *The Practice of Piety* is alſo finiſhed, and beginneth to be bound up. And my humble requeſt to your honour is, that we may again reimpoſe the Primer and Catechiſm; for though the laſt impreſſion be not quite ſpent, yet quickly they will; and I am old, ready to be gone, and deſire to leave as many books as I can. I know not what to add in this diſtreſſing day of our overthrow; ſo I commit your honour to the Lord, and reſt,

Your honour's

to ſerve you in Jeſus Chriſt,

John Eliot.

X.

Roxburg, July 7, 1688.

Right honourable, deep learned, abundantly charitable, and conſtant nurſing father,

SIR,

I AM drawing home, and am glad of an opportunity to take my leave of your honour with all thankfulneſs. Sir, many years ſince you were pleaſed to commit 30*l.* into my hand, upon a deſign for the promoting Chriſt his kingdom among the Indians; which gift of yours I have religiouſly kept, waiting for an opportunity ſo to improve it; but God hath not pleaſed yet to open ſuch a door. I am old, and deſire to finiſh that matter, and take the boldneſs to requeſt your honour, that it may be thus diſpoſed of. It being in the hand of major *Gookins*'s relict widow, and he died poor, though full of good works, and greatly beneficent to the Indians, and bewailed by them to this day; therefore let his widow have 10*l.* his eldeſt ſon, who holds up a lecture among the Indians and Engliſh 10*l.* and the third 10*l.* give it to Mr. *John Cotton*, who helped me much in the ſecond edition of the Bible. And alſo I muſt commit to him the care and labour of the reviſal of two other ſmall treatiſes, *viz.* Mr. *Shegheard*'s *Sincere Convert* and *Sound Believer*, which I tranſlated into the Indian language many years ſince; and now I hope, that the honourable corporation will be at the charge to print them, by your honour's favour and countenance. But I cannot commit them to the preſs without a careful reviſal, which none but Mr. *Cotton* is able to help me to perform.

The work in general ſeemeth to my ſoul to be in and well toward a reviving. Many churches of confeſſors of Chriſt are in motions to gather into church eſtates, who do carefully keep the ſabbath. And out of theſe profeſſors of religion, we do gather up and call in ſuch as are willing to confeſs Jeſus Chriſt, and ſeek ſalvation by him. Touching other matters, what our loſſes and changes be, and how trading, *&c.* are ſpoiled, I am ſilent; but my prayer to God is, *Iſaiah* i. 25, 26. *And I will turn my hand upon thee, and purely purge away the droſs, and take away all thy tin, and I will reſtore thy judges as at the firſt, and thy counſellors as at the beginning*, &c. So do, O Lord.

Sir, the Lord prolong your days, and fill you with all grace, until you arrive at the fulneſs of glory, where I leave you, and reſt,

Your honour's

to ſerve you in Jeſus Chriſt,

John Eliot.

NUMBER VI.

Letters of thanks from the Governor, *&c.* of *New England*, to Mr. *Boyle*, for his ſervices to that Colony. *Life*, p. cxxxix.

I.

Governor and General Court of New-England *to Mr.* Boyle.

Honourable Sir,

THE occaſion of our giving you this trouble, is from the confidence we have of your favour and care of theſe his Majeſty's colonies in *New England*, manifeſted by your continued endeavours, as in promoting that good work of the natives converſion, ſo in taking opportunities for ingratiating us with his Majeſty and the right honourable the Lord Chancellor, as we underſtand by your letter to Mr. *Winthrop*, whereby you have given us that comfortable information of his Majeſty's grace towards us, in expreſſing himſelf in a very favourable manner, and that the

Lord Chancellor did aſſure you, (with giving you commiſſion to aſſure our friends in the city) that the King intends not any injury to our charter, or the diſſolution of our civil government, or the infringement of our liberty of conſcience, and that the doing of thoſe things is not the buſineſs of the commiſſioners; the truth whereof we believe (as we ought) having the word of ſo gracious a King. But alas! Sir, the commiſſion impowering thoſe commiſſioners to hear and determine all cauſes, whether military, civil, or criminal, (what they have further by inſtruction at preſent we know not) ſhould this take place, what will become of our civil government, which hath been (under God) the hedge to that liberty for our conſciences, for which the firſt adventurers paſſed through, and bore up againſt all difficulties and diſcouragements that encountered them, as in the way to, ſo in the continuance in this wilderneſs? Sir, we return unto you our true and hearty thanks for your former favours, and crave the continuance thereof as opportunity ſhall offer, and the great mover of hearts ſhall incline you in appearing our friend ſtill, that, if poſſible, the commiſſion may be recalled; for which end we have made our humble ſupplication to his Majeſty, in whoſe eyes, if we may find favour, we and our poſterity ſhall have cauſe to bleſs the Lord. But if the decree be paſt, ſo that it may not be recalled, we ſhall wait the Lord's iſſue with us; and whatever may be the conjectures of any rendering alterations here adviſable, the iſſue will ſpeak them to be the ſubverſion of all that, which makes this place or our abode herein deſirable; or if any of thoſe that deſire a dominion over us, (not to ſerve his Majeſty's intereſt in advancing plantation work, with the countenance of godlineſs, but to ſerve themſelves by his Majeſty's authority, and our ruin) ſhall prevail, it will to poſterity be rendered a diſſervice to his Majeſty's honour, and ſuch a damage, as the procurers will not be able to repair. We can ſooner leave our place, and all our preſent outward enjoyments, than leave that, which was the firſt ground of our wandering from our native country; nor are we thereby made ſuch ſtrangers thereunto, but we can rather chuſe to return, and take our lot with our brethren, than abide here under the deprivement of the ends of our travels. Our way is with the Lord. Craving your honour's pardon for this boldneſs, lifting up our beſt deſires to the Lord for you, we remain,

SIR,

Your humble ſervants,

in the name and by order of the General Court,
held at *Boſton* in *New-England*, Oct. 19, 1664.

Jo. Endecott, Governor.

II.

Mr. Boyle *to* Henry Aſhurſt, *Eſq;*

SIR,

I HAD ſooner ſent you an account of your letter, but that, although I delivered the petition to my Lord Chancellor, ſoon after I received it, and have waited on his Lordſhip ſeveral times ſince for an anſwer, yet I found him always engaged in buſineſs, either with the King, or Duke, or ſuch great perſons, as made me think it improper to deſire to be admitted. But being there this morning ſomewhat early, ſome of his relations, that ſaw me there, would needs bring me in, before I expected it, though a great perſon intervening obliged us to diſcourſe the moſt part aloud, till the Portugal reſident ſurprizing my Lord Chancellor with a requeſt to have audience, put an untimely end to our conference.

P. S. I forgot to tell you, that it is not all the New-Englanders, but only the Maſſachuſets, that have diſreſpectfully uſed the commiſſioners, to whom the reſt of the colonies have behaved themſelves otherwiſe. And when I ſpeak civilly to my Lord Chancellor of your friend Mr. *Winthrop*, his Lordſhip aſſented to the character I had given him.

III.

Governor and Deputy Governor, &c. *of* New-England *to Mr*. Boyle.

Honourable Sir,

AS an addition to your former kindneſs, touching the preſent of maſts ſent from this colony to his Majeſty, we are lately informed, that you have been pleaſed to ſpeak on our behalf in the ears of his moſt excellent Majeſty, our gracious Sovereign, when our adverſaries, by their hard ſpeeches and falſe ſuggeſtions, have laboured to alienate his royal heart and affections from us; which favour of yours (to a people that are ſo great ſtrangers to you, and ſo undeſerving your love) calls for gratitude. And therefore, ſhould we be ſilent in our moſt thankful acknowledgment thereof, firſt unto God, that hath ſo inclined your heart, and nextly, to yourſelf, as an inſtrument (and if we may preſume to ſay) an advocate for this part of God's poor church in the wilderneſs, it would render us moſt unworthy of our profeſſion.

Sir, we need not put you in mind, that the poor church of Chriſt, in all ages, even from righteous *Abel*'s time unto this day, hath not wanted adverſaries: the ancient enmity, which God hath put between the two ſeeds, will never reconcile; that example in *Ezra*'s and *Nehemiah*'s time do ſufficiently evince this; for although the people of God then had ample charters from thoſe great princes, *Cyrus* and *Artaxerxes*, yet God was pleaſed, for the trial of his church, and the illuſtration of his own glory (in their ſalvations) to permit a *Sanballat*, a *Tobiah*, and others, falſely to accuſe that people to thoſe princes of diſloyalty. Sir, we hear that our adverſaries there are plotting and deſigning againſt our peace; ſo much the more cauſe have we to lie in the duſt before the Lord, imploring his aſſiſtance and ſalvation, as the matter ſhall require. And alſo it is our duty not to neglect the uſe of that little means that is left us, in order to the preſervation of our quietneſs and liberties; among which, this application to yourſelf, and by you to our moſt gracious King (whoſe royal heart the Lord hath graciouſly inclined hitherto to favour our righteous cauſe) is the principal.

Sir, we hear of ſeveral things againſt us, which we do not particularly underſtand; but ſo far as is intimated to us, we will make bold here briefly to mention them, with our anſwers to them.

I. Our loyalty is queſtioned. To this we anſwer (in all humility, not boaſtingly) that the demonſtrations of our loyalty are known to thouſands; particularly, 1. We never proclaimed or acted in the name of the late power in *England* in his Majeſty's abſence, as all other remote colonies did. 2. It is known, that in our public prayers, as well as in private families, we pray for our King. 3. When a ſquadron of his ſhips, under Sir *John Harman*, commander, were in the *Weſt-Indies* ſtreightened for proviſions, we freely and ſeaſonably ſent a ſhip laden with proviſions for their ſupply. 4. In that preſent of a ſhip laden with maſts, ſent for the ſupply of his royal navy. Thoſe two laſt things coſt this poor colony ſome thouſands of pounds; and we have not heard, that any of his Majeſty's colonies (though far exceeding us in riches) have given higher demonſtrations of their loyalty.

II. We are ſaid to be factious in the principles of religion. Anſwer. If Mr. *Perkins*, and thoſe good old Puritans in King *Edward* the VIth and Queen *Elizabeth*'s time, did, in their principles of religion, teach evil doctrine (which we conceive no true Proteſtant will ſay) then may we be rendered ſuch; for our religion and principles are the ſame for ſubſtance with thoſe old Chriſtians and reformers called Puritans.

III. It is ſaid, we are a divided people. We acknowledge it is a matter to be greatly bewailed, that the church of God, all the world over (by reaſon of man's weakneſs and infirmity) doth labour under diverſity of perſuaſions and apprehenſions in matters of religion, and conſequently do not live in that bleſſed and ſweet unity that God requires: but for our parts (ſome petty differences excepted) we bleſs God, we have much peace and tranquillity in church and ſtate.

IV. We are charged with carrying it diſreſpectfully towards his Majeſty's commiſſioners. To this we ſay, that God and man can witneſs for us, that our treatment of them was with civility, according to our mean conditions. Indeed, as to yielding obedience unto their mandates, which were deſtructive to our royal charter; as that was contrary to his Majeſty's inſtructions and letter ſent to us by them, ſo we had no reaſon to ſubmit to them therein.

V. We are blamed for a great omiſſion touching baptizing of infants. To this we anſwer, that our principles declared to the world in print, particularly that of the laſt ſynod held here, doth ſpeak our judgments to run parallel with other reformed churches, viz. That viſible confederate believers and their ſeed are ſubjects of baptiſm. Indeed in practice there hath been ſome omiſſions thereof, as to the largeſt extent, eſpecially in ſome places; but endeavours are daily uſed to reduce each one to the rule.

VI. We are accuſed of rigidneſs to ſuch as differ from us in matters of religion. To this we ſay, that from the firſt ſettling this plantation, theſe heterodoxes of Familiſm, Anabaptiſm, and of late Quakeriſm, have been looked upon by the godly here as great errors, and the promoters of them diſturbers of peace and order. Thoſe awful and tremendous motions of that ſort of people in *Germany*, and elſewhere, hath ſufficiently alarmed all pious and prudent men to provide a defenſative againſt them. Hence, from our firſt times, laws have been made to ſecure us from that danger; which have, at ſome times, upon juſt occaſions, been executed upon ſome of that ſort of people who have exceeded the rules of moderation in matters of practice: but this we may ſay truly, that ſome peaceable Anabaptiſts, and ſome of other ſects, who have deported themſelves quietly, have and do live here, under the protection of this government, undiſturbedly.

Lastly, we are accuſed for graſping after dominions, more than belongs to us, and in particular, for taking in a place called the Province of *Mayne*, belonging to Mr. *Gorge*. To this we anſwer, that our patent (which is of greater antiquity than his) doth take in that place; and this may be clearly demonſtrated. Again, Mr. *Gorge*'s predeceſſor, finding no profit, deſerted the government thereof, and left the people under ſuch confuſion and diſorder, that they were neceſſitated to petition earneſtly unto the Maſſachuſets, to take them under their government; which they did, at their earneſt deſire, to prevent their devouring one another. The truth is, there is no profit or benefit doth accrue to our government by their addition, but cumber and trouble. They are generally a very poor people, and contribute nothing to us for the ſupport of government in this place. We may truly ſay, our main end in taking them under us hath been a deſire to do them good, outwardly and inwardly; eſpecially to encourage a pious and able miniſter to live among them, and to preach the Goſpel to them, which, through the favour of God, hath been in ſome meaſure attained. Before they came under us, we know not of one preaching and pious miniſter in five or ſix villages there, and ſince (through God's favour) they have been well provided therewith. Godly miniſters indeed were very ſhy to go among them to live, before they were ſettled under this government; and at ſuch time, when the commiſſioners took them off from us, and ſettled ſome juſtices among themſelves, it was but a little while after the commiſſioners were gone, but that people fell into ſuch diviſions and confuſions, that many of their miniſters left them; and the people again earneſtly ſued to us for protection and government, finding no benefit (as they alleged) by ſuch as the commiſſioners had appointed to rule them. Peradventure Mr. *Gorge* and ſome others may apprehend, they are deprived of honour and profit by us in this matter; but, we believe, as it hath, ſo it will be found, that neither the one nor the other would accrue to them, if they had it under their power, according to their deſire.

THUS, noble Sir, we have made bold to give you an account (as briefly as we could) of what we hear is objected againſt us, and our anſwers; committing all to your goodneſs and wiſdom, to make uſe of as you ſhall ſee occaſion.

So deſiring, in all humility, your pardon for our preſumption, in giving you this trouble, with our cordial prayers unto the God of all mercy and grace, to pour upon your head and heart his richeſt bleſſings; with our moſt humble ſervice and love to you preſented, we take leave, deſiring always to remain,

Your honour's

moſt affectionate friends and ſervants,

John Leverett, Governor.
Samuel Symonds, Deputy Governor.
Daniel Gookins, Aſſiſtant.
Richard Ruſſell.
Thomas Dauforth.
John Pynchon.
William Stoughton.
Edward Tyny.

THIS is a duplicate of a letter ſent in December laſt. Dated in *Boſton* in *New-England*, May 10, 1673.

PREFACES

PREFACES
OF
Mr. BOYLE
TO
BOOKS wrote by other perſons, ADVERTISEMENTS, &c.

Mr. *Boyle*'s Preface to *A Tract touching the Skill of a better Way of Anatomy of Man's Body*, by *Yonker Louis de Bills*, Lord of *Koppenſdam*, &c. tranſlated from the *Low-Dutch*, by Mr. *Pell*, and printed at *London*, 1659, in 12mo.

To my much eſteemed friend Samuel Hartlib *the elder, Eſq*;

SIR, October 13, 1659.

YOUR deſires were wont to tend ſo much to the public good, that your virtuoſi would think I wanted philanthropy, as well as civility, if I ſhould refuſe to comply with ſuch as your letter brings me. I ſend you therefore the propoſitions of Monſieur *de Bills*, engliſhed out of Low-Dutch, at my requeſt, by ſuch a perſon, as you will readily think can tranſlate very well, though he can better write things worthy to be tranſlated, when I ſhould have told you, that his name is Mr. *Pell*. The deſign avowed to be aimed at in the propoſitions, and much more the matter of fact, without which they might appear extravagant, made the print, I confeſs, very welcome to me. For though in ſome papers I formerly told you of, I had mentioned divers things I had intended to try, and ſome, that I had already obſerved, in order to the preſervation of animal ſubſtances, and the making ſome of them more durable ſubjects for the anatomiſt to deal with; yet beſides that moſt of the ways I propoſed to myſelf were as yet little more than bare deſigns, I never aimed at (and ſcarce ſo much as hoped for) ſuch ſtrange things, as in this paper the ingenuous publiſher of it tells us he hath already actually performed. And therefore I ſuppoſe, that it will not be unwelcome to you, if not having had the opportunity to ſee the effect of this gentleman's ſkill myſelf, and being ſufficiently indiſpoſed to believe or affirm any ſtrange thing that I have not ſeen, I annex the other paper you lately deſired of me; I mean that, wherein the propoſer's Dutch print came incloſed to me from the *Hague*. For beſides that it contains ſomething of particular, it comes from a French gentleman, whoſe teſtimony may well be conſiderable in matters of this nature; for he is one of that ſelect ſociety of Pariſian philoſophers, of which you and I have ſometimes diſcourſed; and you will find a celebrating mention made of him in the life of *Gaſſendus*, whoſe friend and diſciple he was. And his curioſity leading him to imitate divers of the old Greek philoſophers, who travelled through many countries, only to enrich themſelves with what knowledge they could meet with in them, he went purpoſely to viſit Monſieur *de Bills*, ſoon after he had left *England*, which he lately traverſed; and in his paſſage honoured me with ſeveral of his viſits. I am ſo willing that you ſhould be as wary as I in admitting unlikely things into your belief, that, in putting the French I received into Engliſh, I have been (purpoſely to avoid ſtraining his expreſſions) more careful to do right to his words, than to his ſtile. And as for thoſe things, that appear to have been left out by me, they are ſome of them ſuch matters as concern not our propoſer, and others but ſuch compliments, as require rather my bluſhes, than my tranſlating. The conſent you deſire of me to publiſh the propoſals in our language I muſt not refuſe you. For it is the leaſt we owe to a perſon, that hath endeavoured to oblige mankind, and to thoſe that are willing to encourage ſuch endeavours, by acquainting the ingenious here with his propoſitions, to ſtrive to procure him ſubſcribers, and to afford them the opportunity of encouraging and aſſiſting ſo uſeful a work, as this (ſuppoſing the truth of the hiſtorical part of the paper) is like to prove. It may alſo be hoped, that the noiſe of ſuch diſcoveries abroad will rouſe up our ingenious men at home, and excite them to endeavour to emulate, if not ſurpaſs them; and to keep up that reputation for anatomical diſcoveries and ſkill, that this nation hath of late deſervedly enjoyed. Nor will it be amiſs, by the ſame means, to give as many of thoſe in authority, as may happen to read what the ſtates of *Holland* have done, in favour of a ſtranger, occaſion to take notice, what countenance neighbouring magiſtrates, that are not thought unpolitic neither, are forward to give to thoſe deſigns, that aim at the advancement of real learning. Which may perhaps invite thoſe that can do it, to give encouragement to the Engliſh wits; which I am groundedly confident want nothing but encouragement, to perform things in this kind, that would really advantage the public, as well as highly gratify ſuch perſons as Mr. *Hartlib*, and his

affectionate humble ſervant,

Robert Boyle.

A LETTER

A
LETTER
Prefixed to a Tract tranſlated from French into Engliſh,

BY

Dr. PETER du MOULIN,

ENTITLED,

The DEVIL of MASCON:

The fourth edition of which was printed at *Oxford*, 1669,
in 12mo.

To my reverend and learned friend, Dr. Peter du Moulin.

SIR,

THOUGH I ſuppoſe you will look upon my ſending you Monſieur *Perreaud's* French book, as a minding you of the promiſe you were the other day pleaſed to make me of putting it into an Engliſh dreſs; yet I hope you will do me the right to believe, that if the ſubject were not extraordinary, and if my own pen were not (as you know it is) pre-engaged to a theme of a very diſtant nature, I ſhould think it injurious to the public, and to you, to be acceſſary to his turning tranſlator of another's books, that hath already manifeſted in ſeveral languages, how able he is to write excellent ones of his own.

I MUST freely confeſs to you, that the powerful inclinations, which my courſe of life and ſtudies hath given me to diffidence and backwardneſs of aſſent, and the many fictions and ſuperſtitions, which (as far as I have hitherto obſerved) are wont to blemiſh the relation, where ſpirits and witches are concerned, would make me very backward to contribute any thing to your publiſhing, or any man's believing, a ſtory leſs ſtrange than this of Monſieur *Perreaud*.

BUT the converſation I had with that pious author during my ſtay at *Geneva*, and the preſent he was pleaſed to make me of this treatiſe before it was printed, in a place where I had opportunities to enquire both after the writer, and ſome paſſages of the book, did at length overcome in me (as to this narrative) all my ſettled indiſpoſedneſs to believe ſtrange things. And ſince I find, that you have received an account both of Monſieur *Perreaud* himſelf, and ſeveral things relating to his book, from that great ſcholar and excellent perſon your father, I have no reaſon to doubt, but that as your ſkill in the tongues, out of which and into which this treatiſe is to be tranſlated, will bring it the greateſt advantages, that it can receive from a tranſlator's pen; ſo the reputation, which your and your learned father's names will give it, will prove as effectual as any thing of that nature can be, to make wary readers as much believe even the amazing paſſages of it, as I hope you do that great truth of my being in a high degree,

SIR,

your affectionate friend and humble ſervant,

Robert Boyle.

An Advertiſement of Mr. *BOYLE*'s,

About the loſs of many of his writings, addreſſed to *J. W.* to be communicated to thoſe of his friends, that are virtuoſi; which may ſerve as a kind of preface to moſt of his mutilated and unfiniſhed writings. *London*, printed in May, 1688 two pages, folio.

As for the report, that doubtleſs has reached your ears, of the loſs of ſeveral of my manuſcripts, and the defacing of divers others, it is but too true; and I am very ſenſible of it. But yet it is not barely upon my own account, that I am ſo, but very much upon that of my inquiſitive friends, and Mr. *J. W.* in particular. For I cannot be but troubled, that I find myſelf diſabled to anſwer the expectation they had, that I ſhould gratify their curioſity, by entertaining them with ſeveral tracts upon philoſophical ſubjects; and that ſome unwelcome accidents, that have of late befallen me, oblige me to diſſuade them from expecting hence-forward, that I ſhould preſent them with almoſt any treatiſe, finiſhed and entire. For having been for many years afflicted with a weakneſs of ſight, that neceſſitated me, inſtead of writing myſelf, to dictate to others; and having been neceſſitated to make ſeveral removes, ſome of them with too much haſte to permit me to take an exact care of my papers, or keep all of them together, and take them along with myſelf, from place to place: when, not long ſince, I had occaſion to review and range them, I found, to my ſurprize, as well as trouble, that I wanted four or five centuries of experiments of my own, and other matters of fact, which from time to time I had committed to paper, as they were made and obſerved, and had been, by way partly of diary, and partly of adverſaria, regiſtered and ſet down, one century after another, that I might have them in readineſs to be made uſe of in my deſigned treatiſes. And together with theſe matters of fact, I found miſſing ſeven or eight centuries of notions, remarks, explications and illuſtrations of divers things in philoſophy, which I had committed to writing, as they chanced to occur to my thoughts, and which might have place among the ſame papers with the above mentioned experiments. How all theſe ſhould come to be loſt, whilſt ſome other centuries of notes and ſhort memoirs, ſome of them ſpeculative, and others experimental, eſcaped, I can as little declare, as recover them. But to add to the misfortune (for ſuch it is to me, though perhaps not to the world) one, whom I had ordered to do ſomething with a bottle of oil of vitriol, unluckily broke the glaſs juſt over a flat cheſt of drawers, which I had purpoſely cauſed to be made for no other uſe, than to keep in it my own manuſcript papers, whereof it had then good ſtore. And though I happened to be at that time in the room, and made haſte to unlock the diſtinct drawers, and take them out; yet the highly corroſive liquor had made ſuch haſte and ſuch havock, that ſeveral manuſcripts, and among them ſome that I moſt valued, were quite ſpoiled; inſomuch that there remained not words enough undefaced to declare what ſubject they concerned; and that the other manuſcripts, that miſchievous liquor had reached to, had ſome of them their leaves half conſumed, and others a greater or leſſer part of them; and all that the menſtruum touched, it made ſo rotten, that, notwithſtanding all our diligence, what was once wetted, could never be retrieved.

IT was natural enough, that this concourſe of miſchances ſhould ſuggeſt to me, that I was to take new meaſures in reference to my deſigned but yet unpubliſhed writings. For firſt it ſeemed reaſonable, that either I ſhould wholly ſuppreſs ſome diſcourſes, wherein I had made a conſiderable progreſs, but had not finiſhed them, or elſe ſhould at leaſt acknowledge and give notice they are incomplete, and blemiſhed with divers chaſms, ſince a great many particulars are loſt, that ſhould have done more than fill up thoſe vacancies: which defects I cannot now ſupply, many of the experiments having been made, when I had by me ſome ſuch drugs and other materials, and ſuch exact inſtruments and ſkilful workmen, as I am not now furniſhed with, nor am able to retrieve. Beſides that, I was then alſo ſometimes befriended by opportunities and favourable circumſtances, that I cannot hope for again.

To this firſt reflection it was natural to add another; which was, that ſince I could neither diſcover nor imagine, how moſt of the writings lately mentioned (with ſome others about differing ſubjects) came to be loſt; the ſureſt courſe (if not the only ſure one) that I could take to prevent the like accidents for the future, would be to publiſh from time to time, as faſt as conveniently I could, thoſe remains and fragments, as well as leſs mutilated papers, that yet continued in my hands, premiſing to each diſtinct bundle of them an admonition to thoſe readers, that care for no books, that are not methodical and complete, that they were not only free, but deſired to paſs theſe by, as pieces both confuſed and unfiniſhed.

I MIGHT add, that perhaps it may be more prejudicial to the author than to the reader, that theſe papers come forth with ſuch diſadvantageous circumſtances; ſince, for the moſt part, the method of writings, that treat about experimental philoſophy, is not much minded and remembered by the reader, at leaſt after the firſt peruſal; the notions and experiments themſelves abſtracting from the order they were delivered in, being the things, that philoſophers uſe to take notice of, and permanently retain in their memories. The introductory diſcourſes and prefaces to be met with among ſome of the very incomplete papers that accompany this letter, may perhaps not be unwelcome nor altogether uſeleſs to ſome ingenious men, who will not be diſpleaſed to find themſelves excited, and perchance ſomewhat aſſiſted, to take particular notice of ſome ſubjects, that ſeem worthy of being more thoroughly conſidered and cultivated than yet they have been; and ſome perchance may think the deſigns I had upon ſome ſubjects, not unfit to be purſued by them in their own way and ſtile. And as for theſe heaps of fragments, that ſeem to be more of a chaotic nature (if I may ſo ſpeak) ſince the particulars they mainly conſiſt of, are matters of fact, their being huddled together, without method, (though not always without order) may not hinder them from being fit, if well diſpoſed of, to have places ſomewhere or other in the hiſtory of nature; and to become not unſerviceable materials in the ſtructure, that is aimed at in this age, of an uſeful and well grounded philoſophy.

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II. A Letter prefixed to the fourth Edition of a Tract tranſlated from the *French* by Dr. *Peter du Moulin*, intitled, *The Devil of Maſcon*. *Oxferden* 1669, in 12mo.

III. An Advertiſement of Mr. *Boyle*'s about the Loſs of many of his Writings, addreſſed to *J. W.* to be communicated to thoſe of his Friends that are *Virtuoſi*; which may ſerve as a kind of a Preface to moſt of his mutilated and unfiniſhed Writings. *London* 1688, Fol.

A Liſt of the Titles of the MSS. of Mr. BOYLE, not inſerted in the Six Volumes, for the Reaſons mentioned in the Preface.

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17. Papers on the Martyrdom of *Theodora*, *&c.* never printed.
18. Eſſays on Ethicks, three Books. 1645.
19. Eſſays on Sin and Piety.

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4. Hiſtory of Colours.
5. Hiſtory of human Blood, an Appendix to the publiſhed Tract.
6. A Philoſophical Chaos.
7. Chymico-Medical Chaos.
8. Uſes and Bounds of Experience in natural Philoſophy.
9. The Utility of Experiments ſpeculative and active.
10. *Cogitationes Phyſicæ Miſcellaneæ*.
11. Of compiling a Natural Hiſtory; the Imperfection of—the Uſefulneſs of, *&c.*
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16. Of Poiſons, and of turning them into Antidotes.
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26. Paradoxes about Phlegms.
27. Seminal Principles.
28. Seeds of Plants, Animals, *&c.*
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30. Examen of one Cauſe of the Magnetiſm of the Earth.
*31. The Sceptical Naturaliſt.
*32. Conſiderations touching occult Qualities.

*33. On ſpontaneous Generation.
34. Strange Reports; concerning the ſecond Sight, Relations communicated.
35. A large Collection of Things learned by converſing with Sea-captains, Pilots, *&c.* who have traded to the *Indies*, Eaſt and Weſt, to *Hudſon's Bay*, *&c.*
36. MS. Copies of many of his Tracts turned into *Latin.*
37. A great number of Letters from learned Perſons abroad in *Latin*, *French*, *Italian*, *Dutch*, *&c.* The Number of Letters of all ſorts, printed and unprinted, beſides many ſuppoſed to be loſt, amounts to 1500, and more.

N. B. There are many ſubordinate Titles of Tracts, comprehended under the more general ones above-mentioned, both Theological and Philoſophical, omitted for the ſake of Brevity.

Directions to the Binder.

The Sixteen Copper-Plates are to be placed at the End of the Volumes, to which they reſpectively belong.

NEW EXPERIMENTS PHYSICO-MECHANICAL,

TOUCHING

The **SPRING** of the **AIR**, and its **EFFECTS**;

Made, for the moſt Part, in a

NEW PNEUMATICAL ENGINE.

Written by Way of LETTER

To the Right Honourable CHARLES Lord Viſcount of DUNGARVAN,
eldeſt Son to the Earl of CORKE.

To the Reader.

ALTHOUGH the following treatiſe being far more prolix than becomes a letter, and than I at firſt intended it, I am very unwilling to encreaſe the already exceſſive bulk of the book by a preface; yet there are ſome particulars, that I think myſelf obliged to take notice of to the reader, as things that will either concern him to know, or me to have known.

IN the firſt place then: If it be demanded why I publiſh to the world a letter, which, by its ſtyle and divers paſſages, appears to have been written as well for, as to a particular perſon; I have chiefly theſe two things to anſwer; the one, that the experiments therein related, having been many of them tried in the preſence of ingenious men, and by that means having made ſome noiſe among the Virtuoſi (inſomuch that ſome of them have been ſent into foreign countries, where they have had the luck not to be deſpiſed) I could not, without quite tiring more than one amanuenſis, give out half as many copies of them as were ſo earneſtly deſired, that I could not civilly refuſe them. The other, that intelligent perſons in matters of this kind perſuade me, that the publication of what I had obſerved touching the nature of the air, would not be uſeleſs to the world; and that in an age ſo taken with novelties as is ours, theſe new experiments would be grateful to the lovers of free and real learning: ſo that I might at once comply with my grand deſign of promoting experimental and uſeful philoſophy, and obtain the great ſatisfaction of giving ſome to ingenious men; the hope of which is, I confeſs, a temptation, that I cannot eaſily reſiſt.

OF my being ſomewhat prolix in many of my experiments, I have theſe reaſons to render: that ſome of them being altogether new, ſeemed to need the being circumſtantially related, to keep the reader from diſtruſting them: that divers circumſtances I did here and there ſet down for fear of forgetting them, when I may hereafter have occaſion to make uſe of them in my other writings: that in divers caſes I thought it neceſſary to deliver things circumſtantially, that the perſon I addreſſed them to might, without miſtake, and with as little trouble as is poſſible, be able to repeat ſuch unuſual experiments: and that after I conſented to let my obſervations be made public, the moſt ordinary reaſon of my prolixity was, that foreſeeing, that ſuch a trouble as I met with in making thoſe trials carefully, and the great expence of time that they neceſſarily require (not to mention the charges of making the engine, and employing a man to manage it) will probably keep moſt men from trying again theſe experiments, I thought I might do the generality of my readers no unacceptable piece of ſervice, by ſo punctually relating what I carefully obſerved, that they may look upon theſe narratives as ſtanding records in our new pneumatics, and need not reiterate themſelves an experiment to have as diſtinct an idea of it, as may ſuffice them to ground their reflexions and ſpeculations upon.

And becauſe ſometimes 'tis the diſcourſe made upon the experiment, that makes it appear prolix, I have commonly left a conſpicuous interval betwixt ſuch diſcourſes, and the experiments whereunto they belong, or are annexed; that they, who deſire only the hiſtorical part of the account we give of our engine, may read the narratives, without being put to the trouble of reading the reflexions too: which I here take notice of, for the ſake of thoſe, that are well verſed in the new philoſophy and in the mathematicks: that ſuch may ſkip what was deſigned but for ſuch perſons as may be leſs acquainted, even than I, with matters of this nature (ſcarce ſo much as mentioned by any writer in our language) and not for them, from whom I ſhall be much more forward to learn, than to pretend to teach them. Of my being wont to ſpeak rather doubtfully, or heſitantly, than reſolvedly, concerning matters wherein I apprehend ſome difficulty, I have in another treatiſe (which may, through God's aſſiſtance, come abroad ere long) given a particular, and, I hope, a ſatisfactory account: wherefore I ſhall now defend my practice but by the obſervation of *Ariſtotle*, who ſomewhere notes, that to ſeem to know all things certainly, and to ſpeak poſitively of them, is a trick of bold and young fellows; whereas thoſe, that are indeed intelligent and conſiderate, are wont to employ more wary or diffident expreſſions, or (as he ſpeaks) προϛιθέασιν ἀεὶ τὸ ἴσως ϗ τὸ τάχα.

There are divers reflexions, and other paſſages in the following epiſtle, and even ſome experiments (occaſionally mentioned) which may ſeem either impertinent or ſuperfluous, but are not ſo; being purpoſely written, either to evince ſome truth oppoſed, or diſprove ſome erroneous conceit maintained by ſome eminent new philoſopher, or by ſome other ingenious men, who, I preſumed, would eaſily forgive me the having on ſuch occaſions purpoſely omitted their names; though an inquiſitive perſon will probably diſcover divers of them, by the mention of the opinions diſprov'd in the experiments I am excuſing.

Ever ſince I diſcerned the uſefulneſs of ſpeculative geometry to natural philoſophy, the unhappy diſtempers of my eyes have ſo far kept me from being much converſant in it, that I fear I ſhall need the pardon of my mathematical readers for ſome paſſages, which, if I had been deeply ſkill'd in geometry, I ſhould have treated more accurately.

And indeed, having, for reaſons elſewhere deduced, purpoſely kept myſelf a ſtranger to moſt of the new hypotheſes in philoſophy, I am ſenſible enough, that the engine I treat of hath prevailed with me to write of ſome ſubjects, which are ſufficiently remote from thoſe I have been moſt converſant in. And having been reduced to write the greateſt part of the enſuing letter at a diſtance, not only from my library, but from my own manuſcripts, I cannot but fear, that my diſcourſes do not only want many choice things, wherewith the learned writings of others might have enriched or embelliſhed them; but that partly for this reaſon, and partly for that touched upon a little before, it is poſſible I may have mentioned ſome notions already publiſhed by others, without taking notice of the authors, not out of any deſign to defraud deſerving men, but for want of knowing ſuch particulars to have been already publiſhed by them; eſpecially the experiments of our engine being themſelves ſufficient to hint ſuch notions as we build upon them.

The order of the experiments every reader may alter, as ſuits beſt with his own deſign in purſuing them: for not only all thoſe, betwixt whom there is an affinity in nature (by belonging to one ſubject) are not always placed one by another, but they are not ſtill ſet down ſo much as in the order wherein they were made; but moſt commonly in that caſual one, wherein my occaſions induced me to diſpatch them to the preſs. And, which is worſe, I did uſually ſend quite away the former experiments, before the latter were written, or perhaps ſo much as made: whereby I loſt the advantage of correcting and ſupplying the imperfections of what I had formerly written, by the light of my ſubſequent trials and diſcoveries.

Besides all this, the diſtemper in my eyes forbidding me not only to write my ſelf ſo much as one experiment, but even to read over myſelf what I dictated to others; I cannot but fear, that beſides the author's miſtakes, this edition may be blemiſhed by many, that may be properly imputed to a very unſkilful writer (whom I was oftentimes by haſte reduced, againſt my cuſtom, to employ) and may have eſcaped the diligence of that learned friend, that doth me the favour to overſee the preſs; eſpecially, there being the diſtance of two days journey betwixt it and me.

I need not, perhaps, repreſent to the equitable reader, how much the ſtrange confuſions of this unhappy nation, in the midſt of which I have made and written theſe experiments, are apt to diſturb that calmneſs of mind and undiſtractedneſs of thoughts, that are wont to be requiſite to happy ſpeculations. But I preſume, that by all theſe things put together, he will readily perceive, that I have been ſo far from following the poet's prudent counſel touching the ſlow publication of books deſigned to purchaſe credit by,

——*nonumque prematur in annum,*

that I ſuffer this treatiſe to come abroad into the world with a multitude of diſadvantages.

But if it be demanded, why then I did not make it fitter for the preſs, before I ſent it thither? my anſwer muſt be, that not at firſt imagining, that this ſort of experiments would prove any thing near ſo troubleſome, either to make, or to record, as I afterwards found them, I did, to engage the printer to diſpatch, promiſe him to ſend him the whole epiſtle in a very ſhort time. So that although now and then the occaſional vacations of the preſs, by reaſon of feſtivals, or the abſence of the corrector, gave me the leiſure to expatiate upon ſome ſubject; yet being oftentimes called upon to diſpatch the papers to the preſs, my promiſe, and many unexpected avocations, obliged me to a haſte, which, though it hath detracted nothing from the faithfulneſs of the hiſtorical part of our book, hath (I fear) been diſadvantageous enough to all the reſt. And I made the leſs ſcruple to let the following papers paſs out of my hands, with all their imperfections, becauſe, as the public affairs, and my own, were then circumſtanced, I knew not when (if at all) I ſhould be again in a condition to proſecute experiments of this kind; eſpecially, ſince (to omit my being almoſt weary of being, as it were, confined to one ſort of experiments) I am pre-engaged (if it pleaſe God to vouchſafe me life and health) to employ my firſt leiſure in the publication of ſome other phyſiological papers, which I thought it would make me much the fitter to take in hand, if I firſt diſpatched all that I had at this time to write touching our engine.

I HAVE this farther to add, by way of excuſe, that as it hath been my deſign in publiſhing theſe experiments to gratify ingenious men; ſo, if I have not been much flattered, I may hope, that the various hints to be met with in the following letter will (at leaſt) ſomewhat awaken mens thoughts, and excite them to new ſpeculations, (ſuch as perhaps even inquiſitive men would ſcarce elſe light upon;) and I need not deſpair, that even the examination of ſuch new ſuſpicions and enquiries will hence alſo, at leaſt occaſionally, be facilitated: I ſaid occaſionally, becauſe it being, as it is proverbially ſaid, *facile inventis addere*; it ſeems not irrational to expect, that our engine itſelf, and divers of our experiments, will be much promoted by the induſtry of inventive and mathematical wits, whoſe contrivances may eaſily either correct or ſupply, and conſequently ſurpaſs many of thoſe we have made uſe of. And, particularly, if men by ſkill and patience can arrive both to evacuate ſuch receivers as ours, till there be no more air left in them, than there ſeems to have remained in the glaſſes made uſe of about the Magdeburgic experiment (hereafter to be mentioned), and to keep out the air for a competent while, the uſefulneſs and diſcoveries of our engine will not be a little advanced. And perhaps that may belong to it, which I remember *Seneca* ſpeaks of nature; *initiatos* (ſaith he) *nos credimus, in veſtibulo ejus hæremus.* For being now in a place, where we are not quite deſtitute of moderately ſkilful artificers, we have, ſince the concluſion of the following letter, made ſome additions to our engine, by whoſe help we find (upon ſome new trials) that we may be able, without much of new trouble, to keep the ambient air out of the exhauſted receiver for a whole day. And perhaps we ſhould be able to keep it out much longer, if before we ſhall have diſpatched ſome urgent affairs, and publiſhed ſome papers, for which a kind of promiſe is thought to make us debtors to the preſs, we could be at leiſure to proſecute ſuch experiments, as may poſſibly afford a ſupplement to the following treatiſe, from which I ſhall now no longer detain the reader.

Friendly Reader,

I KNOW all perſons, that have a public ſpirit for the advancement of learning, will think much, that this piece came not out in a language of more general uſe, than this you ſee it now attired in; eſpecially ſince the excellent noble perſon, who is the author, is known to be well able himſelf (being almoſt univerſally a linguiſt) to have given it either the old Latin, or the newer French dreſs.

BUT if it be an honour to a language to be preferred, and this honour breeds ſometimes an emulation, as anciently it did between the Greeks and Romans, it cannot be thought unhandſome for an Engliſh nobleman to have preferred his own: and it may be a ſufficient reaſon for the gentry of foreign parts to learn our ſpeech, or keep interpreters, that they are ſure to have for their requital, from many of our Engliſh writers (as here from this piece) much curiouſly ingenious and profitable learning.

BUT as to this particular, (give me leave to uſe words from a ſtory;) ſince the mountain cannot come to *Mahomet*, *Mahomet* will go to the mountain: I mean thus; becauſe many witty men, perſons of honour and eſtate eſpecially, may be ſuppoſed to be able to make a better account, by employing their ſtudies and time on matter than words, and ſo are juſtly impeded from learning languages; and becauſe (as I may judge)

judge) the noble author is willing to oblige all men, he hath already provided, that this piece ſhall ſhortly be done into Latin, that ſo it may come home to divers worthy perſons in its ſtream, who cannot travel to find it out in its firſt origin.

HAVING therefore leave ſo to do, I cannot forbear to give the world the advertiſement of this Latin edition, leſt ſome ſkilful artiſt ſhould take needleſs pains about a work, which will ere long (by God's furtherance) be done to his hands. For ſuch unprofitable expences of ſtudy have too frequently happened, and too much to the diſadvantage of learning, for want of a ſufficient correſpondence and intercourſe between ſuch as are exerciſed in the mines of wiſdom.

THIS is all the trouble I ſhall at preſent give you: nor ſhall I need mind you, if you have a true guſt for the book you read, to have an honour and thankful regard to the perſon, that hath favoured us with the communication of theſe his trials, and is manifeſtly ſo great a patron and friend to experimental learning, and all true wiſdom: for ſhould you fail in this, you might deſervedly be deprived of ſome other obſervations on the ſame ſubject, which the author, I hear, hath made ſince the finiſhing of this treatiſe.

I DESIRE to be excuſed, that I do not make excuſes for the ſlowneſs of the publication, hoping that the long expectation you have had of it, will enhance, and not diminiſh your delight in the enjoyment of a piece like to be, amongſt the ſtudents in accurate philoſophy, of ſo general acceptance. Farewel.

R. Sh.

TO THE

LORD of *DUNGARVAN*,

my Honoured and Dear NEPHEW.

MY DEAR LORD,

RECEIVING in your laſt from *Paris* a deſire, that I would add ſome more experiments to thoſe I formerly ſent you over; I could not be ſo much your ſervant as I am, without looking upon that deſire as a command; and conſequently, without thinking myſelf obliged to conſider by what ſort of experiments it might the moſt acceptably be obeyed. And at the ſame time, perceiving by letters from ſome other ingenious perſons at *Paris*, that ſeveral of the Virtuoſi there were very intent upon the examination of the intereſt of the air, in hindering the deſcent of the quickſilver, in the famous experiment touching a vacuum; I thought I could not comply with your deſires in a more fit and ſeaſonable manner, than by proſecuting and endeavouring to promote that noble experiment of *Torricellius*; and by preſenting your Lordſhip an account of my attempts to illuſtrate a ſubject, about which its being ſo much diſcourſed of where you are, together with your inbred curioſity, and love of experimental learning, made me ſuppoſe you ſufficiently inquiſitive.

AND though I pretend not to acquaint you, on this occaſion, with any ſtore of new diſcoveries, yet poſſibly I ſhall be ſo happy, as to aſſiſt you to know ſome things, which you did formerly but ſuppoſe; and ſhall preſent you, if not with new theories, at leaſt with new proofs of ſuch as are not yet become unqueſtionable. And if what

I ſhall deliver hath the good fortune to encourage and aſſiſt you to proſecute the hints it will afford, I ſhall account myſelf, in paying of a duty to you, to have done a piece of ſervice to the commonwealth of learning. Since it may highly conduce to the advancement of that experimental philoſophy, the effectual purſuit of which requires as well a purſe as a brain, to endear it to hopeful perſons of your quality, who may accompliſh many things, which others cannot but wiſh, or at moſt but deſign, by being able to employ the preſents of fortune in the ſearch of the myſteries of nature.

AND I am not faintly induced to make choice of this ſubject, rather than any of the expected chymical ones, to entertain your Lordſhip upon, by theſe two conſiderations: the one, that the air being ſo neceſſary to human life, that not only the generality of men, but moſt other creatures that breathe, cannot live many minutes without it, any conſiderable diſcovery of its nature ſeems likely to prove of moment to mankind. And the other is, that the ambient air being that, whereto both our own bodies, and moſt of the others we deal with here below, are almoſt perpetually contiguous, not only its alterations have a notable and manifeſt ſhare in thoſe obvious effects, that men have already been invited to aſcribe thereunto, (ſuch as are the various diſtempers incident to human bodies, eſpecially if crazy in the ſpring, the autumn, and alſo on moſt of the great and ſudden changes of weather;) but likewiſe, the further diſcovery of the nature of the air will probably diſcover to us, that it concurs more or leſs to the exhibiting of many phænomena, in which it hath hitherto ſcarce been ſuſpected to have any intereſt. So that a true account of any experiment that is new concerning a thing, wherewith we have ſuch conſtant and neceſſary intercourſe, may not only prove of ſome advantage to human life, but gratify philoſophers, by promoting their ſpeculations on a ſubject, which hath ſo much opportunity to ſolicit their curioſity.

AND I ſhould immediately proceed to the mention of my experiments, but that I like too well that worthy ſaying of the naturaliſt *Pliny*, *benignum eſt & plenum ingenui pudores, fateri per quos profeceris*, not to conform to it, by acquainting your Lordſhip, in the firſt place, with the hint I had of the engine I am to entertain you with. You may be pleaſed to remember, that a while before our ſeparation in *England*, I told you of a book, that I had heard of, but not peruſed, publiſhed by the induſtrious Jeſuit *Schottus*; wherein, it was ſaid, he related how that ingenious gentleman, *Otto Gericke*, conſul of *Magdeburg*, had lately practiſed in *Germany* a way of emptying glaſs veſſels, by ſucking out the air at the mouth of the veſſel, plunged under water. And you may alſo perhaps remember, that I expreſſed myſelf much delighted with this experiment, ſince thereby the great force of the external air (either ruſhing in at the opened orifice of the emptied veſſel, or violently forcing up the water into it) was rendered more obvious and conſpicuous than in any experiment that I had formerly ſeen. And though it may appear by ſome of thoſe writings I ſometimes ſhewed your Lordſhip, that I had been ſolicitous to try things upon the ſame ground; yet in regard this gentleman was before-hand with me in producing ſuch conſiderable effects by means of the exſuction of air, I think myſelf obliged to acknowledge the aſſiſtance and encouragement the report of his performances hath afforded me.

In Præfat. Lib. I.

BUT, as few inventions happen to be at firſt ſo complete, as not to be either blemiſhed with ſome deficiencies needful to be remedied, or otherwiſe capable of improvement; ſo when the engine, we have been ſpeaking of, comes to be more attentively conſidered, there will appear two very conſiderable things to be deſired in it. For firſt, the wind-pump (as ſomebody not improperly calls it) is ſo contrived, that

to

to evacuate the veſſel, there is required the continual labour of two ſtrong men for divers hours. And next (which is an imperfection of much greater moment) the receiver, or glaſs to be emptied, conſiſting of one entire and uninterrupted globe and neck of glaſs; the whole engine is ſo made, that things cannot be conveyed into it, whereon to try experiments: ſo that there ſeems but little (if any thing) more to be expected from it, than thoſe very few phænomena, that have been already obſerved by the author, and recorded by *Schottus*. Wherefore to remedy theſe inconveniences, I put both Mr. *G.* and *R. Hook* (who hath alſo the honour to be known to your Lordſhip, and was with me when I had theſe things under conſideration) to contrive ſome air-pump, that might not, like the other, need to be kept under water (which on divers occaſions is convenient) and might be more eaſily managed: and after an unſucceſsful trial or two of ways propoſed by others, the laſt-named perſon fitted me with a pump, anon to be deſcribed. And thus the firſt imperfection of the German engine was in good meaſure, though not perfectly remedied: and to ſupply the ſecond defect, it was conſidered, that it would not perhaps prove impoſſible to leave in the glaſs to be emptied a hole large enough to put in a man's arm cloathed; and conſequently other bodies, not bigger than it, or longer than the inſide of the veſſel. And this deſign ſeemed the more hopeful, becauſe I remembered, that having ſeveral years before often made the experiment *de vacuo* with my own hands; I had, to examine ſome conjectures that occurred to me about it, cauſed glaſſes to be made with a hole at that end, which uſes to be ſealed up, and had nevertheleſs been able, as occaſion required, to make uſe of ſuch tubes, as if no ſuch holes had been left in them, by deviſing ſtopples for them, made of the common plaiſter called diachylon; which, I rightly enough gueſſed, would, by reaſon of the exquiſite commixtion of its ſmall parts, and cloſeneſs of its texture, deny all acceſs to the external air. Wherefore, ſuppoſing that by the help of ſuch plaiſters carefully laid upon the commiſſures of the ſtopple and hole to be made in the receiver, the external air might be hindered from inſinuating itſelf between them into the veſſel, we cauſed ſeveral ſuch glaſſes, as you will find deſcribed a little lower, to be blown at the glaſs-houſe. And though we could not get the workmen to blow any of them ſo large, or of ſo convenient a ſhape as we would fain have had; yet finding one to be tolerably fit, and leſs unfit than any of the reſt, we were content to make uſe of it in that engine; of which, I ſuppoſe, you by this time expect a deſcription, in order to the recital of the phænomena exhibited by it.

To give your Lordſhip then, in the firſt place, ſome account of the engine itſelf; it conſiſts of two principal parts; a glaſs veſſel, and a pump to draw the air out of it.

THE former of theſe (which we, with the glaſs-men, ſhall often call a receiver, for its affinity to the large veſſels of that name, uſed by chymiſts) conſiſts of a glaſs with a wide hole at the top, of a cover to that hole, and of a ſtop-cock faſtened to the end of the neck, at the bottom.

THE ſhape of the glaſs, you will find expreſſed in the firſt figure of the annexed ſcheme. And for the ſize of it, it contained about 30 wine quarts, each of them containing near two pound (of 16 ounces to the pound) of water. We ſhould have been better pleaſed with a more capacious veſſel; but the glaſs-men profeſſed themſelves unable to blow a larger, of ſuch a thickneſs and ſhape as was requiſite to our purpoſe.

AT the very top of the veſſel A, you may obſerve a round hole, whoſe diameter BC is of about four inches; and whereof the orifice is incircled with a lip of glaſs, almoſt an inch high: for the making of which lip, it was requiſite (to mention that

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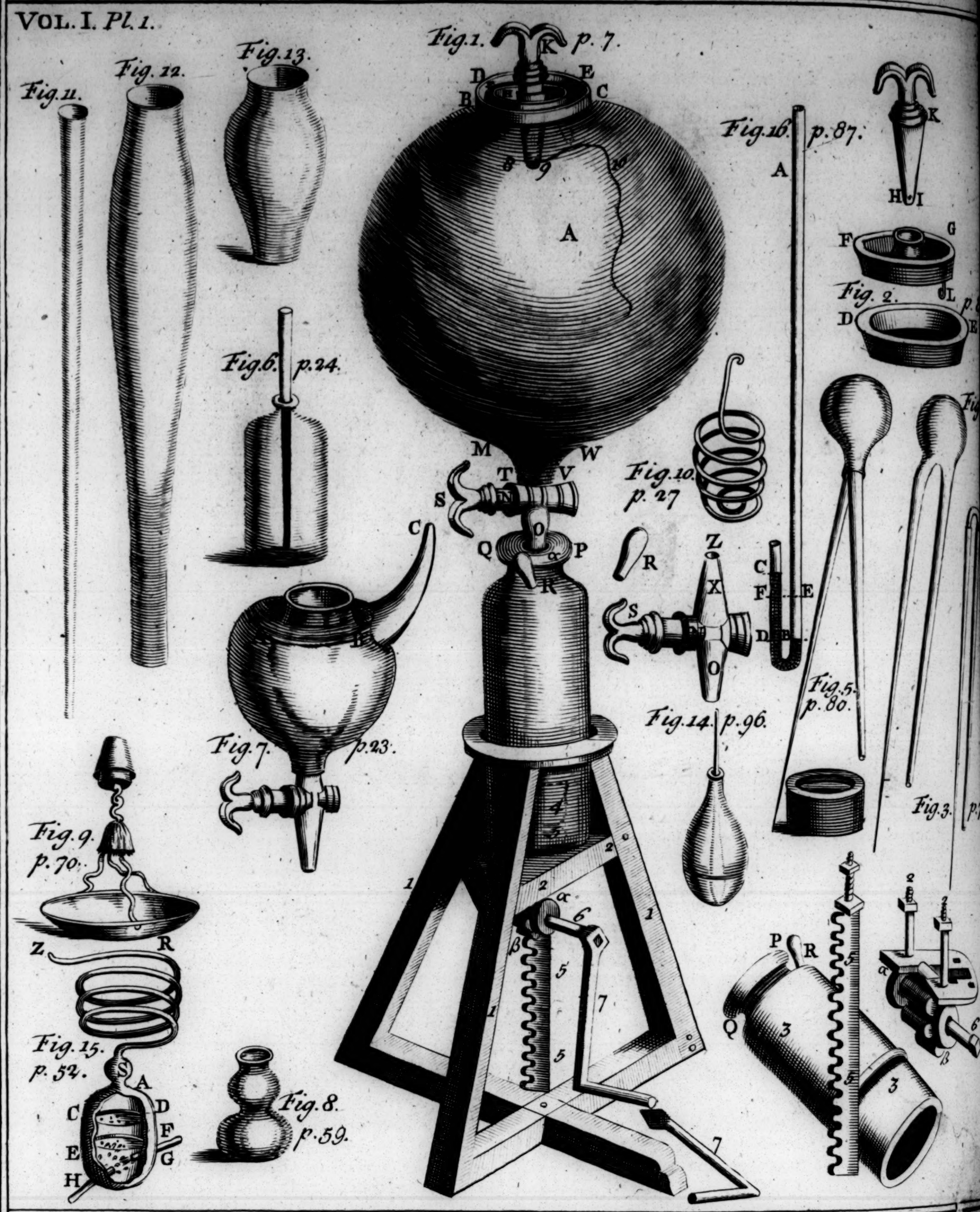

Fig. 11.
Fig. 12.
Fig. 13.
Fig. 1. K p. 7.
D E
B C
8 9 10
A
Fig. 16. p. 87.
A
K
H I
F G
Fig. 2.
D L E
Fig. 6. p. 24.
M T V W
S N
O
Q P
R
C
B
Fig. 10. p. 27
R
Z
X
C
F E
S
D B
O
Fig. 5. p. 80.
Fig. 14. p. 96.
Fig. 7. p. 23.
Fig. 3.
Fig. 9. p. 70.
1 2 4 2 1
α 6
β 5
5 7
1
5
7
P R
Q 3
5
5 3
2 2
α
6
β
Z R
Fig. 15. p. 52.
A
C D
F
E G
H
Fig. 8. p. 59.

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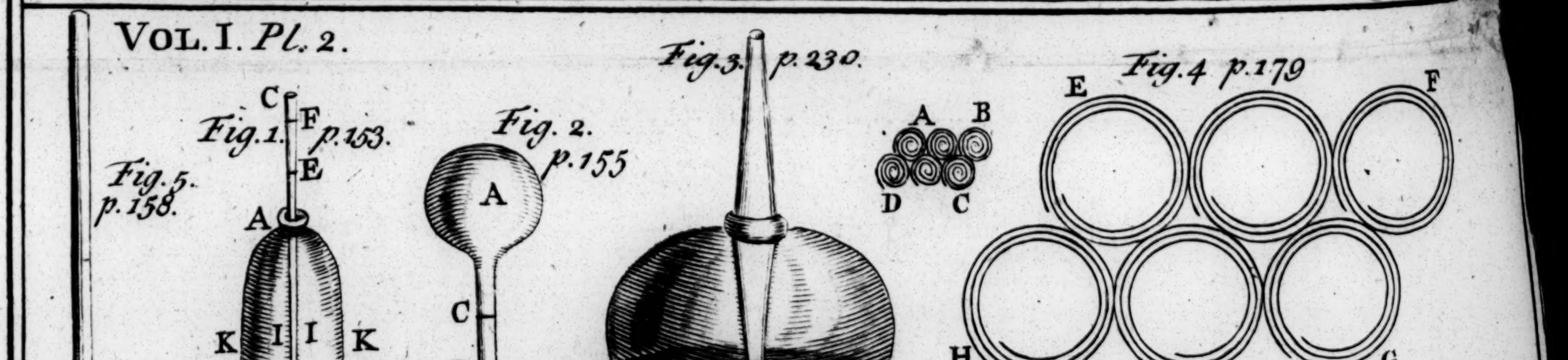

Fig. 5. p. 158.
C
F
Fig. 1. p. 153.
E
A
K I I K
G
H D I
B
Fig. 2. p. 155
A
C
D
Fig. 3. p. 230.
A B
D C
Fig. 4 p. 179
E F
H G
Fig. 6. p. 183.
M
C
O
G
L
D H I F B
N
E
K
A

J. Mynde f.

upon the by, in caſe your Lordſhip ſhould have ſuch another engine made for you) to have a hollow and tapering pipe of glaſs drawn out, whereof the orifice above mentioned was the baſis; and then to have the cone cut off with an hot iron, within about an inch of the points B, C.

The uſe of the lip is to ſuſtain the cover delineated in the ſecond figure; where D E points out a braſs ring, ſo caſt, as that it doth cover the lip B C of the firſt figure, and is cemented on, upon it, with a ſtrong and cloſe cement. To the inward tapering orifice of this ring (which is about three inches over) are exquiſitely ground the ſides of the braſs ſtopple F G; ſo that the concave ſuperficies of the one, and the convex of the other, may touch one another in ſo many places, as may leave as little acceſs, as poſſible, to the external air. And in the midſt of this cover is left a hole H I, of about half an inch over, invironed alſo with a ring or ſocket of the ſame metal, and fitted likewiſe with a braſs ſtopple K, made in the form of the key of a ſtop-cock, and exactly ground into the hole H I it is to fill; ſo as that, though it be turned round in the cavity it poſſeſſes, it will not let in the air, and yet may be put in or taken out at pleaſure, for uſes to be hereafter mentioned. In order to ſome of which, it is perforated with a little hole 8, traverſing the whole thickneſs of it at the lower end; through which, and a little braſs ring L faſtened to one ſide (no matter which) of the bottom of the ſtopple F G, a ſtring 8, 9, 10, might paſs, to be employed to move ſome things in the capacity of the emptied veſſel, without any where unſtopping it.

The laſt thing belonging to our receiver is the ſtop-cock, deſigned in the firſt figure by N, for the better faſtening of which to the neck, and exacter excluſion of the air, there was ſodered on to the ſhank of the cock X, a plate of tin M T U W, long enough to cover the neck of the receiver. But becauſe the cementing of this was a matter of ſome difficulty, it will not be amiſs to mention here the manner of it; which was, that the cavity of the tin plate was filled with a melted cement, made of pitch, roſin, and wood-aſhes, well incorporated; and to hinder this liquid mixture from getting into the orifice Z of the ſhank X, that hole was ſtopped with a cock, to which was faſtened a ſtring, whereby it might be pulled out at the upper orifice of the receiver; and then, the glaſs neck of the receiver being well warmed was thruſt into this cement, and over the ſhank, whereby it was effected, that all the ſpace betwixt the tin plate and the receiver, and betwixt the internal ſuperficies of the receiver and the ſhank of the cock was filled with the cement. And ſo we have diſpatched the firſt upper part of the engine.

The undermoſt remaining part conſiſts of a frame, and of a ſucking-pump, or, as we formerly called it, an air-pump, ſupported by it. The frame is of wood, ſmall but very ſtrong, conſiſting of three legs, 1, 1, 1, ſo placed, that one ſide of it may ſtand perpendicular, that the free motion of the hand may not be hindered. In the midſt of which frame is tranſverſely nailed a board 2, 2, 2, which may not improperly be called a midriff; upon which reſts, and to which is ſtrongly faſtened, the main part of the pump itſelf, which is the only thing remaining to be deſcribed.

The pump conſiſts of four parts, a hollow cylinder, a ſucker, a handle to move that ſucker, and a valve.

The cylinder was (by a pattern) caſt of braſs; it is in length about 14 inches, thick enough to be very ſtrong, notwithſtanding the cylindrical cavity left within it: this cavity is about three inches diameter, and makes as exact a cylinder as the artificer was able to bore. This hollow cylinder is fitted with a ſucker 4, 4, 5, 5, conſiſting of two parts; the one 4, 4, ſomewhat leſs in diameter than the cavity of the cylinder; upon which is nailed a good thick piece of tanned ſhoe-leather, which will go ſo cloſe to the cylinder, that it will need to be very forcibly knocked and rammed in, if at any time it be taken out: which is therefore done, that it may the more exactly hinder the air from inſinuating itſelf betwixt it and the ſides of the cylinder whereon it is to move.

To the midſt of this former part of the ſucker is ſtrongly faſtened the other; namely, a thick and narrow plate of iron 5, 5, ſomewhat longer than the cylinder; one of whoſe edges is ſmooth, but at the other edge it is indented (as I may ſo ſpeak) with a row of teeth, delineated in the ſcheme, into whoſe intervals are to be fitted the teeth of a ſmall iron nut $\alpha\ \beta$ (as tradeſmen call it) which is faſtened by two ſtaples 2, 2, to the under ſide of the formerly mentioned tranſverſe board 2, 2, 2, on which the cylinder reſts, and is turned to and fro by the third piece of this pump, namely, the handle or ***manubrium*** 7, of which the figure gives a ſufficient deſcription.

THE fourth and laſt part of this cylinder is the valve R, conſiſting of a hole bored through at the top of the cylinder, a little tapering towards the cavity; into which hole is ground a tapering peg of braſs, to be thruſt in and taken out at pleaſure.

THE engine being thus deſcribed, it will be requiſite to add, that ſomething is wont to be done before it be ſet on work, for the more eaſy moving of the ſucker, and for the better excluſion of the outward air; which, when the veſſel begins to be exhauſted, is much more difficult to be kept out, than one would eaſily imagine.

THERE muſt then be firſt poured in at the top of the receiver a little ſallad oil, partly to fill up any ſmall intervals, that may happen to be betwixt the contiguous ſurfaces of the internal parts of the ſtop-cock; and partly that it may be the more eaſy to turn the key S, backwards and forwards. Pretty ſtore of oil muſt alſo be poured into the cylinder, both that the ſucker may ſlip up and down in it the more ſmoothly and freely, and that the air might be the better hindered from getting in between them. And for the like reaſons, a little oil is to be uſed alſo about the valve. Upon which occaſion, it would not be omitted (for it is ſtrange) that oftentimes, when neither the pouring in of water, nor even of oil alone, proved capable to make the ſucker move eaſily enough in the cylinder; a mixture of both theſe liquors would readily (ſometimes even to admiration) perform the deſired effect. And laſtly, the braſs cover of the receiver being put into the braſs ring formerly deſcribed, that no air may get between them, it will be very requiſite to plaiſter over very carefully the upper edges of both with the plaiſter formerly mentioned, or ſome other as cloſe, which is to be ſpread upon the edges with an hot iron; that, being melted, it may run into and fill up all the crannies, or other little cavities, at which the air might otherwiſe get entrance.

ALL things being thus fitted, and the lower ſhank O of the ſtop-cock being put into the upper orifice of the cylinder, and into which it was exactly ground; the experimenter is firſt, by turning the handle, to force the ſucker to the top of the cylinder, that there may be no air left in the upper part of it. Then ſhutting the valve with the plug, and turning the other way, he is to draw down the ſucker to the bottom of the cylinder; by which motion of the ſucker, the air that was formerly in the cylinder being thruſt out, and none being permitted to ſucceed in its room, it is manifeſt, that the cavity of the cylinder muſt be empty in reference to the air. So that if thereupon the key of the ſtop-cock be ſo turned, as that through the perforation of it a free paſſage be opened betwixt the cylinder and the receiver, part of the air formerly contained in the receiver will nimbly deſcend into the cylinder. And this air being, by the turning back of the key, hindered from the returning into the receiver, may, by the opening of the valve, and forcing up of the ſucker to the top of the cylinder again, be driven out into the open air. And thus by the repetition of the motion of the ſucker upward and downward, and by opportunely turning the key, and ſtopping the valve, as occaſion requires, more or leſs air may be ſucked out of the receiver, according to the exigency of the experiment, and the intention of him that makes it.

YOUR Lordſhip will perhaps think, that I have been unneceſſarily prolix in this firſt part of my diſcourſe: but if you had ſeen how many unexpected difficulties we found to keep out the external air, even for a little while, when ſome conſiderable part of the internal had been ſucked out, you would peradventure allow, that I might have ſet down more circumſtances than I have, without ſetting down any, whoſe knowledge, he that ſhall try the experiment, may not have need of. Which is ſo true, that, before we proceed any further, I cannot think it unſeaſonable to advertiſe your Lordſhip, that there are two chief ſorts of experiments, which we deſigned in our engine to make trial of: the one, ſuch as may be quickly diſpatched, and therefore may be tried in our engine, though it leak a little, becauſe the air may be faſter drawn out by nimbly plying the pump, than it can get in at undiſcerned leaks; I ſay, at undiſcerned leaks, becauſe ſuch as are big enough to be diſcovered, can ſcarce be uneaſy to be ſtopt. The other ſort of experiments conſiſt of thoſe, that require, not only that the internal air be drawn out of the receiver, but that it be likewiſe for a long time kept out of it. Such are the preſervation of animal and other bodies therein, the germination and growth of vegetables, and other trials of ſeveral ſorts, which it is apparent cannot be well made, unleſs the external air can, for a competent while, be excluded: ſince, even at a very ſmall leak, there may enough get in, to make the vacuum ſoon loſe that name; by which I here declare once for all, that I underſtand not a ſpace, wherein there is no body at all, but ſuch as is either altogether, or almoſt totally devoid of air.

Now this diſtinction of experiments I thought fit to premiſe to the enſuing narratives, becauſe, upon trial, we found it ſo exceeding (and ſcarce imaginably) difficult a matter, to keep out the air from getting at all in at any imperceptible hole or flaw whatſoever, (in a veſſel immediately ſurrounded with the compreſſed atmoſphere) that in ſpite of all our care and diligence we never were able totally to exhauſt the receiver, or keep it, when it was almoſt empty, any conſiderable time, from leaking more or leſs: although (as we have lately intimated) by unwearied quickneſs in plying the pump, the internal air can be much faſter drawn out than the external can get in, till the receiver come to be almoſt quite empty. And that is enough to enable men to diſcover hitherto unobſerved phænomena of nature.

THE experiments therefore of the firſt ſort will, I fear, prove the only ones, wherewith my avocations will allow me to entertain your Lordſhip in this letter. For till your further commands ſhall engage me to undertake, by God's permiſſion, ſuch an employment, and more leiſure ſhall better fit me for it, I know not whether I ſhall be in a condition to try what may be done, to enable me to give you ſome account of the other ſort of experiments alſo.

EXPERIMENT I.

TO proceed now to the phænomena exhibited to us by the engine above deſcribed; I hold it not unfit to begin with what doth conſtantly and regularly offer itſelf to our obſervation, as depending upon the fabric of the engine itſelf, and not upon the nature of this or that particular experiment, which it is employed to try.

FIRST then, upon the drawing down of the ſucker (the valve being ſhut) the cylindrical ſpace, deſerted by the ſucker, is left devoid of air; and therefore, upon the turning of the key, the air contained in the receiver ruſheth into the emptied cylinder, till the air in both thoſe veſſels be brought to about an equal meaſure of dilatation. And therefore, upon ſhutting the receiver by returning the key, if you open the valve, and force up the ſucker again, you will find, that after this firſt exſuction you will drive out almoſt a whole cylinder full of air: but at the following exſuctions, you will draw leſs and leſs of air out of the receiver into the cylinder, becauſe there will ſtill remain leſs and leſs air in the receiver itſelf; and conſequently, the particles of the remaining air, having more room to extend themſelves in, will leſs preſs out one another. This you will eaſily perceive, by finding that you ſtill force leſs and leſs air out of the cylinder; ſo that when the receiver is almoſt exhauſted, you may force up the ſucker almoſt to the top of the cylinder, before you will need to unſtop the valve to let out any air. And if at ſuch time, the valve being ſhut, you let go the handle of the pump, you will find the ſucker forcibly carried up to the top of the cylinder, by the protruſion of the external air; which, being much leſs rarified than that within the cylinder, muſt have a more forcible preſſure upon the ſucker, than the internal is able to reſiſt: and by this means you may know how far you have emptied the receiver. And to this we may add, on this occaſion, that conſtantly upon the turning of the key to let out the air from the receiver into the emptied cylinder, there is immediately produced a conſiderably briſk noiſe, eſpecially whilſt there is any plenty of air in the receiver.

FOR the more eaſy underſtanding of the experiments triable by our engine, I thought it not ſuperfluous nor unſeaſonable in the recital of this firſt of them, to inſinuate that notion, by which it ſeems likely, that moſt, if not all of them, will prove explicable. Your Lordſhip will eaſily ſuppoſe, that the notion I ſpeak of is, that there is a ſpring, or elaſtical power in the air we live in. By which ἐλατὴρ or ſpring of the air, that which I mean is this; that our air either conſiſts of, or at leaſt abounds with, parts of ſuch a nature, that in caſe they be bent or compreſſed by the weight of the incumbent part of the atmoſphere, or by any other body, they do endeavour, as much as in them lieth, to free themſelves from that preſſure, by bearing againſt the contiguous bodies that keep them bent; and, as ſoon as thoſe bodies are removed, or reduced to give them way, by preſently unbending and ſtretching out themſelves, either quite, or ſo far forth as the contiguous bodies that reſiſt them will permit, and thereby expanding the whole parcel of air, theſe elaſtical bodies compoſe.

THIS notion may perhaps be ſomewhat further explained, by conceiving the air near the earth to be ſuch a heap of little bodies, lying one upon another, as may be reſembled to a fleece of wool. For this (to omit other likeneſſes betwixt them) conſiſts of many ſlender and flexible hairs; each of which may indeed, like a little ſpring, be eaſily bent or rolled up; but will alſo, like a ſpring, be ſtill endeavouring to ſtretch itſelf out again. For though both theſe hairs, and the aëreal corpuſcles to which we liken them, do eaſily yield to external preſſures; yet, each of them (by virtue of its ſtructure) is endowed with a power or principle of ſelf-dilatation; by virtue whereof, though the hairs may by a man's hand be bent and crouded cloſer together, and into a narrower room than ſuits beſt with the nature of the body; yet, whilſt the compreſſion laſts, there is in the fleece they compoſe an endeavour outwards, whereby it continually thruſts againſt the hand that oppoſes its expanſion. And upon the removal of the external preſſure, by opening the hand more or leſs, the compreſſed wool doth, as it were, ſpontaneouſly expand or diſplay itſelf towards the recovery of its former more looſe and free condition, till the fleece hath either regained its former dimenſions, or at leaſt approached them as near as the compreſſing hand (perchance not quite opened) will permit. This power of ſelf-dilatation is ſomewhat more conſpicuous in a dry ſpunge compreſſed, than in a fleece of wool. But yet we rather choſe to employ the latter on this occaſion, becauſe it is not, like a ſpunge, an entire body, but a number of ſlender and flexible bodies, looſely complicated, as the air itſelf ſeems to be.

There is yet another way to explicate the ſpring of the air; namely, by ſuppoſing with that moſt ingenious gentleman, Monſieur *Des Cartes*, that the air is nothing but a congeries or heap of ſmall and (for the moſt part) of flexible particles, of ſeveral ſizes, and of all kind of figures, which are raiſed by heat (eſpecially that of the ſun) into that fluid and ſubtle ethereal body that ſurrounds the earth; and by the reſtleſs agitation of that celeſtial matter, wherein thoſe particles ſwim, are ſo whirled round, that each corpuſcle endeavours to beat off all others from coming within the little ſphere requiſite to its motion about its own centre; and in caſe any, by intruding into that ſphere, ſhall oppoſe its free rotation, to expel or drive it away: ſo that, according to this doctrine, it imports very little, whether the particles of the air have the ſtructure requiſite to ſprings, or be of any other form (how irregular ſoever) ſince their elaſtical power is not made to depend upon their ſhape or ſtructure, but upon the vehement agitation, and (as it were) brandiſhing motion, which they receive from the fluid æther, that ſwiftly flows between them, and whirling about each of them (independently from the reſt) not only keeps thoſe ſlender aëreal bodies ſeparated and ſtretched out (at leaſt, as far as the neighbouring ones will permit) which otherwiſe, by reaſon of their flexibleneſs and weight, would flag or curl; but alſo makes them hit againſt, and knock away each other, and conſequently require more room than that, which, if they were compreſſed, they would take up.

By theſe two differing ways, my Lord, may the ſprings of the air be explicated. But though the former of them be that, which by reaſon of its ſeeming ſomewhat more eaſy, I ſhall for the moſt part make uſe of in the following diſcourſe; yet am I not willing to declare peremptorily for either of them againſt the other. And indeed, though I have in another treatiſe endeavoured to make it probable, that the returning of elaſtical bodies (if I may ſo call them) forcibly bent, to their former poſition, may be mechanically explicated; yet I muſt confeſs, that to determine whether the motion of reſtitution in bodies proceed from this, that the parts of a body of a peculiar ſtructure are put into motion by the bending of the ſpring, or from the endeavour of ſome ſubtle ambient body, whoſe paſſage may be oppoſed or obſtructed, or elſe its preſſure unequally reſiſted by reaſon of the new ſhape or magnitude, which the bending of a ſpring may give the pores of it: to determine this, I ſay, ſeems to me a matter of more difficulty, than at firſt ſight one would eaſily imagine it. Wherefore I ſhall decline meddling with a ſubject, which is much more hard to be explicated than neceſſary to be ſo by him, whoſe buſineſs it is not, in this letter, to aſſign the adequate cauſe of the ſpring of the air, but only to manifeſt, that the air hath a ſpring, and to relate ſome of its effects.

I know not whether I need annex, that though either of the above-mentioned hypotheſes, and perhaps ſome others, may afford us an account plauſible enough of the air's ſpring; yet I doubt, whether any of them gives us a ſufficient account of its nature. And of this doubt I might here mention ſome reaſons, but that, peradventure, I may (God permitting) have a fitter occaſion to ſay ſomething of it elſewhere. And therefore I ſhould now proceed to the next experiment, but that I think it requiſite, firſt, to ſuggeſt to your Lordſhip what comes into my thoughts, by way of anſwer to a plauſible objection, which I foreſee you may make againſt our propoſed doctrine, touching the ſpring of the air. For it may be alledged, that though the air were granted to conſiſt of ſpringy particles (if I may ſo ſpeak) yet thereby we could only give an account of the dilatation of the air in wind-guns, and other pneumatical engines, wherein the air hath been compreſſed, and its ſprings violently bent by an apparent external force; upon the removal of which, it is no wonder, that the air ſhould, by the motion of reſtitution, expand itſelf till it hath recovered its more natural dimenſions: whereas, in our above-mentioned firſt experiment, and in almoſt all others triable in our engine, it appears not, that any compreſſion of the air preceded its ſpontaneous dilatation or expanſion of itſelf. To remove this difficulty, I muſt deſire your Lordſhip to take notice, that of whatever nature the air, very remote from the earth, may be, and whatever the ſchools may confidently teach to the contrary, yet we have divers experiments to evince, that the atmoſphere we live in is not (otherwiſe than comparatively to more ponderous bodies) light, but heavy. And did not their gravity hinder them, it appears not why the ſtreams of the terraqueous globe, of which our air in great part conſiſts, ſhould not riſe much higher, than the refractions of the ſun, and other ſtars, give men ground to think, that the atmoſphere, (even in the judgment of thoſe recent aſtronomers, who ſeem willing to enlarge its bounds as much as they dare,) doth reach.

BUT leſt you ſhould expect my ſeconding this reaſon by experience; and leſt you ſhould object, that moſt of the experiments, that have been propoſed to prove the gravity of the air, have been either barely propoſed, or perhaps not accurately tried; I am content, before I paſs further, to mention here, that I found a dry lamb's bladder containing near about two thirds of a pint, and compreſſed by a packthread tied about it, to loſe a grain and the eighth part of a grain of its former weight, by the receſs of the air upon my having prickt it: and this with a pair of ſcales, which, when the full bladder and the correſpondent weight were in it, would manifeſtly turn either way with the 32^{d} part of the grain. And if it be further objected, that the air in the bladder was violently compreſſed by the packthread and the ſides of the bladder, we might probably (to wave prolix anſwers) be furniſhed with a reply, by ſetting down the differing weight of our receiver, when emptied, and when full of uncompreſſed air, if we could here procure ſcales fit for ſo nice an experiment; ſince we are informed, that in the German experiment, commended at the beginning of this letter, the ingenious triers of it found, that their glaſs veſſel, of the capacity of 32 meaſures, was lighter when the air had been drawn out of it, than before, by no leſs than one ounce and $\frac{3}{10}$, that is, an ounce and very near a third. But of the gravity of the air, we may elſewhere have occaſion to make further mention.

TAKING it then for granted, that the air is not devoid of weight, it will not be uneaſy to conceive, that that part of the atmoſphere, wherein we live, being the lower part of it, the corpuſcles, that compoſe it, are very much compreſſed by the weight of all thoſe of the like nature, that are directly over them; that is, of all the particles of air, that being piled up upon them, reach to the top of the atmoſphere. And though the height of this atmoſphere, according to the famous *Kepler*, and ſome others, ſcarce exceeds eight common miles; yet other eminent and later aſtronomers would promote the confines of the atmoſphere to exceed ſix or ſeven times that number of miles. And the diligent and learned *Ricciolo* makes it probable, that the atmoſphere may, at leaſt in divers places, be at leaſt fifty miles high. So that, according to a moderate eſtimate of the thickneſs of the atmoſphere, we may well ſuppoſe, that a column of air, of many miles in height, leaning upon ſome ſpringy corpuſcles of air here below, may have weight enough to bend their little ſprings, and keep them bent: as, (to reſume our former compariſon,) if there were fleeces of wool piled up to a mountainous height one upon another, the hairs, that compoſe the lowermoſt locks, which ſupport the reſt, would, by the weight of all the wool above them, be as well ſtrongly compreſſed, as if a man ſhould ſqueeze them together in in his hands, or employ any ſuch other moderate force to compreſs them. So that we need not wonder, that upon the taking off the incumbent air from any parcel of the atmoſphere here below, the corpuſcles, whereof that undermoſt air conſiſts, ſhould diſplay themſelves, and take up more room than before.

AND if it be objected, that in water, the weight of the upper and of the lower part is the ſame; I anſwer, that, (beſides that it may be well doubted whether the obſervation, by reaſon of the great difficulty, hath been exactly made,) there is a manifeſt diſparity betwixt the air and water: for I have not found, upon an experiment purpoſely made, (and in another treatiſe recorded) that water will ſuffer any conſiderable compreſſion; whereas we may obſerve in wind-guns, (to mention now no other engines) that the air will ſuffer itſelf to be crouded into a comparatively very little room; inſomuch, that a very diligent examiner of the phænomena of wind-guns would have us believe, that in one of them, by condenſation, he reduced the air into a ſpace at leaſt eight times narrower than it before poſſeſſed. And to this, if we add a noble phænomenon of the experiment *de vacuo*; theſe things put together may for the preſent ſuffice to countenance our doctrine. For that noble experimenter, Monſieur *Paſcal* (the ſon) had the commendable curioſity to cauſe the Torricellian experiment to be tried at the foot, about the middle, and at the top of that high mountoin (in *Auvergne*, if I miſtake not) commonly called *Le Puy de Domme*; whereby it was found, that the mercury in the tube fell down lower, about three inches, at the top of the mountain, than at the bottom. And a learned man awhile ſince informed me, that a great Virtuoſo, friend to us both, hath, with not unlike ſucceſs, tried the ſame experiment in the lower and upper parts of a mountain in the weſt of *England*. Of which the reaſon ſeems manifeſtly enough to be this, that upon the tops of high mountains, the air, which bears againſt the reſtagnant quickſilver, is leſs preſſed by the leſs ponderous incumbent air; and conſequently is not able totally to hinder the deſcent of ſo tall and heavy a cylinder of quickſilver, as at the bottom of ſuch mountains did but maintain an æquilibrium with the incumbent atmoſphere.

AND if it be yet further objected againſt what hath been propoſed touching the compactneſs and preſſure of the inferiour air, that we find this very air to yield readily to the motion of little flies, and even to that of feathers, and ſuch other light and weak bodies; which ſeems to argue, that the particles of our air are not ſo compreſſed as we have repreſented them, eſpecially ſince, by our former experiment, it appears, that the air readily dilated itſelf downward, from the receiver into the pump, when it is plain, that it is not the incumbent atmoſphere, but only the ſubjacent air in the braſs cylinder that hath been removed: If this, I ſay, be objected, we may reply, that, when a man ſqueezeth a fleece of wool in his hand, he may feel, that the wool inceſſantly bears againſt his hand, as that which hinders the hairs it conſiſts of, to recover their former and more natural extent. So each parcel of the air about the earth doth conſtantly endeavour to thruſt away all thoſe contiguous bodies, (whether aëreal or more groſs,) that keep it bent, and hinder the expanſion of its parts, which will dilate themſelves, or fly abroad towards that part, (whether upwards or downwards,) where they find their attempted dilatation of themſelves leſs reſiſted by the neighbouring bodies. Thus the corpuſcles of that air we have been all this while ſpeaking of, being unable, by reaſon of their weight, to aſcend above the convexity of the atmoſphere, and by reaſon of the reſiſtance of the ſurface of the earth and water, to fall down lower, they are forced, by their own gravity and this reſiſtance, to expand and diffuſe themſelves about the terreſtrial globe; whereby it comes to paſs, that they muſt as well preſs the contiguous corpuſcles of air, that on either ſide oppoſe their dilatation, as they muſt preſs upon the ſurface of the earth; and, as it were recoiling thence, endeavour to thruſt away thoſe upper particles of air that lean upon them.

And, as for the eaſy yielding of the air to the bodies that move in it, if we conſider, that the corpuſcles, whereof it conſiſts, though of a ſpringy nature, are yet ſo very ſmall, as to make up (which it is manifeſt they do) a fluid body, it will not be difficult to conceive, that in the air, as in other bodies that are fluid, the little bodies it conſiſts of, are in an almoſt reſtleſs motion, whereby they become (as we have more fully diſcourſed in another treatiſe) very much diſpoſed to yield to other bodies, or eaſy to be diſplaced by them; and that the ſame corpuſcles are likewiſe ſo variouſly moved, as they are entire corpuſcles, that if ſome ſtrive to puſh a body placed among them towards the right hand (for inſtance,) others, whoſe motion hath an oppoſite determination, as ſtrongly thruſt the ſame body towards the left; whereby neither of them proves able to move it out of its place, the preſſure on all hands being reduced as it were to an æquilibrium: ſo that the corpuſcles of the air muſt be as well ſometimes conſidered under the notion of little ſprings, which remaining bent, are in their entire bulk tranſported from place to place; as under the notion of ſprings diſplaying themſelves, whoſe parts fly abroad, whilſt, as to their entire bulk, they ſcarce change place: as the two ends of a bow, ſhot off, fly from one another; whereas the bow itſelf may be held faſt in the archer's hand. And that it is the equal preſſure of the air on all ſides upon the bodies that are in it, which cauſeth the eaſy ceſſion of its parts, may be argued from hence; that if by the help of our engine the air be but in great part, though not totally, drawn away from one ſide of a body without being drawn away from the other, he that ſhall think to move that body to and fro, as eaſily as before, will find himſelf much miſtaken.

In a diſcourſe touching Fluidity and Firmneſs.

In verification of which we will, to divert your Lordſhip a little, mention here a phænomenon of our engine, which even to divers ingenious perſons hath at firſt ſight ſeemed very wonderful.

EXPERIMENT II.

THE thing that is wont to be admired, and which may paſs for our ſecond experiment, is this; that if, when the receiver is almoſt empty, a by-ſtander be deſired to lift up the braſs key (formerly deſcribed as a ſtopple in the braſs cover) he will find it a difficult thing to do ſo, if the veſſel be well exhauſted; and even when but a moderate quantity of air hath been drawn out, he will, when he hath lifted it up a little, ſo that it is ſomewhat looſe from the ſides of the lip or ſocket, which (with the help of a little oil) it exactly filled before; he will (I ſay) find it ſo difficult to be lifted up, that he will imagine there is ſome great weight faſtened to the bottom of it. And if (as ſometimes hath been done for merriment) only a bladder be tied to it, it is pleaſant to ſee how men will marvel, that ſo light a body, filled at moſt but with air, ſhould ſo forcibly draw down their hand, as if it were filled with ſome very ponderous thing: whereas the cauſe of this pretty phænomenon ſeems plainly enough to be only this: that the air in the receiver being very much dilated, its ſpring muſt be very much weakened, and conſequently it can but faintly preſs up the lower end of the ſtopple; whereas the ſpring of the external air being no way debilitated, he that a little lifts up the ſtopple, muſt with his hand ſupport a preſſure equal to the diſproportion betwixt the force of the internal expanded air, and that of the atmoſphere incumbent upon the upper part of the ſame key or ſtopple: and ſo men being unuſed to find any reſiſtance, in lifting things up, from the free air above them, they are forward to conclude, that that which depreſſeth their hands muſt needs be ſome weight, though they know not where placed, drawing beneath it.

AND, that we have not miſ-aſſigned the cauſe of this phænomenon, ſeems evident enough by this, that as air is ſuffered by little and little to get into the receiver, the weight, that a man fancieth his hand ſupports, is manifeſtly felt to decreaſe more and more; the internal air by this recruit approaching more to an æquilibrium with the external, till at length the receiver growing again full of air, the ſtopple may be lifted up without any difficulty at all.

BY ſeveral other of the experiments afforded by our engine, the ſame notion of the great and equal preſſure of the free air upon the bodies it environs, might be here manifeſted, but that we think it not ſo fit to anticipate ſuch experiments: and therefore ſhall rather employ a few lines to clear up the difficulty touching this matter, which we have obſerved to have troubled ſome even of the philoſophical and mathematical ſpectators of our engine, who have wondered, that we ſhould talk of the air exquiſitely ſhut up in our receiver, as if it were all one with the preſſure of the atmoſphere; whereas the thick and cloſe body of the glaſs, wholly impervious to the air, doth manifeſtly keep the incumbent pillar of the atmoſphere from preſſing in the leaſt upon the air within the glaſs, which it can no where come to touch. To elucidate a little this matter, let us conſider, that if a man ſhould take a fleece of wool, and having firſt by compreſſing it in his hand reduced it into a narrower compaſs, ſhould nimbly convey and ſhut it cloſe up into a box juſt fit for it; though the force of his hand would then no longer bend thoſe numerous ſpringy bodies that compoſe the fleece, yet they would continue as ſtrongly bent as before, becauſe the box they are incloſed in would as much reſiſt their re-expanding of themſelves, as did the hand that put them in. For thus we may conceive, that the air being ſhut up, when its parts are bent by the whole weight of the incumbent atmoſphere, though that weight can no longer lean upon it, by reaſon it is kept off by the glaſs, yet the corpuſcles of the air within that glaſs continue as forcibly bent, as they were before their incluſion, becauſe the ſides of the glaſs hinder them from diſplaying or ſtretching out themſelves. And if it be objected, that this is unlikely, becauſe even glaſs-bubbles, ſuch as are wont to be blown at the flame of a lamp, exceeding thin, and hermetically ſealed, will not break; whereas it cannot be imagined, that ſo thin a priſon of glaſs could reſiſt the elaſtical force of all the included air, if that air were ſo compreſſed as we ſuppoſe; it may be eaſily replied, that the preſſure of the inward air againſt the glaſs is countervailed by the equal preſſure of the outward againſt the ſame glaſs. And we ſee in bubbles, that by reaſon of this, an exceeding thin film of water is often able, for a good while, to hinder the eruption of a pretty quantity of air. And this may be alſo more conſpicuous in thoſe great ſpherical bubbles, that boys ſometimes blow with water, to which ſoap hath given a tenacity. But that, if the preſſure of the ambient air were removed, the internal air may be able to break thicker glaſſes, than thoſe lately mentioned, will appear by ſome of the following experiments; to which we ſhall therefore now haſten, having, I fear, been but too prolix in this excurſion, though we thought it not amiſs to annex to our firſt experiments ſome general conſiderations touching the ſpring of the air, becauſe (this doctrine being yet a ſtranger to the ſchools) not only we find not the thing itſelf to be much taken notice of; but of thoſe few that have heard of it, the greater part have been forward to reject it, upon a miſtaken perſuaſion, that thoſe phænomena are the effects of nature's abhorrency of a vacuum, which ſeem to be more fitly aſcribable to the weight and ſpring of the air.

EXPERIMENT III.

WE will now proceed to obſerve, that though, by the help of the handle, the ſucker be eaſily drawn down to the bottom of the cylinder; yet, without the help of that leaver, there would be required to the ſame effect a force or weight great enough to ſurmount the preſſure of the whole atmoſphere: ſince otherwiſe the air would not be driven out of its place, when none is permitted to ſucceed into the place deſerted by the ſucker. This ſeems evident from the known Torricellian experiment, in which, if the inverted tube of mercury be but 25 digits high, or ſomewhat more, the quickſilver will not fall, but remain ſuſpended in the tube, becauſe it cannot preſs the ſubjacent mercury with ſo great a force, as doth the incumbent cylinder of the air, reaching thence to the top of the atmoſphere; whereas, if the cylinder of mercury were three or four digits longer, it would over-power that of the external air, and run out into the veſſel'd mercury, till the two cylinders came to an æquilibrium, and no further. Hence we need not wonder, that though the ſucker move eaſily enough up and down in the cylinder by the help of the manubrium; yet if the manubrium be taken off, it will require a conſiderable ſtrength to move it either way. Nor will it ſeem ſtrange, that if, when the valve and ſtop-cock are well ſhut, you draw down the ſucker, and then let go the manubrium; the ſucker will, as it were of itſelf, re-aſcend to the top of the cylinder, ſince the ſpring of the external air findeth nothing to reſiſt its preſſing up the ſucker. And for the ſame reaſon, when the receiver is almoſt evacuated, though, having drawn down the ſucker you open the way from the receiver to the cylinder, and then intercept that way again by returning the key; the ſucker will, upon the letting go the manubrium, be forcibly carried up almoſt to the top of the cylinder; becauſe the air within the cylinder, being equally dilated and weakened with that of the glaſs, is unable to withſtand the preſſure of the external air, till it be driven into ſo little ſpace, that there is an equilibrium betwixt its force and that of the air without. And congruouſly hereunto we find, that in this caſe the ſucker is drawn down with little leſs difficulty, than if the cylinder, being devoid of air, and the ſtop-cock were exactly ſhut. We might take notice of ſome other things, that depend upon the fabric of our engine itſelf; but to ſhun prolixity, we will, in this place, content ourſelves to mention one of them, which ſeems to be of greater moment than the reſt, and it is this: that when the ſucker hath been impelled to the top of the cylinder, and the valve is ſo carefully ſtopped, that there is no air left in the cylinder above the ſucker; if then the ſucker be drawn to the lower part of the cylinder, he that manageth the pump findeth not any ſenſibly greater difficulty to depreſs the ſucker, when it is nearer the bottom of the cylinder, than when it is much farther off. Which circumſtance we therefore think fit to take notice of, becauſe an eminent modern naturaliſt hath taught, that when the air is ſucked out of a body, the violence wherewith it is wont to ruſh into it again, as ſoon as it is allowed to re-enter, proceeds mainly from this; that the preſſure of the ambient air is ſtrengthened upon the acceſſion of the air ſucked out; which, to make itſelf room, forceth the neighbouring air to a violent ſubingreſſion of its parts; which, if it were true, he that draweth down the ſucker, would find the reſiſtance of the external air increaſed as he drawed it lower, more of the diſplaced air being thruſt into it to compreſs it. But, by what hath been diſcourſed upon the firſt experiment, it ſeems more probable, that without any ſuch ſtrengthening of the preſſure of the outward air, the taking quite away or the debilitating of the reſiſtance from within, may ſuffice to produce the effects under conſideration. But this will perhaps be illuſtrated by ſome or other of our future experiments, and therefore ſhall be no longer inſiſted on here.

EXPERIMENT IV.

HAVING thus taken notice of ſome of the conſtant phænomena of our engine itſelf, let us now proceed to the experiments triable in it.

WE took then a lamb's bladder large, well dried, and very limber, and leaving in it about half as much air as it could contain, we cauſed the neck of it to be ſtrongly tied, ſo that none of the included air, though by preſſure, could get out. This bladder being conveyed into the receiver, and the cover luted on, the pump was ſet on work, and after two or three exſuctions of the ambient air (whereby the ſpring of that which remained in the glaſs was weakened) the impriſoned air began to ſwell in the bladder; and, as more and more of the air in the receiver was, from time to time, drawn out; ſo did that in the bladder more and more expand itſelf, and diſplay the folds of the formerly flaccid bladder: ſo that before we had exhauſted the receiver near ſo much as we could, the bladder appeared as full and ſtretched, as if it had been blown up with a quill.

AND that it may appear, that this plumpneſs of the bladder proceeded from the ſurmounting of the debilitated ſpring of the ambient air remaining in the veſſel, by the ſtronger ſpring of the air remaining in the bladder; we returned the key of the ſtop-cock, and by degrees allowed the external air to return into the receiver: whereupon it happened, as was expected, that as the air came in from without, the diſtended air in the bladder was proportionably compreſſed into a narrower room, and the ſides of the bladder grew flaccid, till the receiver having re-admitted its wonted quantity of air, the bladder appeared as full of wrinkles and cavities as before.

THIS experiment is much of the ſame nature with that, which was, ſome years ago, ſaid to be made by that eminent geometrician Monſieur *Roberval*, with a carp's bladder emptied and conveyed into a tube, wherein the experiment *de vacuo* was afterwards tried; which ingenious experiment of his juſtly deſerveth the thanks of thoſe, that have been, or ſhall be, ſollicitous to diſcover the nature of the air.

BUT to return to our experiment; we may take notice of this circumſtance in it, that after the receiver hath been in ſome meaſure emptied, the bladder doth, at each exſuction, ſwell much more conſpicuouſly than it did at any of the firſt exſuctions; inſomuch that towards the end of the pumping, not only a great fold or cavity, in the ſurface of the bladder may be made even, by the ſtretching of the inward ſelf-expanding air; but we have ſometimes ſeen, upon the turning of the key to let the ambient air paſs out of the receiver into the cylinder; we have ſeen (I ſay) the air in the bladder ſuddenly expand itſelf ſo much and ſo briſkly, that it manifeſtly lifted up ſome light bodies that leaned upon it, and ſeemed to lift up the bladder itſelf.

WE let down into the receiver with the forementioned bladder two other much ſmaller, and of the ſame kind of animal; the one of theſe was not tied up at the neck, that there might be liberty left to the air that was not ſqueezed out (which might amount to about a fifth part of what the bladder held before) to paſs out into the receiver; the other had the ſides of it ſtretched out and preſſed together, almoſt into the form of a cup, that they might intercept the leſs air betwixt them, and then was ſtrongly tied up to the neck. This done, and the air being in ſome meaſure ſucked out of the pneumatical glaſs (if I may ſo call it) the bladder, mentioned at the beginning of our experiment, appeared extended every way to its full dimenſions; whereas neither of the two others did remarkably ſwell, and that, whoſe neck was not tied, ſeemed very little, if at all leſs wrinkled, than when it was put in.

WE made likewiſe a ſtrong ligature about the middle of a long bladder partly emptied, and, upon the drawing the air out of the receiver, could obſerve no ſuch ſwelling betwixt the ligature and the neck of the bladder, which had been purpoſely left open, as betwixt the ſame ligature and the bottom of the bladder, whence the included air could no way get out.

BUT a farther and ſufficient manifeſtation, whence the intumeſcence of the bladder proceeds, may be deduced from the following experiment.

EXPERIMENT V.

TO try then at once both what it was that expanded the bladder, and what a powerful ſpring there is even in the air we are wont to think uncompreſſed; we cauſed a bladder dry, well tied and blown moderately full, to be hung in the receiver by one end of a ſtring, whoſe other end was faſtened to the inſide of the cover: and upon drawing out the ambient air that preſſed on the bladder; the internal air not finding the wonted reſiſtance, firſt ſwelled and diſtended the bladder, and then broke it, with ſo wide and crooked a rent, as if it had been forcibly torn aſunder with hands. After which a ſecond bladder being conveyed in, the experiment was repeated with like ſucceſs: and I ſuppoſe it will not be imagined, that in this caſe the bladder was broken by its own fibres, rather than by the impriſoned air.

AND of this experiment theſe two phænomena may be taken notice of: the one, that the bladder at its breaking gave a great report, almoſt like a cracker; and the other, that the air contained in the bladder had the power to break it with the mentioned impetuoſity, long before the ambient air was all, or near all, drawn out of the receiver.

BUT, to verify what we ſay in another diſcourſe, where we ſhow, that even true experiments may, by reaſon of the eaſy miſtake of ſome unheeded circumſtance, be unſucceſsfully tried; we will advertiſe, on this occaſion, that we did oftentimes in vain try the breaking of bladders, after the manner above mentioned; of which the cauſe appeared to be this, that the bladders we could not break, having been brought us already blown from thoſe that ſold them, were grown dry before they came to our hands: whence it came to paſs, that, if we afterwards tied them very hard, they were apt to fret, and ſo become unſerviceable; and if we tied them but moderately hard, their ſtiffneſs kept them from being cloſed ſo exactly, but that when the included air had in the exhauſted receiver diſtended them as much as eaſily it could, it would in part get out between the little wrinkles of the ſphincter of the neck. Whence alſo it uſually happened, that upon the letting in the air from without, the bladders appeared more flaccid and empty than before they were put in; whereas when the bladders were brought us moiſt from the butchers, we could, without injuring them, tie their necks ſo cloſe, that none of the air, once blown in, could get out of them, but by violently breaking them.

IT would not be amiſs on this occaſion to point at ſomething, which may deſerve a more deliberate ſpeculation than we can now afford it; namely, that the elaſtical power of the ſame quantity of air may be as well increaſed by the agitation of the aëreal particles (whether only moving them more ſwiftly and ſcattering them, or alſo extending or ſtretching them out, I determine not) within an every way incloſing and yet yielding body; as diſplayed by the withdrawing of the air that preſſed it without. For we found, that a bladder but moderately filled with air and ſtrongly tied, being a while held near the fire, not only grew exceeding turgid and hard, but afterwards, being brought nearer to the fire, ſuddenly broke with ſo loud and vehement a noiſe, as ſtunned thoſe that were by, and made us for a while after almoſt deaf.

EXPERIMENT VI.

HAVING thus ſeen, that the air hath an elaſtical power, we were next deſirous to know in ſome meaſure, how far a parcel of air might by this its own ſpring be dilated. And though we were not provided of inſtruments fit to meaſure the dilatation of the air any thing accurately, yet becauſe an imperfect meaſure of it was more deſirable than none at all, we deviſed the following method as very eaſily practicable.

We took a limber lamb's bladder, which was thoroughly wetted in fair water, that the ſides of it being ſqueezed together, there might be no air left in its folds; (as indeed we could not afterwards upon trial diſcern any.) The neck of this bladder was ſtrongly tied about that of a ſmall glaſs, (capable of holding five full drachms of water) the bladder being firſt ſo compreſſed, that all the included air was only in the glaſs, without being preſſed there. Then the pump being ſet on work, after a few exſuctions, the air in the little phial began to dilate itſelf, and produce a ſmall tumour in the neck of the bladder; and as the ambient air was more and more drawn away, ſo the included air penetrated farther and farther into the bladder, and by degrees lifted up the ſides and diſplayed its folds, till at length it ſeemed to have blown it up to its full extent: whereupon the external air, being permitted to flow back into the receiver, repulſed the air that had filled the bladder into its former narrow receptacle, and brought the bladder to be again flaccid and wrinkled as before. Then taking out the bladder, but without ſevering it from the glaſs, we did, by a hole made at the top of the bladder, fill the veſſel, they both made up, with water, whoſe weight was five ounces five drachms and a half: five drachms whereof were above mentioned to be the contents of the bottle. So that in this experiment, when the air had moſt extended the bladder, it poſſeſſed in all above nine times as much room as it did when it was put into the receiver. And it would probably have much enlarged its bounds, but that the bladder by its weight, and the ſticking together of its ſides, did ſomewhat reſiſt its expanſion: and, which was more conſiderable, the bladder appeared tumid enough, whilſt yet a pretty deal of air was left in the receiver, whoſe exſuction would, according to our former obſervation, probably have given way to a farther expanſion of the air, eſpecially ſuppoſing the dilatation not to be reſtrained by the bladder.

Since we wrote the other day the former experiment, we have met with ſome glaſſes not very unfit for our purpoſe; by means of which we are now able, with a little more trouble, to meaſure the expanſion of the air a great deal more accurately than we could by the help of the above-mentioned bladder, which was much too narrow to allow the air its utmoſt diſtenſion.

We took then firſt a cylindrical pipe of glaſs, whoſe bore was about a quarter of an inch in diameter; this pipe was ſo bent and doubled, that, notwithſtanding its being about two feet in length, it might have been ſhut up into a ſmaller receiver, not a foot high; but by misfortune it cracked in the cooling, whereby we were reduced to make uſe of one part which was ſtraight and entire, but exceeded not ſix or ſeven inches. This little tube was open at one end; and at the other, where it was hermetically ſealed, had a ſmall glaſs bubble to receive the air whoſe dilatation was to be meaſured.

ALONG the ſide of this tube was paſted a ſtraight narrow piece of parchment, divided into twenty-ſix equal parts, marked with black lines and figures, that by them might be meaſured both the included air and its dilatation. Afterwards we filled the tube with water almoſt to the top; and ſtopping the open end with the finger, and inverting the tube, the air was permitted to aſcend to the above-mentioned glaſs bubble. And by reaſon this aſcent was very ſlow, it gave us the opportunity to mark how much more or leſs than one of the twenty-ſix diviſions this air took up. By this means, after a trial or two, we were enabled to convey to the top of the glaſs a bubble of air equal enough, as to ſight, to one of thoſe diviſions. Then the open end of the tube being put into a ſmall phial, whoſe bottom was covered with water, about half an inch high; we included both glaſſes into a ſmall and ſlender receiver, and cauſed the pump to be ſet on work. The event was, that at the firſt exſuction of the air there appeared not any expanſion of the bubble, comparable to what appeared at the ſecond, and that upon a very few exſuctions the bubble reaching as low as the ſurface of the ſubjacent water, gave us cauſe to think, that if our pipe had not been broken, it would have expanded itſelf much farther: wherefore we took out the little tube, and found, that beſides the twenty-ſix diviſions formerly mentioned, the glaſs bubble and ſome part of the pipe, to which the divided parchment did not reach, amounted to ſix diviſions more. Whereby it appears, that the air hath taken up one and thirty times as much room as before, and yet ſeemed capable of a much greater expanſion, if the glaſs would have permitted it. Wherefore after the former manner, we let in another bubble, that by our gueſs was but half as big as the former, and found, that upon the exſuction of the air from the receiver, this little bubble did not only fill up the whole tube, but (in part) break through the ſubjacent water in the phial, and thereby manifeſt itſelf to have poſſeſſed ſixty and odd times its former room.

THESE two experiments are mentioned to make way for the more eaſy belief of that which is now to follow. Finding then that our tube was too ſhort to ſerve our turn, we took a ſlender quill of glaſs, which happened to be at hand, though it were not ſo fit for our purpoſe as we could have wiſhed, in regard it was three or four times as big at one end as the other. This pipe, which was thirty inches long, being hermetically ſealed at the ſlender end, was almoſt filled with water; and after the above-related manner a bubble was conveyed to the top of it, and the open extreme was put into a phial that had a little fair water at the bottom: then the cover, by means of a ſmall hole purpoſely made in it for the glaſs pipe to ſtand out at, was cemented on to the receiver, and the pump being ſet on work, after ſome exſuctions, not only the air manifeſtly appeared extended below the ſurface of the ſubjacent water, but one of the byſtanders affirms, that he ſaw ſome bubbles come out at the bottom of the pipe, and break through the water. This done, we left off pumping, and obſerved how at the unperceived leaks of the receiver the air got in ſo faſt, that it very quickly impelled up the water to the top of the tube, excepting a little ſpace whereunto that bubble was repulſed, which had ſo lately poſſeſſed the whole tube. This air at the ſlender end appeared to be a cylinder of $\frac{5}{6}$ parts of an inch in length, but when the pipe was taken out and turned upſide down, it appeared at the other end inferior in bulk to a pea.

THESE things being thus done, we took (to make the experiment the more exactly) a ſmall pair of ſcales, ſuch as goldſmiths uſe to weigh gold coin in; and weighing the tube and water in it, we found them to amount to one ounce thirty grains and an half: then we poured in as much water as ſerved to fill up the tube, wherein before we had left as much ſpace unfilled up as was poſſeſſed by the bubble; and weighing again the pipe and water, we found the weight increaſed only by one grain. Laſtly, pouring out the water, and carefully freeing the pipe from it, (which yet we could not perfectly do) we weighed the glaſs alone, and found it to want two drachms and thirty-two grains of its former weight: ſo that the bubble of air taking up the room but of one grain in weight of water, it appeared, that the air by its own ἐλατὴρ was ſo rarified, as to take up one hundred fifty-two times as much room as it did before; though it were then compreſſed by nothing but the ordinary preſſure of the contiguous air. I know not whether it be requiſite to take notice, that this experiment was made indeed in a moiſt night, but in a room, in whoſe chimney there was burning a good fire, which did perhaps ſomewhat rarify the air, of which the bubble conſiſted.

IT hath ſeemed almoſt incredible, which is related by the induſtrious *Merſennus*, that the air, by the violence of the heat, though as great as our veſſels can ſupport without fuſion, can be ſo dilated as to take up ſeventy times as much room as before: wherefore becauſe we were willing to have a confirmation of ſo ſtrange a phænomenon, we once more conveyed into the tube a bubble of the bigneſs of the former, and proſecuting the experiment as before with the ſame water, we obſerved, that the air did manifeſtly ſtretch itſelf ſo far, as to appear ſeveral times a good way below the ſurface of the water in the phial, and that too with a ſurface very convex toward the bottom of the pipe. Nay, the pump being plied a little longer, the air did manifeſtly reach to that place, where the bottom of the tube leaned upon the bottom of the phial, and ſeemed to knock upon it and rebound from it: which circumſtances we add, partly that the phænomenon we have been relating may not be imputed to the bare ſubſiding of the water that filled the tube, upon the taking off the preſſure of ambient air; and partly alſo, that it may appear, that if our experiments have not been ſo accurately made as with fitter inſtruments might perhaps be poſſible, yet the expanſion of the air is likely to be rather greater than leſſer than we have made it; ſince the air was able to preſs away the water at the bottom of the pipe, though that were about two inches below the ſurface of the water that was then in the phial, and would have been at leaſt as high in the pipe, if the water had only ſubſided and not been depreſſed: ſo that it ſeems not unlikely, that if the experiment could be ſo made, as that the expanſion of the air might not be reſiſted by the neighbouring bodies, it would yet enlarge its bounds, and perhaps ſtretch itſelf to two hundred times its former bulk, if not more. However, what we have now tried will I hope, ſuffice to hinder divers of the phænomena of our engine from being diſtruſted: ſince in that part of the atmoſphere we live in, that which we call the free air (and preſume to be ſo uncompreſſed) is crouded into ſo very ſmall a part of that ſpace which, if it were not hindered, it would poſſeſs. We would gladly have tried alſo, whether the air at its greateſt expanſion could be farther rarified by heat; but, do what we could, our receiver leaked too faſt to let us give ourſelves any ſatisfaction in that particular.

EXPERIMENT VII.

TO diſcover likewiſe, by the means of that preſſure of the air, both the ſtrength of glaſs, and how much intereſt the figure of a body may have in its greater or leſſer reſiſtance, to the preſſure of other bodies, we made theſe farther trials.

WE cauſed to be blown with a lamp a round glaſs bubble, capable of containing, by gueſs, about five ounces of water, with a ſlender neck about the bigneſs of a ſwan's quill; and it was purpoſely blown very thin, as phials made with lamps are wont to be, that the thinneſs of the matter might keep the roundneſs of the figure from making the veſſel too ſtrong. Then having moderately emptied the receiver, and taken it out of the pump, we ſpeedily applied to the orifice of the bottom of it, the neck of the newly mentioned glaſs, carefully ſtopping the crannies with melted plaiſter, that no air might get in at them: and after turning the key of the ſtop-cock, we made a free paſſage for the air to paſs out of the bubble into the receiver; which it did with great celerity, leaving the bubble as empty as the receiver itſelf, as appeared to us by ſome circumſtances not now to be inſiſted on. Notwithſtanding all which, the veſſel, continuing as entire as before, gave us cauſe to wonder that the bare roundneſs of the figure ſhould enable a glaſs, almoſt as thin as paper, to reſiſt ſo great a preſſure as that of the whole incumbent atmoſphere. And having reiterated the experiment, we found again, that the preſſure of the ambient body, thruſting all the parts inwards, made them, by reaſon of their arched figure, ſo ſupport one another, that the glaſs remained as whole as at firſt.

Now that the figure of the glaſs is of great moment in this matter, may be evinced by this other experiment.

EXPERIMENT VIII.

WE took a glaſs helmet or alembick (delineated by the ſeventh figure) ſuch as chymiſts uſe in diſtillations, and containing by conjecture between two and three pints: the roſtrum or roſe of it marked with *c* was hermetically cloſed; and at the top of it was a hole, into which was fitted and cemented one of the ſhanks of a middle-ſized ſtop-cock; ſo that the glaſs being turned upſide-down, the wide orifice (which in common glaſs-helmets is the only one) was upwards; and to that wide orifice was fitted a caſt cover of lead, which was carefully cemented on to the glaſs: then the other ſhank of the ſtop-cock being with cement likewiſe faſtened into the upper part of the pump, the exſuction of the air was endeavoured. But it was not long before, the remaining air being made much too weak to balance the preſſure of the ambient air, the glaſs was (not without a great noiſe) cracked almoſt half round, along that part of it where it began to bend inwards; as if in the figure the crack had been made according to the line *a b*. And upon an endeavour to pump out more of the air, the crack once begun, appeared to run on farther; though the glaſs where it was broken ſeemed to be (by conjecture) above ten, ſome thought above twenty times, as thick as the bubble mentioned in the foregoing experiment.

THIS will perhaps make it ſeem ſtrange, that having taken another glaſs bubble blown at the ſame time, and like, for aught we diſcerned for ſize, thickneſs, and figure, to that thin one formerly mentioned; and having ſealed it up hermetically, and ſuſpended it in the receiver, the exſuction of the ambient air did not enable the impriſoned air to break, or in the leaſt to crack the bubble; though the experiment were laboriouſly tried, and that ſeveral times with bubbles of other ſizes: but that perhaps the heat of the candle or lamp wherewith ſuch glaſſes are hermetically ſealed, (not to mention the warmth of his hands that ſealed it) might ſo rarify the contained air, as much to weaken its ſpring, may ſeem probable by the following experiments.

EXPERIMENT IX.

WE took a glaſs phial able to hold three or four ounces of water, and of the thickneſs uſual in glaſſes of that ſize; into the neck of this was put a moderately ſlender pipe of glaſs, which was carefully faſtened, with a mixture of equal parts of pitch and roſin, to the neck of the phial, and which reached almoſt to the bottom of it, as the ſixth figure declareth.

THIS phial being, upon a particular deſign, filled with water, till that came up in it a pretty deal higher than the lower end of the pipe, was put into one of our ſmall receivers, (containing between a pint and a quart) in ſuch manner as that the glaſs pipe, paſſing through a hole made purpoſely for it in the leaden cover of the receiver, was for the moſt part of it without the veſſel, which being exactly cloſed, the pump was ſet on work: but at the very firſt exſuction, and before the ſucker was drawn to the bottom of the cylinder, there flew out of the phial a piece of glaſs half as broad as the palm of a man's hand, and it was thrown out with ſuch violence, that hitting againſt the neighbouring ſide of the receiver, it not only daſhed itſelf to pieces, but cracked the very receiver in many places, with a great noiſe that much ſurpriſed all that were in the room. But it ſeemed, that in ſo little a receiver, the air about the phial being ſuddenly drawn out, the air impriſoned in the veſſel, having on it the whole preſſure of the atmoſphere (to which by the pipe open at both ends, it and the water were expoſed) and not having on the other ſide the wonted preſſure of the ambient air to balance that other preſſure, the reſiſtance of glaſs was finally ſurmounted, and the phial once beginning to break where it was weakeſt, the external air might ruſh in with violence enough to throw the cracked parcel ſo forcibly againſt the neighbouring ſide of the receiver, as to break that too.

AND this may be preſumed ſufficient to verify what we delivered in that part of our appendix to the firſt experiment, where we mentioned the almoſt equal preſſure of the air on either ſide of a thin glaſs veſſel, as the cauſe of its not being broken by the forcible ſpring of the contained air. But yet that it be not ſuſpected, that chance had an intereſt in ſo odd an experiment as we have been relating, we will add, that, for farther ſatisfaction, we reiterated it in a round glaſs containing by gueſs about ſix ounces of water: this phial we put into ſuch a ſmall receiver as was lately mentioned, in ſuch manner, as that the bottom of it reſted upon the lower part of the pneumatical glaſs, and the neck came out through the leaden cover of the ſame at a hole made purpoſely for it. But being made circumſpect by the foregoing miſchance, we had put the phial into a bladder, before we put it into the receiver to hinder this laſt-named glaſs from being endangered by the breaking of the other. Then the pneumatical veſſel being cloſed, ſo that no way was left for the outward air to get into it, but by breaking through the phial, into whoſe cavity it had free acceſs by the mouth of it, (which was purpoſely left open,) the ſucker being nimbly drawn down, the external air immediately preſſed forcibly as well upon the leaden-cover as the phial; and the cover happening to be in one place a little narrower than the edge of the pneumatical glaſs, was depreſſed, and thruſt into it ſo violently by the incumbent air, that getting a little within the tapering lip of the glaſs, it did like a kind of wedge) thruſt out that ſide where it was depreſſed, ſo as, (though the receiver was new) to ſplit it. This accident being thus mentioned upon the bye to confirm what we formerly ſaid touching the fitneſs or unfitneſs of glaſſes of ſome figures to reſiſt the preſſure of the atmoſphere; we will proceed to relate the remaining part of the experiment: namely, that having fitted on a wider cover to the ſame receiver, and cloſed both that and the crack with cement, we proſecuted the experiment in the manner above related, with this ſucceſs; that upon the quick depreſſing of the ſucker, the external air burſt the body of the phial into above an hundred pieces, many of them exceeding ſmall, and that with ſuch violence, that we found a wide rent, beſides many holes, made in the bladder itſelf.

AND to evince, that theſe phænomena were the effects of a limited and even moderate force, and not of ſuch an abhorrency of a vacuum, as that to avoid it, many have been pleaſed to think, that nature muſt, upon occaſion, exerciſe an almoſt boundleſs power; we afterwards purpoſely tried this experiment with ſeveral glaſſes ſomewhat thicker than thoſe phials, and found the event to verify our conjecture, that it would not ſucceed; for the glaſſes were taken out as entire as they were put in.

AND here, my Lord, I hold it not unfit, upon occaſion of the mention that hath been made of our having employed ſmall receivers, and one of them, notwithſtanding its being cracked, to annex theſe two advertiſements.

FIRST then, beſides the great pneumatical glaſs ſo often mentioned, and the proportionate ſtop-cock, we thought fit to provide ourſelves with ſome ſmall receivers, blown of cryſtalline glaſs, of ſeveral ſhapes, and furniſhed with ſmaller ſtop-cocks purpoſely made; and this we did upon hopes, that when we had ſurmounted the difficulties to be met with in cementing the glaſſes to the ſtop-cocks, and the pneumatical veſſels to the pump, ſo exquiſitely as is requiſite for our purpoſe, we ſhould from the ſmallneſs of our receivers receive a four-fold advantage. The firſt, that by reaſon of the ſlenderneſs of the veſſels, and their being made of much purer and clearer metal (as the glaſs-men ſpeak) than the great receiver, we might have a more perfect view of every thing happening within them. The next, that ſuch ſmall veſſels might be emptied with leſs labour and in much leſs time. The third, that this nimble exſuction of the ambient air would make many changes in the bodies ſhut up in theſe glaſſes more ſudden and conſpicuous, than otherwiſe they would prove. And the laſt, that we ſhould be able to draw and keep out the air much more perfectly from ſuch ſmall veſſels than from our large receiver: but though we were not much diſappointed in the expectation of the three firſt advantages, yet we were in our hopes of the fourth; for beſides the great difficulty we found in fitting together the glaſſes, the ſtop-cocks, and the covers; beſides this, I ſay, we found ourſelves ſeldom able to draw, and keep out the air ſo far as to make the remaining air in theſe receivers weaker than the remaining air in our great receiver. For though ſometimes the leaks of ſome of theſe little receivers may be much (either fewer or ſmaller) than thoſe of the larger veſſel; yet a little air getting into one of theſe, wherein it had but litttle room to expand and diſplay itſelf, might preſs as much upon all parts of the internal ſurface of the veſſel, and upon the included bodies, as a greater quantity of the air in a veſſel in whoſe capacity it might find more room to expand itſelf.

THE other thing, that we were to advertiſe, is, that, it is not every ſmall crack that can make ſuch a receiver as is of a roundiſh figure altogether uſeleſs to our experiment, in regard that upon the exſuction of the internal air, the ambient air on all ſides preſſing the glaſs inwards or towards the middle, doth conſequently thruſt the lips of the crack cloſer, and ſo rather cloſe than increaſe it.

THIS I mention partly becauſe receivers fit for our turn are more eaſily cracked than procured, and therefore ought not to be unneceſſarily thrown away as unſerviceable; and partly, becauſe I think it becomes one, that profeſſeth himſelf a faithful relator of experiments not to conceal from your lordſhip, that after a few of the foregoing experiments were made, there happened in the great receiver a crack of about a ſpan long, beginning at the upper orifice, and occaſioned, as it ſeemed, by the exceſſive heat of too large an iron that was employed to melt the cement about that orifice. But having laid upon this crack a broad plaiſter, (which in one of our eſſays, written ſome years ſince, to your ingenious and hopeful couſin *Jones*, we extol for the mending of cracked receivers, and other chymical glaſſes;) and having afterwards thickly overlaid this plaiſter with diachylon, we neither could then, nor can yet perceive, that the veſſel leaks ſenſibly at that crack.

THE plaiſter was made of good quick lime finely poudered, and nimbly ground with a peſtle in a mortar, with a quantity (I know not how much preciſely, not having thoſe eſſays in this place) of ſcrapings of cheeſe and a little fair water, no more than is juſt neceſſary to bring the mixture to a ſomewhat ſoft paſte, which, when the ingredients are exquiſitely incorporated, will have a ſtrong and ſtinking ſmell: then it muſt be immediately ſpread upon a linen cloth of three or four fingers breadth, and preſently applied, leſt it begin to harden. But if your Lordſhip had ſeen, how we mended with it receivers even for the moſt ſubtle chymical ſpirits, you would ſcarce wonder at the ſervice it hath done in our pneumatical glaſs.

EXPERIMENT X.

WE took a tallow-candle of ſuch a ſize, that eight of them make about a pound; and having in a very commodious candleſtick let it down into the receiver, and ſo ſuſpended it, that the flame burnt almoſt in the middle of the veſſel, we did in ſome two minutes exactly cloſe it up: and upon pumping very nimbly, we found, that, within little more than half a minute after, the flame went out, though the ſnuff had been purpoſely left of that length we judged the moſt convenient for the laſting of the flame.

BUT the ſecond time having put in the ſame candle into the receiver (after it had by the blaſts of a pair of bellows been freed from fumes) the flame laſted about two minutes from the time the pumper began to draw out the air; upon the firſt exſuction whereof, the flame ſeemed to contract it ſelf in all its dimenſions. And theſe things were farther obſervable, that after the two or three firſt exſuctions of the air, the flame (except at the very top) appeared exceeding blue, and that the flame ſtill receded more and more from the tallow, till at length it appeared to poſſeſs only the very top of the wick, and there it went out.

THE ſame candle being lighted again was ſhut into the receiver, to try how it would laſt there without drawing forth the air, and we found, that it laſted much longer than formerly; and before it went out, receded from the tallow towards the top of the wick, but not near ſo much as in the former experiment.

AND having an intention to obſerve particularly, what the motion of the ſmoke would be in theſe experiments, we took notice, that when the air was not drawn out, there did, upon the extinction of the flame, a conſiderable part of the wick remain kindled, which (probably by reaſon of the circulation of the air in the veſſel, occaſioned by the heat) emitted a ſteam, which aſcended ſwiftly and directly upwards in a ſlender and uninterrupted cylinder of ſmoke, till it came to the top, whence it manifeſtly recoiled by the ſides to the lower part of the veſſel. Whereas when the flame went out upon the exſuction of the air one time (when the flame retired very leiſurely to the top) we perceived it not to be followed by any ſmoke at all. And at another time the upper part of the wick remaining kindled after the extinction of the flame, the ſlender ſteam of fumes that did ariſe aſcended but a very little way, and then after ſome uncertain motions this and that way, did, for the moſt part, ſoon fall downwards.

BEING deſirous alſo to try, whether there would be any difference as well in our receiver, as there is wont to be elſewhere betwixt candles made of wax and thoſe made of tallow, as to their duration; we took ſlender tapers of white wax, (commonly called virgins wax) that being found to burn with much leſs ſmoke than common yellow wax. Six of theſe of like bigneſs, and each of them of about the thickneſs of a ſwan's quill, we preſſed together into one candle: and having lighted all the wicks, we let in the above-mentioned wax-candle into the receiver, and made what haſte we could to cloſe it up with cement. But, though in the mean while we left open the valve of the cylinder, the hole of the ſtop-cock, and that in the cover of the receiver, that ſome air might get in to cheriſh the flame, and the ſmoke might have a vent; yet for ſo great a flame the air ſufficed not ſo much as till the cover ſhould be perfectly luted on: ſo that before we were quite ready to imploy the pump, the candle was extinguiſhed. Wherefore we took but one of the above-mentioned tapers, and having lighted it, cloſed it up in the receiver, to try how long a ſmall flame with a proportionable ſmoke would continue in ſuch a quantity of air: but we found upon two ſeveral trials, that from the beginning of pumping, the flame went out in about a minute of an hour. It appeared indeed to us, that the ſwinging of the wire to and fro (in the engine ſhaken by pumping) haſtened the vaniſhing of the flame, which ſeemed by that motion to be caſt ſometimes on one ſide of the wick and ſometimes on the other. But though once we purpoſely refrained pumping after a very few exſuctions of the air, that the flame might not be agitated, yet it laſted not much longer than the newly mentioned time. And laſtly, cloſing up the ſame taper, lighted again, to diſcover how long it would laſt without drawing out of the air, we found that it burnt for a while vividly enough, but afterwards began to be leſſened more and more in all its dimenſions. And we obſerved, that the flame did not, as before, retire itſelf by little and little towards the top, but towards the bottom of the wick (from which yet it did a little withdraw upwards juſt before it went out) ſo that the upper part of the wick appeared for a pretty while manifeſtly above the top of the flame, which having laſted about five minutes, was ſucceeded by a directly aſcending ſtream of ſmoke.

EXPERIMENT XI.

THERE was taken a wire, which being bent almoſt in the form of a ſcrew, conſtituted ſuch an inſtrument, to contain coals, and leave them every way acceſſible to the air, as the tenth figure declareth; the breadth of this veſſel was no leſs than that it might with eaſe be conveyed into the receiver: and having filled it to the height of about five inches with thoroughly kindled wood-coals, we let it down into the glaſs; and ſpeedily cloſing it, we cauſed the pumper to ply his work, and obſerved, that upon the very firſt exſuction of the air (though perhaps not becauſe of that only) the fire in the coals began to grow very dim, and though the agitation of the veſſel did make them ſwing up and down (which in the free air would have retarded the extinction of the fire) yet when we could no longer diſcern any redneſs at all in any of them, caſting our eyes upon a minute watch we kept by us on this occaſion, we found, that from the beginning of the pumping (which might be about two minutes after the coals had been put in glowing) to the total diſappearing of the fire, there had paſſed but three minutes.

WHEREUPON, to try the experiment a little farther, we preſently took out the coals, in which it ſeems there had remained ſome little parcels of fire, rather covered than totally quenched: for in the open air the coals began to be re-kindled in ſeveral places, wherefore having by ſwinging them about in the wire, throughly lighted them the ſecond time, we let them down again into the receiver, and cloſed it ſpeedily as before; and then waiting till the fire ſeemed totally extinct, without meddling with the pump, we found that from the time the veſſel was cloſed, till that no fire at all could be perceived, there had paſſed about four minutes: whereby it ſeemed to appear, that the drawing away of the ambient air made the fire go out ſooner than otherwiſe it would have done; though that part of the air that we drew out, left the more room for the ſtifling ſtreams of the coals to be received into.

LASTLY, having taken out the wire and put other coals into it, we did in the ſame room where the engine ſtood, let it hang quietly by a ſtring in the open air, to try how long the fire would laſt without agitation, when no air was kept from it; and we found, that the fire began to go out firſt at the top and outſide of the coals, but inwards and near the bottom the fire continued viſible for above half an hour; a great part of the coals, eſpecially thoſe next the bottom, being burnt to aſhes before the fire went out.

WE cauſed likewiſe a piece of iron to be forged, of the bigneſs of a middle-ſized charcoal, and having made it red-hot throughout; we cauſed it in the lately mentioned wire, to be ſpeedily conveyed and ſhut up into the receiver, being deſirous to try what would become of a glowing body, by reaſon of its texture more vehemently hot than a burning coal of the ſame bigneſs, and yet unlike to ſend forth ſuch copious and ſtifling fumes: but we could not obſerve any manifeſt change upon the exſuction of the air. The iron began indeed to loſe its fiery redneſs at the top, but that ſeemed to be becauſe it was at the upper end ſomewhat more ſlender than at the lower: the redneſs, though it were in the day-time, continued viſible about four minutes; and then, before it did quite diſappear, we turned the key of the ſtop-cock, but could not diſcern any change of the iron upon the ruſhing in of the air. Yet ſome little remainders of wax that ſtuck to the wire, and were turned into fumes by the heat of the neighbouring iron, ſeemed to afford a more plentiful, or at leaſt a much more expanded ſmoke, when the air was ſucked out, than afterwards, though allowance was made for the decreaſing heat of the iron. And laſtly, notwithſtanding a conſiderable exſuction of the ambient air, though not by far ſo great a one as might have been made by the engine; and notwithſtanding the inconſiderable diſſipation of the parts of the iron; the ſurrounding ſides of the receiver were ſenſibly, and almoſt offenſively heated by it; inſomuch that a pretty while after the iron was taken out, the ſides of the glaſs manifeſtly retained a warmth: which would not be unfit to be conſidered by a perſon at more leiſure than I am now.

EXPERIMENT XII.

BEING willing to try after this ſomething, that would not cheriſh much fire at once, and would keep fire much longer than a coal; we took a piece of match, ſuch as ſoldiers uſe, of the thickneſs of a man's little finger, or ſomewhat thicker; and this being well lighted at one end, was by a ſtring ſuſpended with that end downwards in the cavity of the receiver, which was immediately cloſed: and yet by that time it could well be ſo, the copious fumes of the match had near filled and darkened the receiver. Wherefore, leſt the veſſel ſhould be endangered, the pump was nimbly plied, and a great deal of air and ſmoke mixt together was drawn out, whereby the receiver growing more clear, we could diſcern the fire in the match to burn more and more languidly: and notwithſtanding that by the diligence uſed in pumping, it ſeemed to have room enough allowed it to throw out fumes; yet, after no long time, it ceaſed from being diſcernible either by its light or its ſmoke. And, though by that we were invited to ſuppoſe it quite extinguiſhed, yet we continued pumping a-while, in proſecution of another experiment we were trying at the ſame time: and this we did the more willingly, becauſe of a ſuſpicion the experiment about the coals might eaſily ſuggeſt, and which the event declared not to have been altogether groundleſs. For upon the admiſſion of the external air, the fire, that ſeemed to have gone out a pretty while before, did preſently revive; and being as it were refreſhed by the new air, and blown by the wind made by that air in ruſhing in, it began again to ſhine and diſſipate the neighbouring fuel into ſmoke as formerly.

EXPERIMENT XIII.

AWHILE after we let down into the receiver, together with a lighted piece of match, a great bladder well tied at the neck, but very lank, as not containing actually much (if any thing) above a pint of air, but being capable of containing ten or twelve times as much.

OUR ſcope in this experiment was partly to try, whether or no the ſmoke of the match, repleniſhing the receiver, would be able to hinder the dilatation of the inward air, upon the exſuction of the ambient; and partly to diſcover, whether the extinction of the fire in the match did proceed from want of air, or barely from the preſſure of its own fumes, which, for want of room to expand themſelves in, might be ſuppoſed to recoil upon the fire, and ſo to ſtifle it.

THE event of our trial was, that at the beginning of our pumping the match appeared well lighted, though it had almoſt filled the receiver with its plentiful fumes: but by degrees it burnt more and more dimly, notwithſtanding, that by the nimble drawing out the air and ſmoke, the veſſel were made leſs opacous, and leſs full of compreſſing matter; as appeared by this, that the longer we pumped, the leſs air and ſmoke came out of the cylinder at the opening of the valve, and conſequently the leſs came into it before; yet the fire in the match went but ſlowly out. And when afterwards, to ſatisfy ourſelves of its expiration, we had darkened the room, and in vain endeavoured to diſcover any ſpark of fire, as we could not for ſome time before by the help of candles diſcern the leaſt riſing of ſmoke) we yet continued pumping ſix or ſeven times; and after all that, letting in the air, the ſeemingly dead fire quickly revived, and manifeſted its recovery by light and ſtore of ſmoke, with the latter of which it quickly began to repleniſh the receiver. Then we fell to pumping afreſh, and continued that labour ſo long, till the re-kindled match went out again: and thinking it then fit not to ceaſe from pumping ſo ſoon as before, we found, that in leſs than half a quarter of an hour the fire was got out for good and all, and paſt the poſſibility of being recovered by the re-admitted air.

SOME circumſtances beſides thoſe already mentioned, occurred in the making of the experiment; of which theſe are the principal.

FIRST, when the receiver was full of ſmoke, if the cylinder were emptied, immediately upon the turning of the ſtop-cock, the receiver would appear manifeſtly darkened to his eye that looked upon the light through it; and this darkneſs was much leſs when the receiver was much leſs filled with fumes: it was alſo inſtantaneous, and ſeemed to proceed from a ſudden change of place and ſituation in the exhalations, upon the vent ſuddenly afforded them and the air they were mixt with, out of the receiver into the cylinder.

THE next thing we obſerved was a kind of halos that appeared a good while about the fire, and ſeemed to be produced by the ſurrounding exhalations.

AND laſtly, it is remarkable, that even when the fumes ſeemed moſt to repleniſh the receiver, they did not ſenſibly hinder the air included in the bladder from dilating itſelf after the ſame manner (for aught we could diſcern) as it would have otherwiſe done: ſo that before the fire or the match was quite extinct, the bladder appeared ſwelled at leaſt to ſix or ſeven times its former capacity.

SINCE the writing of theſe laſt lines, we took a ſmall receiver, capable of containing (by gueſs) about a pound and a half of water; and in the midſt of it we ſuſpended a lighted match; but though within one minute of an hour (or thereabouts) from the putting in of the match, we had cemented on the cover, yet we could not make ſuch haſte, but that before we began to pump, the ſmoke had ſo filled that ſmall receiver, as for aught we diſcerned, to choke the fire. And having again and again reiterated the experiment, it ſeemed ſtill as at firſt, that we could not cloſe up the veſſel, and pump out all the fumes time enough to reſcue the fire from extinction, whereupon we made uſe of this expedient. As ſoon as we had pumped once or twice, we ſuddenly turned the key, and thereby gave acceſs to the excluded air, which ruſhing violently in, as if it had been forced thorow a pair of bellows, did both drive away the aſhes, fill the glaſs with freſh air, and by blowing the almoſt extinguiſhed fire, rekindled it, as appeared by the match's beginning again to ſmoke, which before it had ceaſed to do. We having by this means obtained a lighted match in the receiver, without being reduced to ſpend time to cloſe it up, commanded the air to be immediately pumped out, and found, that upon the exſuction of it, the match quickly left ſmoking, as it ſeemed, by reaſon of the abſence of the air; and yet if ſome urgent occaſions had not hindered us, we would for greater ſecurity have tried, whether or no the match re-kindled as formerly, would ſmoke much longer, in caſe of no exſuction of the ambient air.

EXPERIMENT XIV.

TO try divers things at once, and particularly whether fire, though we found it would not long laſt, might not be produced in our evacuated receiver; we took a piſtol of about a foot in length, and having firmly tied it to a ſtick almoſt as long as the cavity of the receiver, we very carefully primed it with well-dried gunpowder, and then cocking it, we tied to the tricker one end of a ſtring, whoſe other end was faſtened to the key formerly mentioned to belong to the cover of our receiver. This done, we conveyed the piſtol, together with the annexed ſtaff, into the veſſel; which being cloſed up, and emptied after the uſual manner, we began to turn the key in the cover; and thereby ſhortening the ſtring that reached from it to the piſtol, we pulled aſide the tricker, and obſerved, that, according to our expectation, the force of the ſpring of the lock was not ſenſibly abated by the abſence of the air; (from whoſe *impetus* yet ſome modern naturaliſts would derive the cauſe of the motion of reſtitution in ſolid bodies.) For the cock falling with its wonted violence upon the ſteel, ſtruck out of it as many, and as conſpicuous parts of fire, as, for aught we could perceive, it would have done in the open air. Repeating this experiment divers times, we alſo obſerved, whether or no there would appear any conſiderable diverſity in the motion of the ſhining ſparks, in a place where the remaining air was ſo much rarified, but could not perceive but that they moved ſome of them upwards, as well as ſome of them downwards, and ſome of them ſide-ways, as they are wont to do, when upon ſuch colliſions they fly out in the open air.

WE likewiſe cauſed a piece of ſteel to be made of the form and bigneſs of the flint, in whoſe place we put it, and then the piſtol being cocked and conveyed into the receiver, the tricker was pulled after the air was drawn out; and though the place were purpoſely ſomewhat darkened, yet there appeared not upon the ſtriking of the two ſteels againſt each other the leaſt ſpark of fire; nor did we expect any (having before in vain attempted to ſtrike fire this way in the open air) though we thought fit to make the experiment, to undeceive thoſe, who fancy in rarified air, I know not what ſtrange diſpoſition, to take fire upon a much ſlighter occaſion than this experiment afforded. We have indeed found, that by the dextrous colliſion of two hardened pieces of ſteel, ſtore of ſparks may be ſtruck ont: but that was done with ſuch vehement percuſſion of the edges of the two ſteels, as could not well be compaſſed in our receiver.

BUT the chief thing we deſigned to do with our piſtol, was, to obſerve, whether gunpowder would take fire in our emptied and cloſely ſtopped glaſs? whether the expanſion of the flame would be conſiderably varied by the abſence of ſo much of the ambient air as was drawn out of the receiver? and whether the flame would diffuſe itſelf upward, as it is wont, notwithſtanding its not having about it the uſual proportion of air to force it up? And though moſt of our attempts to fire the gunpowder in the pan of the piſtol ſucceeded not, becauſe we were fain to let it hang almoſt perpendicular in the receiver, whereby the powder was ſhaken down before the ſparks could reach it: yet once the experiment ſucceeded, and the kindled powder ſeemed to make a more expanded flame than it would have done in the open air, but mounted upwards according to its wont; whether by reaſon of that little portion of air, which in ſpight of our pumping remained in the receiver, or for any other cauſe, we have not now the leiſure to conſider. But we muſt not forget, that, upon the extinction of the flame, the receiver appeared darkened with ſmoak, which ſeemed to move freely up and down, and upon the letting in the air at the ſtop-cock, began to circulate much faſter than before. We would have made more obſervations concerning this flame, but that of two or three attempts we afterwards made to repeat the kindling of the powder, not any one ſucceeded; and we have not the leiſure to dwell long upon one kind of trials.

EXPERIMENT XV.

TO theſe experiments concerning fire we added another, which, though it ſucceeded not, may perhaps without impertinency be recorded; partly, becauſe that (as we have in another treatiſe amply declared) it is uſeful to recite what experiments miſcarry as well as ſucceed. And partly alſo, becauſe it is very poſſible, that what we endeavoured in vain, may be performed by your Lordſhip, or ſome other Virtuoſo, that ſhall have ſtancher veſſels than we had, and more ſunny days than the preſent winter allows us.

WE conveyed then into one of our ſmall receivers a piece of matter combuſtible, dry and black, (experience declaring things of that colour to be moſt eaſy kindled;) and carefully cloſing the veſſel, we brought it to a window, at which the ſun, not very far from the meridian, ſhone in very freely: then drawing out the air with ſpeed, we united the ſun-beams with a burning-glaſs upon the combuſtible matter, which began immediately to ſend forth a ſmoak that quickly darkened the receiver; but notwithſtanding all our care and diligence the external air got in ſo faſt, that after divers trials we were fain to leave off the experiment in that glaſs, and induced to make trial of it in our great receiver.

HAVING then after ſome difficulty lodged the combuſtible matter in the cavity of this veſſel in ſuch manner, as that it was almoſt contiguous to that ſide thereof that was next the ſun, we did endeavour with a pretty large burning-glaſs to kindle it; but found, as we feared, that, by reaſon of the thickneſs of the glaſs, (which was alſo of a leſs pure and leſs diaphanous matter than the other) the ſun-beams thrown in by the burning-glaſs, were in their paſſage ſo diſlocated and ſcattered, (not now to mention thoſe many that, being reflected, could not pierce into the cavity of the receiver) that we could not poſſibly unite enough of them to kindle the matter, nor ſo much as to make it ſenſibly ſmoak. Yet we hope, that the ſeeing whether bodies (other than gun-powder) may be kindled, and what would happen to them, when ſet on fire in a place in great meaſure devoid of air, may prove ſo luciferous an experiment, that when the ſeaſon is more favourable, we ſhall, God permitting, make farther trial of it, and acquaint your Lordſhip with the event, if it proves proſperous. In the mean time we ſhall paſs on to other experiments, as ſoon as we have advertiſed your Lordſhip, that we have forborn to make ſuch reflections upon the ſeveral experiments we have ſet down concerning fire, as the matter would have eaſily afforded, and your Lordſhip may perhaps have expected. But I made the leſs ſcruple to forbear the annexing of ſpeculations to theſe recitals, becauſe *Carneades* and *Eleutherius* have in ſome Dialogues concerning heat and flame, (which were laſt year ſeen by ſome friends, and may be, when you pleaſe, commanded by you) mentioned divers of my thoughts and experiments concerning fire.

EXPERIMENT XVI.

WE deſigned to try, whether or no divers magnetical experiments would exhibit any unuſual phænomena, being made in our evacuated receiver inſtead of the open air: but for want of leiſure and conveniency to proſecute ſuch trials, we were induced to reſerve the reſt for another time, and to content ourſelves with making that which follows. We conveyed into the receiver a little pedeſtal of wood, in the midſt of which was perpendicularly erected a ſlender iron, upon whoſe ſharp point an excited needle of ſteel purpoſely made, and of about five inches long, was ſo placed, that hanging in an æquilibrium it could move freely towards either hand. Then the air being after the uſual manner pumped out, we applied a load-ſtone moderately vigorous to the outſide of the glaſs, and found, that it attracted or repelled the ends of the needle, according to the laws magnetical, without any remarkable difference from what the ſame load-ſtone would have done, had none of the air been drawn away from about the needle; which, when the load-ſtone was removed, after ſome tremulous vibrations to and fro, reſted in a poſition, wherein it looked north and ſouth.

EXPERIMENT XVII.

PROCEED we now to the mention of that experiment, whereof the ſatisfactory trial was the principal fruit I promiſed myſelf from our engine, it being then ſufficiently known, that in the experiment *de vacuo*, the quickſilver in the tube is wont to remain elevated, above the ſurface of that whereon it leans, about 27 digits. I conſidered, that, if the true and only reaſon why the quickſilver falls no lower, be, that at that altitude the mercurial cylinder in the tube is in an æquilibrium with the cylinder of air ſuppoſed to reach from the adjacent mercury to the top of the atmoſphere; then if this experiment could be tried out of the atmoſphere, the quickſilver in the tube would fall down to a level with that in the veſſel, ſince then there would be no preſſure upon the ſubjacent, to reſiſt the weight of the incumbent mercury. Whence I inferred (as eaſily I might) that if the experiment could be tried in our engine, the quickſilver would ſubſide below 27 digits, in proportion to the exſuction of air, that ſhould be made out of the receiver. For, as when the air is ſhut into the receiver, it doth (according to what hath above been taught) continue there as ſtrongly compreſſed, as it did whilſt all the incumbent cylinder of the atmoſphere leaned immediately upon it; becauſe the glaſs, wherein it is penned up, hinders it to deliver itſelf, by an expanſion of its parts, from the preſſure wherewith it was ſhut up. So if we could perfectly draw the air out of the receiver, it would conduce as well to our purpoſe, as if we were allowed to try the experiment beyond the atmoſphere.

WHEREFORE (after having ſurmounted ſome little difficulties, which occurred at the beginning) the experiment was made after this manner: we took a ſlender and very curiouſly blown cylinder of glaſs, of near three foot in length, and whoſe bore had in diameter a quarter of an inch, wanting a hair's breadth: this pipe being hermetically ſealed at one end, was, at the other, filled with quickſilver, care being taken in the filling, that as few bubbles as was poſſible ſhould be left in the mercury. Then the tube beiog ſtopt with the finger and inverted, was opened, according to the manner of the experiment, into a ſomewhat long and ſlender cylindrical box (inſtead of which we now are wont to uſe a glaſs of the ſame form) half filled with quickſilver: and ſo, the liquid metal being ſuffered to ſubſide, and a piece of paper being paſted on level with its upper ſurface, the box and tube and all were by ſtrings carefully let down into the receiver: and then, by means of the hole formerly mentioned to be left in the cover, the ſaid cover was ſlipt along as much of the tube as reached above the top of the receiver; and the interval, left betwixt the ſides of the hole and thoſe of the tube, was very exquiſitely filled up with melted (but not over-hot) diachylon, and the round chink, betwixt the cover and the receiver, was likewiſe very carefully cloſed up: upon which cloſure there appeared not any change in the height of the mercurial cylinder, no more than if the interpoſed glaſs-receiver did not hinder the immediate preſſure of the ambient atmoſphere upon the incloſed air; which hereby appears to bear upon the mercury, rather by virtue of its ſpring than of its weight; ſince its weight cannot be ſuppoſed to amount to above two or three ounces, which is inconſiderable in compariſon to ſuch a cylinder of mercury as it would keep from ſubſiding.

All things being thus in a readineſs, the ſucker was drawn down; and, immediately upon the egreſs of a cylinder of air out of the receiver, the quickſilver in the tube did, according to expectation, ſubſide: and notice being carefully taken (by a mark faſtened to the outſide) of the place where it ſtopt, we cauſed him that managed the pump to pump again, and marked how low the quickſilver fell at the ſecond exſuction; but continuing this work, we were quickly hindered from accurately marking the ſtages made by the mercury, in its deſcent, becauſe it ſoon ſunk below the top of the receiver, ſo that we could henceforward mark it no other ways than by the eye. And thus, continuing the labour of pumping for about a quarter of an hour, we found ourſelves unable to bring the quickſilver in the tube totally to ſubſide; becauſe, when the receiver was conſiderably emptied of its air, and conſequently that little that remained grown unable to reſiſt the irruption of the external, that air would (in ſpight of whatever we could do) preſs in at ſome little avenue or other; and though much could not thereat get in, yet a little was ſufficient to counterbalance the preſſure of ſo ſmall a cylinder of quickſilver, as then remained in the tube.

Now (to ſatisfy ourſelves farther, that the falling of the quickſilver in the tube to a determinate height, proceedeth from the æquilibrium, wherein it is at that height with the external air, the one gravitating, the other preſſing with equal force upon the ſubjacent mercury) we returned the key and let in ſome new air; upon which the mercury immediately began to aſcend (or rather to be impelled upwards) in the tube, and continued aſcending, till, having returned the key, it immediately reſted at the height which it had then attained: and ſo, by turning and returning the key, we did ſeveral times at pleaſure impel it upwards, and check its aſcent. And laſtly, having given a free egreſs at the ſtop-cock to as much of the external air as would come in, the quickſilver was impelled up almoſt to its firſt height: I ſay almoſt, becauſe it ſtopt near a quarter of an inch beneath the paper-mark formerly mentioned; which we aſcribed to this, that there was (as is uſual in this experiment) ſome little particles of air engaged among thoſe of the quickſilver; which particles, upon the deſcent of the quickſilver, did manifeſtly riſe up in bubbles towards the top of the tube, and by their preſſure, as well as by leſſening the cylinder by as much room as they formerly took up in it, hindered the quickſilver from regaining its firſt height.

This experiment was a few days after repeated, in the preſence of thoſe excellent and deſervedly famous Mathematic Profeſſors, Dr. *Wallis*, Dr. *Ward*, and Mr. *Wren*, who were pleaſed to honour it with their preſence; and whom I name, both as juſtly counting it an honour to be known to them, and as being glad of ſuch judicious and illuſtrious witneſſes of our experiment; and it was by their gueſs, that the top of the quickſilver in the tube was defined to be brought within an inch of the ſurface of that in the veſſel.

And here, for the illuſtration of the foregoing experiment, it will not be amiſs to mention ſome other particulars relating to it.

First then, when we endeavoured to make the experiment with the tube cloſed at one end with diachylon inſtead of an hermetical ſeal, we perceived, that upon the drawing of ſome of the air out of the receiver, the mercury did indeed begin to fall, but continued afterwards to ſubſide, though we did not continue pumping. When it appeared, that though the diachylon, that ſtopt the end of the tube, were ſo thick and ſtrong, that the external air could not preſs it in, (as experience taught us that it would have done, if there had been but little of it;) yet the ſubtler parts of it were able (though ſlowly) to inſinuate themſelves through the very body of the plaiſter, which it ſeems was not of ſo cloſe a texture, as that which we mentioned ourſelves to have ſucceſsfully made uſe of, in the experiment *de vacuo* ſome years ago. So that now we begin to ſuſpect, that perhaps one reaſon, why we cannot perfectly pump out the air, may be, that when the veſſel is almoſt empty, ſome of of the ſubtler parts of the external air may, by the preſſure of the atmoſphere, be ſtrained through the very body of the diachylon into the receiver. But this is only conjecture.

Another circumſtance of our experiment was this, that if (when the quickſilver in the tube was fallen low) too much ingreſs were, at the hole of the ſtop-cock, ſuddenly permitted to the external air; it would ruſh in with that violence, and bear ſo forcibly upon the ſurface of the ſubjacent quickſilver, that it would impel it up into the tube rudely enough to endanger the breaking of the glaſs.

We formerly mentioned, that the quickſilver did not, in its deſcent, fall as much at a time, after the two or three firſt exſuctions of the air, as at the beginning. For, having marked its ſeveral ſtages upon the tube, we found, that at the firſt ſuck it deſcended an inch and $\frac{3}{8}$, and at the ſecond an inch and $\frac{3}{8}$; and when the veſſel was almoſt emptied, it could ſcarce at one exſuction be drawn down above the breadth of a barley-corn. And indeed we found it very difficult to meaſure, in what proportion theſe decrements of the mercurial cylinder did proceed; partly, becauſe (as we have already intimated) the quickſilver was ſoon drawn below the top of the receiver; and partly becauſe, upon its deſcent at each exſuction, it would immediately reaſcend a little upwards; either by reaſon of the leaking of the veſſel at ſome imperceptible hole or other, or by reaſon of the motion of reſtitution in the air, which, being ſomewhat compreſſed by the fall as well as weight of the quickſilver, would repel it a little upwards, and make it vibrate a little up and down, before they could reduce each other to ſuch an æquilibrium as both might reſt in.

But though we could not hitherto make obſervations accurate enough, concerning the meaſures of the quickſilver's deſcent, to reduce them into any hypotheſis, yet would we not diſcourage any from attempting it; ſince, if it could be reduced to a certainty, it is probable, that the diſcovery would not be unuſeful.

And, to illuſtrate this matter a little more, we will add, that we made a ſhift to try the experiment in one of our above mentioned ſmall receivers, not containing a quart; but (agreeably to what we formerly obſerved) we found it as difficult to bring this to be quite empty as to evacuate the greater; the leaſt external air that could get in (and we could not poſſibly keep it all perfectly out) ſufficing, in ſo ſmall a veſſel, to diſplay a conſiderable preſſure upon the ſurface of the mercury, and thereby hinder that in the tube from falling to a level with it. But this is remarkable, that having two or three times tried the experiment in a ſmall veſſel upon the very firſt cylinder of air that was drawn out of the receiver, the mercury fell in the tube 18 inches and a half, and another trial 19 inches and a half.

But on this occaſion, I hold it not unfit to give your Lordſhip notice, that I hoped from the deſcent of the quickſilver in the tube, upon the firſt ſuck, to derive this advantage; that I ſhould thence be enabled to give a nearer gueſs at the proportion of force betwixt the preſſure of the air (according to its various ſtates, as to denſity and rarefaction) and the gravity of quickſilver, than hitherto hath been done. For in our experiment there are divers things given, that may be made uſe of towards ſuch a diſcovery. For firſt, we may know the capacity of the receiver wherein the experiment is made, ſince, by filling it with water, we may eaſily compute how many quarts, or meaſures of any other denomination, it containeth of air; which air, when ſhut up in the veſſel, may be ſuppoſed to have a preſſure equal to that of the atmoſphere; ſince it is able to keep the quickſilver in the tube from falling any lower than it did in the free and open air. Next, here is given us the capacity of the braſs cylinder, emptied by the drawing down of the ſucker (its bore and height being mentioned in the deſcription of our pump) whereby we may come to know how much of the air contained in the receiver is drawn out at the firſt ſuck. And we may alſo eaſily define, either in weight or cubic meaſures, the cylinder of quickſilver, that anſwers to the cylinder of air lately mentioned, (that mercurial cylinder being in our engine computable by deducting from the entire altitude of that cylinder of quickſilver, the altitude at which it reſts upon the firſt exſuction.) But though, if this experiment were very watchfully tried in veſſels of ſeveral ſizes, and the various deſcents of the quickſilver compared among themſelves, it is not improbable, that ſome ſuch things, as we hoped for, may thereby be diſcovered. Yet, becauſe not only the ſolid contents of as much of the glaſs-tube as remains within the concave ſurface of the receiver, and (which is more difficult) the varying contents of the veſſel containing the mercury, and of as much of the mercury itſelf as is not in the tube, muſt be deducted out of the capacity of the receiver, but there muſt alſo an allowance be made for this, that the cylinder, that is emptied by the drawing down of the ſucker, and comes to be filled upon the letting of the air out of the receiver into it, is not ſo repleniſhed with air as the receiver itſelf at firſt was; becauſe there paſſeth no more air out of the receiver into the cylinder, than is requiſite to reduce the air in the cavity of the cylinder, and in that of the receiver to the ſame meaſure of dilatation. Becauſe of theſe (I ſay) and ſome other difficulties, that require more ſkill in mathematics than I pretend to, and much more leiſure than my preſent occaſions would allow me, I was willing to refer the nicer conſideration of this matter to ſome of our learned and accurate mathematicians, thinking it enough for me to have given the hint already ſuggeſted.

For farther confirmation of what hath been delivered, we likewiſe tried the experiment in a tube of leſs than two foot long: and, when there was ſo much air drawn out of the veſſel, that the remaining air was not able to counterbalance the mercurial cylinder, the quickſilver in the tube ſubſided ſo viſibly, that (the experiment being tried in the little veſſel lately mentioned) at the firſt ſuck it fell above a ſpan, and was afterwards drawn lower and lower for a little while; and the external air being let in upon it, impelled it up again almoſt to the top of the tube: ſo little matters it, how heavy or light the cylinder of quickſilver to ſubſide is, provided its gravity overpower the preſſure of as much external air as bears upon the ſurface of that mercury into which it is to fall.

Lastly, we alſo obſerved, that if (when the mercury in the tube had been drawn down, and by an ingreſs permitted to the external air, impelled up again to its former height) there were ſome more air thruſt up by the help of the pump into the receiver, the quickſilver in the tube would aſcend much above the wonted height of 27 digits, and immediately upon the letting out of that air would fall again to the height it reſted at before.

Your Lordſhip will here perhaps expect, that as thoſe, who have treated of the Torricellian experiment, have for the moſt part maintained the affirmative, or the negative of that famous queſtion, whether or no that noble experiment infer a vacuum? ſo I ſhould on this occaſion interpoſe my opinion touching that controverſy; or at leaſt declare, whether or no, in our engine, the exſuction of the air do prove the place deſerted by the air ſucked out to be truly empty, that is, devoid of all corporeal ſubſtance. But beſides that I have neither the leiſure, nor the ability, to enter into a ſolemn debate of ſo nice a queſtion; your Lordſhip may, if you think it worth the trouble, in the Dialogues not long ſince referred to, find the difficulties on both ſides repreſented, which then made me yield but a very wavering aſſent to either of the parties contending about the queſtion: nor dare I yet take upon me to determine ſo difficult a controverſy.

FOR on the one ſide it appears, that notwithſtanding the exſuction of the air, our receiver may not be deſtitute of all bodies, ſince any thing placed in it, may be ſeen there; which would not be, if it were not pervious to thoſe beams of light, which rebounding from the ſeen object to our eyes, affect us with the ſenſe of it: and that either theſe beams are corporeal emanations from ſome lucid body, or elſe at leaſt the light they convey doth reſult from the briſk motion of ſome ſubtle matter, I could, if I miſtake not, ſufficiently manifeſt out of the Dialogues above-mentioned, if I thought your Lordſhip could ſeriouſly imagine that light could be conveyed without, at leaſt, having (if I may ſo ſpeak) a body for its vehicle.

BY the ſixteenth experiment, it alſo appears that the cloſeneſs of our receiver hinders it not from admitting the effluvia of the load-ſtone; which makes it very probable that it alſo freely admits the magnetical ſteams of the earth; concerning which, we have in another treatiſe endeavoured to manifeſt that numbers of them do always permeate our air.

BUT on the other ſide it may be ſaid, that as for the ſubtle matter which makes the objects encloſed in our evacuated receiver, viſible, and the magnetical effluvia of the earth that may be preſumed to paſs thorough it, though we ſhould grant our veſſel not to be quite devoid of them, yet we cannot ſo reaſonably affirm it to be repleniſhed with them, as we may ſuppoſe, that if they were gathered together into one place without intervals between them, they would fill but a ſmall part of the whole receiver. As in the thirteenth experiment, a piece of match was inconſiderable for its bulk, whilſt its parts lay cloſe together, that afterwards (when the fire had ſcattered them into ſmoke) ſeemed to repleniſh all the veſſel. For (as elſewhere our experiments have demonſtrated) both light and the effluvia of the load-ſtone may be readily admitted into a glaſs, hermetically ſealed, though before their admiſſion, as full of air as hollow bodies here below are wont to be; ſo that upon the exſuction of the air, the large ſpace deſerted by it, may remain empty, notwithſtanding the pretence of thoſe ſubtle corpuſcles, by which lucid and magnetical bodies produce their effects.

AND as for the allegations above-mentioned, they ſeemed to prove but that the receiver devoid of air, may be repleniſhed with ſome etherial matter, as ſome modern Naturaliſts write of, but not that it really is ſo. And indeed to me it yet ſeems, that as to thoſe ſpaces which the Vacuiſts would have to be empty, becauſe they are manifeſtly devoid of air and all groſſer bodies; the Pleniſts (if I may ſo call them) do not prove that ſuch ſpaces are repleniſhed with ſuch a ſubtle matter as they ſpeak of, by any ſenſible effects, or operations of it (of which divers new trials purpoſely made, have not yet ſhewn me any) but only conclude that there muſt be ſuch a body, becauſe there cannot be a void. And the reaſon why there cannot be a void, being by them taken, not from any experiments, or phænomena of nature, that clearly and particularly prove their hypotheſis, but from their notion of a body, whoſe nature,

ture, according to them, conſiſting only in extenſion (which indeed ſeems the property moſt eſſential to, becauſe inſeparable from a body) to ſay a ſpace devoid of body, is, to ſpeak in the ſchoolmen's phraſe, a contradiction *in adjecto*. This reaſon, I ſay, being thus deſumed, ſeems to make the controverſy about a vacuum rather a metaphyſical, than a phyſiological queſtion; which therefore we ſhall here no longer debate, finding it very difficult either to ſatisfy Naturaliſts with this Carteſian notion of a body, or to manifeſt wherein it is erroneous, and ſubſtitute a better in its ſtead.

BUT though we are unwilling to examine any farther the inferences wont to be made from the Torricellian experiment, yet we think it not impertinent to preſent your Lordſhip with a couple of advertiſements concerning it.

FIRST then, if in trying the experiment here or elſewhere, you make uſe of the Engliſh meaſures that mathematicians and tradeſmen are here wont to employ, you will, unleſs you be forewarned of it, be apt to ſuſpect that thoſe that have written of the experiment have been miſtaken. For whereas men are wont generally to talk of the quickſilver's remaining ſuſpended at the height of between ſix or ſeven and twenty inches; we commonly obſerved, when divers years ſince we firſt were ſolicitous about this experiment, that the quickſilver in the tube reſted at about 29 inches and a half above the ſurface of the reſtagnant quickſilver in the veſſel, which did at firſt both amaze and perplex us, becauſe though we held it not improbable that the difference of the groſſer Engliſh air, and that of Italy and France, might keep the quickſilver from falling quite as low in this colder, as in thoſe warmer climates; yet we could not believe that that difference in the air ſhould alone be able to make ſo great an one in the heights of the mercurial cylinders; and accordingly upon enquiry we found, that though the various denſity of the air be not to be overlooked in this experiment, yet the main reaſon why we found the cylinder of mercury to conſiſt of ſo many inches, was this, that our Engliſh inches are ſomewhat inferior in length to the digits made uſe of in foreign parts, by the writers of the experiment.

THE next thing I deſire your Lordſhip to take notice of, is, that the height of the mercurial cylinder is not wont to be found altogether ſo great as really it might prove, by reaſon of the negligence or incogitancy of moſt that make the experiment. For oftentimes upon the opening of the inverted tube into the veſſelled mercury, you may obſerve a bubble of air to aſcend from the bottom of the tube through the ſubſiding quickſilver to the top; and almoſt always you may, if you look narrowly, take notice of a multitude of ſmall bubbles all along the inſide of the tube betwixt the quickſilver and the glaſs; (not now to mention the particles of air that lie concealed in the very body of the mercury:) many of which, upon the quickſilver's forſaking the upper part of the tube, do break into that deſerted ſpace where they find little or no reſiſtance to their expanding of themſelves. Whether this be the reaſon, that upon the application of warm bodies to the emptied part of the tube, the ſubjacent mercury would be depreſſed ſomewhat lower, we ſhall not determine; though it ſeem very probable, eſpecially ſince we found, that, upon the application of linen cloths dipped in water, to the ſame part of the tube, the quickſilver would ſomewhat aſcend; as if the cold had condenſed the impriſoned air (that preſſed upon it) into a leſſer room. But that the deſerted ſpace is not wont to be totally devoid of air, we were induced to think by ſeveral circumſtances: for when an eminent mathematician, and excellent experimenter, had taken great pains and ſpent much time in accurately filling up a tube of mercury, we found that yet there remained ſtore of inconſpicuous bubbles, by inverting the tube, letting the quickſilver fall to its wonted height; and by applying (by degrees) a red-hot iron to the outſide of the tube, over againſt the upper part of the mercurial cylinder, (for hereby the little unheeded bubbles, being mightily expanded, aſcended in ſuch numbers, and ſo faſt to the deſerted ſpace, that the upper part of the quickſilver ſeemed, to our wonder, to boil.) We farther obſerved, that in the trials of the Torricellian experiment, we have ſeen made by others, and (one excepted) all our own, we never found that, upon the inclining of the tube, the quickſilver would fully reach to the very top of the ſealed end: which argued, that there was ſome air retreated thither that kept the mercury out of the unrepleniſhed ſpace.

If your Lordſhip ſhould now demand what are the beſt expedients to hinder the intruſion of the air in this experiment; we muſt anſwer, that of thoſe which are eaſily intelligible without ocular demonſtration; we can at preſent ſuggeſt, upon our own trials, no better than theſe. Firſt, at the open end of the tube the glaſs muſt not only be made as even at the edges as you can, but it is very convenient (eſpecially if the tube be large) that the bottom be every way bent inwards, that ſo the orifice not much exceeding a quarter of an inch in diameter, may be the more eaſily and exactly ſtopped by the experimenter's finger; between which and the quickſilver, that there may be no air intercepted (as very often it happens that there is) it is requiſite that the tube be filled as full as poſſibly it can be, that the finger which is to ſtop it, preſſing upon the accumulated and protuberant mercury, may rather throw down ſome, than not find enough exactly to keep out the air. It is alſo an uſeful and compendious way not to fill the tube at firſt quite of mercury, but to leave near the top about a quarter of an inch empty; for if you then ſtop the open end with your finger, and invert the tube, that quarter of an inch of air will aſcend in a great bubble to the top, and in its paſſage thither, will gather up all the little bubbles, and unite them with itſelf into one great one; ſo that if by reinverting the tube, you let that bubble return to the open end of it, you will have a much cloſer mercurial cylinder than before, and need but to add a very little quickſilver more to fill up the tube exactly. And laſtly, as for thoſe leſſer and inconſpicuous parcels of air which cannot this way be gleaned up, you may endeavour, before you invert the tube, to free the quickſilver from them by ſhaking the tube, and gently knocking on the outſide of it, after every little parcel of quickſilver which you pour in; and afterwards, by forcing the ſmall latitant bubbles of air to diſcloſe themſelves and break, by imploying a hot iron in ſuch manner as we lately mentioned. I remember that by carefully filling the tube, though yet it were not quite free from air, we have made the mercurial cylinder reach to 30 inches and above an eighth, and this in a very ſhort tube: which we therefore mention, becauſe we have found, by experience, that in ſhort tubes a little air is more prejudicial to the experiment than in long ones, where the air having more room to expand itſelf, doth leſs potently preſs upon the ſubjacent mercury.

And ſince we are fallen upon the conſideration of the altitude of the mercurial cylinder, I muſt not conceal from your Lordſhip an experiment relating thereunto, which perhaps will ſet both you and many of your friends, the Vertuoſi, on thinking; and, by diſcloſing ſome things about the air or atmoſphere, that have ſcarce hitherto been taken notice of, may afford you ſome hint conducive to a further diſcovery of the ſubject of this epiſtle.

EXPERIMENT XVIII.

WE took a glaſs tube, which though it were not much above three foot long, we made choice of, becauſe it was of a more than ordinarily even thickneſs. This we filled with mercury, though not with as much care as we could, yet with ſomewhat more than is wont to be uſed in making the Torricellian experiment. Then having, according to the manner, inverted the tube, and opened the mouth of it beneath the ſurface of ſome other quickſilver, that in the tube fell down to the wonted height, leaving, as is uſual, ſome little particles of air in the ſpace it deſerted; as we gueſſed by obſerving, that upon the application of hot bodies to the upper part of the tube, the quickſilver would be a little depreſſed. Laſtly, having put both the tube and the veſſel it leaned on into a convenient wooden frame to keep them from miſchances; we placed that frame in a window within my bed-chamber, that I might both keep the mercury from being ſtirred, and have opportunity to watch from time to time the phænomena it was to exhibit. For the better diſcovery of which, when the quickſilver both in the tube and ſubjacent veſſel was perfectly at reſt, we took notice, by a mark made on the outſide of the glaſs, how high the included liquor then reached.

DURING ſeveral weeks that the tube was kept in that window (which was very rarely opened) I had the opportunity to obſerve, that the quickſilver did ſometimes faintly imitate the liquor of a weather-glaſs, ſubſiding a little in warm, and riſing a little in cold weather; which we aſcribed to the greater or leſſer preſſure of that little air that remained at the top of the tube, expanded or condenſed by the heat or cold that affected the ambient air. But that which I was chiefly careful to obſerve, was this, that oftentimes the quickſilver did riſe and fall in the tube, and that very notably, without conforming itſelf to what is uſual in weather-glaſſes, whoſe air is at the top, nay, quite contrary thereunto: for ſometimes I obſerved it in very cold weather (ſuch as this winter hath already afforded us good ſtore of) to fall down much lower than at other times, when by reaſon of the abſence of both froſt, ſnow, and ſharp winds, the air was comparatively much warmer. And I farther obſerved, that ſometimes the quickſilver would for ſome days together reſt almoſt at the ſame height; and at other times again it would in the compaſs of the ſame day conſiderably vary its altitude, though there appeared no change either in the air abroad, or in the temper of the air within the room (wherein was conſtantly kept a good fire) nor in any thing elſe, to which either I, or ſome eminently learned men, whom I then acquainted with the experiment, could reaſonably impute ſuch a change: eſpecially conſidering that the ſpace wherein the mercury wandered up and down, within about five weeks, amounted to full two inches, of which we found by our ſeveral marks whereby we had taken notice of its ſeveral removes, that it had deſcended about $\frac{9}{16}$ of an inch from the place where it firſt ſettled, and the other inch and $\frac{7}{16}$ it had aſcended. And it ſeems probable that the height of the mercurial cylinder would have varied yet more, if the experiment had been made in the open air, and in a long tube, where the particles of the impriſoned air, by having more room to diſplay themſelves in, might not have had ſo ſtrong a ſpring to work upon the quickſilver with. But for want both of time and of a competent quantity of mercury (which was not to be procured where we then happened to be) we were unable to make any farther trials: which therefore chiefly troubled us, becauſe we would gladly have tried an ingenious experiment which was ſuggeſted unto us by that excellent mathematician Mr. *Wren*, who being invited to name any thing he would have us try touching the preſſure of the air, deſired us to obſerve whether or no the quickſilver in a long tube would not a little vary its height according to the tides, eſpecially about the new and full moon; about which times mariners obſerve thoſe great flowings and ebbs of the ſea, that they call the ſpring tides. For he ſagaciouſly and plauſibly conjectured that ſuch obſervations, accurately made, would diſcover the truth or erroneouſneſs of the Carteſian hypotheſis concerning the ebbing and flowing of the ſea: which *Des Cartes* aſcribeth to the greater preſſure made upon the air by the moon, and the intercurrent ethereal ſubſtance at certain times (of the day, and of the lunary month) than at others. But in regard we found the quickſilver in the tube to move up and down ſo uncertainly, by reaſon, as it ſeems, of accidental mutation in the air; I ſomewhat doubt whether we ſhall find the altitude of the quickſilver to vary as regularly as the experiment is ingeniouſly propoſed. The ſucceſs we ſhall (God permitting us to make trial of it) acquaint your Lordſhip with; and in the mean time take notice, that when we had occaſion to take the tube out of the frame (after it had ſtaid there part of *November* and part of *December)* a good fire being then in the room, becauſe it was a ſnowy day, we found the quickſilver in the tube to be above the upper ſurface of the ſubjacent mercury 29 inches three quarters.

IF your Lordſhip ſhould now aſk me what are the true cauſes of this varying altitude of the mercurial cylinder; I ſhould not undertake to anſwer ſo difficult a queſtion, and ſhould venture to ſay no more, than that among divers poſſible cauſes to which it may be aſcribed, it would not be, perhaps, abſurd to reckon theſe that follow.

FIRST then, we may conſider, that the air in the upper part of the tube is much more rarefied, and therefore more weak than the external air, as may appear by this among other things, that upon the inclining of the tube, the quickſilver will readily aſcend almoſt to the very top of it, and ſo take up eight or nine tenth parts, and perhaps more of that ſpace which it deſerted before: which would not happen, if that whole ſpace had been full of unrarefied air, ſince that (as trial may eaſily ſatisfy you) would not have ſuffered itſelf to be thruſt into ſo narrow a room by ſo weak a preſſure. So that although in our tube when the included air was heated, the quickſilver was ſomewhat depreſſed; yet there is this difference betwixt ſuch a tube and common weather-glaſſes, that in theſe the included and the ambient air are in an æquilibrium as to preſſure, and the weight of the water that keeps them ſeparate is ſcarce conſiderable. Whereas in ſuch a tube as we are ſpeaking of, the air within is very much more dilated than that without. And it is not ſo much the ſpring or reſiſtance of the included air, as the weight of the mercurial cylinder itſelf, that hinders the quickſilver from aſcending higher: for if we ſhould ſuppoſe that deſerted part of the tube perfectly devoid of air, yet would the quickſilver riſe but a little higher in it, and be far from filling it; in regard the outward air would not be able to impel up ſuch a weight much higher; whereas it may, by our former experiments appear, that if all the air in the upper part of the weather-glaſs were away, the water would be impelled up to the very top of it, though the pipe were above thirty foot long.

WE may next conſider, that this rarefied air, at the upper part of our tube, being exactly ſhut up betwixt the glaſs and the quickſilver, it was ſcarce ſubject to any diſcernable alterations, ſave thoſe it received from heat and cold.

AND we may farther conſider, that yet the external air or atmoſphere is ſubject to many alterations, beſides thoſe that proceed from either of thoſe qualities.

FOR the experiment that occaſioned this diſcourſe, ſeems to make it probable enough, that there may be ſtrange ebbings and flowings, as it were, in the atmoſphere, or at leaſt, that it may admit great and ſudden mutations, either as to its altitude or its denſity, from cauſes, as well unknown to us, as the effects are unheeded by us. And that you may not think that there is nothing in nature but our experiment that agrees with this our conjecture, we might put your Lordſhip in mind of the pains and aches that are often complained of by thoſe that have had great wounds or bruiſes, and that do preſage great mutations in the air oftentimes, whilſt to ſtrong and healthy perſons no ſign of any ſuch thing appears. And that is alſo very memorable to this purpoſe, which I remember I have ſomewhere read in a book of the ingenious *Kircherus*, who giving a pertinent admonition concerning the various refractions that may happen in the air, relateth, that during his ſtay in *Malta*, he often ſaw mount *Ætna*, though the next day, notwithſtanding its being extremely clear, he could not ſee it; adding, that *Vintemillius*, a very learned perſon, did oftentimes, from a hill he names, behold the whole iſland he calls *Luprica* protuberant above the ſea, though at other times, notwithſtanding a clear ſky, he could not ſee it. And though perhaps this may be in part aſcribed to the various light and poſition of the ſun, or to the various diſpoſition of the ſpectator's eye, or peradventure to ſome other cauſe; yet the moſt probable cauſe ſeems to be the differing denſity of the air, occaſioned by exhalations capable to increaſe the refraction, and conſequently bring beams to the eye, which otherwiſe would not fall on it. We have likewiſe, in another treatiſe, mentioned our having often obſerved with teleſcopes a plenty of ſteams in the air, which without ſuch a help would not be taken notice of, and which, as they were not at all times to be ſeen even through a teleſcope, ſo they did ſometimes, eſpecially after a ſhower of rain, haſtily diſappear: and when we have viſited thoſe places that abound with mines, we have ſeveral times been told by the diggers, that even when the ſky ſeemed clear, there would, not ſeldom, ſuddenly ariſe, and ſometimes long continue, a certain ſteam (which they uſually call a damp) ſo groſs and thick that it would oftentimes put out their very candles, if they did not ſeaſonably prevent it. And I think it will eaſily be granted, that the aſcenſion of ſuch ſteams into this or that part of the air, and their mixing with it, are very like to thicken it; as on the other ſide either heat or the ſudden condenſation of the air in another part of the atmoſphere (to mention now no other cauſes) are capable of rarefying it.

NOR will it very much import the main ſcope of our diſcourſe, whether it be ſuppoſed that the copious ſteams the earth ſends into the air, thicken that part of the atmoſphere that receives them, and make it more heavy; or that ſometimes the fumes may aſcend with ſuch celerity, that though the air be thickened, yet they rather diminiſh than encreaſe its gravitation, in regard that the quickneſs of their aſcent not only keeps them from gravitating themſelves, but may hinder the preſſing downwards of many aërial corpuſcles that they meet with in their way upwards. This, I ſay, is of no great importance to our preſent diſcourſe, ſince either way the terreſtrial ſteam may here and there conſiderably alter the gravity or preſſure of the atmoſphere.

YOUR Lordſhip may alſo be pleaſed to remember, that by our ſeventeenth experiment it appeared, that, as when the air in the receiver was expanded more than ordinarily, the quickſilver in the tube did proportionably ſubſide; ſo when the air in the ſame receiver was a little more than ordinarily compreſſed, it did impel up the quickſilver in the tube, above the wonted height of betwixt ſix and ſeven and twenty digits.

AND if to theſe things we annex, that, for aught we can find by trials purpoſely made, the degree of rarity or denſity of the air, ſhut up into our receiver, doth not ſenſibly alter its temperature as to cold or heat; it will not, I hope, appear abſurd to conceive, that ſince the air, included in the tube, could but very faintly hinder the aſcent of the quickſilver, or preſs it downwards; ſince too, that included air could ſcarce immediately receive any ſenſible alteration, ſave either by heat or cold; and ſince alſo, that according to the bare denſity or rarity of the air incumbent on the ſubjacent quickſilver in the veſſel, that in the tube was impelled more or leſs high; ſuch changes happening in the neighbouring part of the outward air, either by the aſcenſion of groſs or copious exhalations, or by any other cauſe (of which there may be divers) as were capable to make conſiderable alterations in the conſiſtence of the air, as to rarity and denſity, may be able proportionably to alter the height of the quickſilver: I rather ſay, that ſuch alterations may be, than that they are the cauſes of our phænomenon; becauſe I think it ſufficient, if I have propoſed conjectures not altogether irrational about a new myſtery of nature, touching which, the chief things I pretend to, is to give occaſion to the curious to enquire farther into it, than I have been yet able to do.

EXPERIMENT XIX.

THE ſame reaſon that moved us to conclude, that by the drawing of the receiver, the mercury would deſcend in a tube ſhorter than ſix and twenty digits, induced us alſo to expect, that by the ſame means water might be brought to ſubſide in glaſs-tubes of a moderate length: though by the noble experiment, ſaid to have been accurately made in *France*, by Monſieur *Paſchal*, we are informed that a tube of no leſs than about two and thirty foot, was found requiſite to make the experiment *de vacuo* ſucceed with water inſtead of quickſilver; ſo tall a cylinder of that lighter liquor being, it ſeems, requiſite to equal the weight of a mercurial cylinder of ſix or ſeven and twenty digits, and ſurmount the preſſure of the atmoſphere.

WE took then a tube of glaſs, hermetically ſealed at one end, of about four foot in length, and not very ſlender: this, at the open end, we filled with common water, and then ſtopped that end till we had inverted the tube, and opened it beneath the ſurface of a quantity of the like water, contained in a ſomewhat deep and ſlender veſſel. This veſſel, with the tube in it, was let down into the receiver, and the receiver being cloſed up after the accuſtomed manner, the pump was ſet on work.

As much of the event as concerns our preſent purpoſe, was this, that till a conſiderable part of the air was drawn out of the receiver, the tube continued top full of water as when it was put in, it being requiſite that a great part of the air, formerly contained in the receiver, ſhould be drawn out, to bring the remaining air to an æquilibrium, with ſo ſhort and light a cylinder of water. But when once the water began to fall in the tube, then each exſuction of air made it deſcend a little lower, though nothing near ſo much as the quickſilver at the beginning did in the experiment formerly mentioned. Nor did there appear ſo much inequality in the ſpaces tranſmitted by the water in its deſcent, as there did in thoſe obſerved in the fall of the quickſilver; of which the cauſe will ſcarce ſeem abſtruſe to him that ſhall duly reflect upon what hath been already delivered. And whereas we drew down the quickſilver in the tube ſo far, as to bring it within an inch of the ſurface of the other quickſilver into which it was to fall; the loweſt we were able to draw down the water was, by our conjecture, to about a foot or more above the ſurface of that in the veſſel; of which I know not whether it will be needful to aſſign ſo obvious a cauſe as that, though the little air remaining in the receiver could not hinder a cylinder of above an inch high of quickſilver from ſubſiding; yet it might be very well able, by its preſſure, to countervail the weight of a cylinder of a foot long or more, of a liquor ſo much leſs ponderous than quickſilver, as water is. And in fine, to conclude our experiment, when the water was drawn down thus low, we found, that by letting in the outward air, it might be immediately impelled up again to the higher parts of the tube.

We will add no more concerning this experiment, ſave that having tried it in one of our ſmall receivers, we obſerved, that upon the firſt exſuction of the air the water did uſually ſubſide divers inches, and at the ſecond (exſuction) fall down much lower, ſubſiding ſometimes near two foot; as alſo that upon the letting in of the air from without, the water was impelled up with very great celerity.

EXPERIMENT XX.

THAT the air hath a notable elaſtical power (whenceſoever that proceeds) we have, I ſuppoſe, abundantly evinced, and it begins to be acknowledged by the eminenteſt modern naturaliſts. But whether or no there be in water ſo much as a languid one, ſeems hitherto to have been ſcarce conſidered, nor hath been yet, for aught I know, determined either way by any writer; which invited us to make the following experiment.

There was taken a great glaſs-bubble, with a long neck, (ſuch as chymiſts are wont to call a philoſophical egg) which being filled with common water till the liquor reached about a ſpan above the bubble, and a piece of paper being there paſted on, was put unſtopped into the receiver, and then the air was ſucked out after the wonted manner. The event was this, that a conſiderable part of the air, penned up in the receiver, was drawn out, before we diſcerned any expanſion of the water: but, continuing the labour of pumping, the water manifeſtly began to aſcend in the ſtem of the glaſs, and divers bubbles looſening themſelves from the lower parts of the veſſel, made their way through the body of the water, to the top of it, and there brake into the receiver: and after the water once appeared to ſwell, then at each time the ſtop cock was turned to let out the air from the receiver into the pump, the water in the neck of the glaſs did ſuddenly riſe about the breadth of a barley-corn in the neck of the glaſs, and ſo attained by degrees to a conſiderable height above the mark formerly mentioned. And at length (to make the expanſion of the water more evident) the outward air was ſuddenly let in, and the water immediately ſubſided, and deſerted all the ſpace it had newly gained in the glaſs.

And, on this occaſion, it will not perhaps be amiſs to acquaint your Lordſhip here (though we have already mentioned it in another paper, to another purpoſe) with another expedient that we made uſe of two or three years ago, to try whether or no water had a ſpring in it. About that time then, that great and learned promoter of experimental philoſophy Dr. *Wilkins*, doing me the honour to come himſelf, and bring ſome of his inquiſitive friends to my lodging, we there had in readineſs a round and hollow veſſel of pewter, great enough to contain two pounds of water, and exactly cloſe every where but at one little hole where it was to be filled; then partly by ſucking out the air, and partly by injecting water with a ſyringe, it was (not without ſome difficulty) filled up to the top; and that hole being placed directly upwards, there was a little more water leiſurely forced in by the ſyringe. Upon which, though the veſſel were permitted to reſt, and the hole kept in its former poſture, yet the compreſſed wat r leiſurely ſwelled above the orifice of the hole, and divers drops ran over along the ſides of the veſſel. After this we cauſed a ſkilful pewterer (who had made the globe) to cloſe it up in our preſence with ſoder ſo exquiſitely, that none ſuſpected there was any thing left in it beſides water. And laſtly, the veſſel thus ſodered up, was warily and often ſtruck in divers places with a wooden mallet, and thereby was manifeſtly compreſſed, whereby the incloſed water was crouded into leſs room than it had before: and thereupon we took a needle, and with it and the mallet perforated the veſſel, and drew out the needle again; the water (but in a very ſlender ſtream) was ſuddenly thrown after it into the air, to the height of two or three feet. As for the other phænomena of this experiment, ſince they belong not to our preſent purpoſe, and are partly mentioned in another of our papers, we ſhall, inſtead of recording them here, give this advertiſement: that as evidently as this experiment, and that made in our receiver, ſeem to prove a power in the water to expand and reſtore itſelf after compreſſion; yet for a reaſon to be met with ere long, I judged it not ſafe to infer that concluſion from theſe premiſes, till I had made ſome of the following trials, to the mention of which I will therefore haſten.

EXPERIMENT XXI.

TO diſcover whether the expanſion of the water really proceeded from an elaſtical power in the parts of the water itſelf, we thought it requiſite to try two things: the one, whether or no the atmoſphere gravitates upon bodies under water; and the other, whether in caſe it do gravitate, the intumeſcence of the water may not be aſcribed to ſome ſubſtance ſubtler than itſelf, reſiding in it. In order to the ſatisfying myſelf about the firſt of theſe, I intended to let down into the receiver a veſſel of water, wherein ſhould be immerſed a very ſmall oiled bladder, almoſt devoid of air, but ſtrongly tied up at the neck with a ſtring, and detained a little under water by ſuch a weight faſtened to that ſtring, as ſhould juſt be able to keep the bladder from ſwimming, and no more. For I ſuppoſed, that if when all things were thus ordered, the receiver were emptied, in caſe there were any ſuch preſſure of the atmoſphere upon water, as I was inclined to believe, the air within the bladder, being, upon the exſuction of the air within the receiver, freed from that preſſure, and being preſſed only by the ſmall weight of the incumbent water, would conſiderably expand itſelf; but whilſt we were preparing bladders for this experiment, there occurred an eaſy way for the making at once both the diſcoveries I deſired.

WE took then a glaſs phial, containing by gueſs a pound and ſome ounces of water; this we filled top full, and then we put into the neck of it a glaſs pipe a pretty deal bigger than a gooſe-quill, open at both ends, and of divers inches in length: one end of this pipe was ſo put into the neck of the phial, as to reach a little below it, and then was carefully cemented thereto, that no air might get into the phial, nor any water get out of it, otherwiſe than through the pipe; and then the pipe being warily filled, about half-way up to the top, with more water, and a mark being paſted over againſt the upper ſurface of the liquor, the phial thus fitted with the pipe was, by ſtrings let down into the receiver, and according to the wonted manner exquiſitely cloſed up in it.

THIS done, we began to pump out the air, and when a pretty quantity of it had been drawn away, the water in the pipe began to riſe higher in the pipe, at the ſides of which ſome little bubbles diſcovered themſelves. After a little while longer, the water ſtill ſwelling, there appeared at the bottom of the pipe a bubble about the bigneſs of a ſmall pea, which aſcending through the pipe to the top of the water, ſtayed there a while and then broke; but the pump being nimbly plied, the expanſion of the water ſo increaſed, that quickly, getting up to the top of the pipe, ſome drops of it began to run down along the outſide of it; which obliged us to forbear pumping a while, and give the water leave to ſubſide within leſs than two inches of the bottom of the pipe. After this, the pump being again ſet on work, the bubbles began to aſcend from the bottom of the pipe, being not all of a ſize, but yet ſo big, that eſtimating one with another, they appeared to be of the ſize of the ſmaller ſort of peas; and of theſe we reckoned about ſixty which came up one after another, beſides ſtore of ſmaller ones, of which we made no reckoning. And at length, growing weary of reckoning and pumping too (becauſe we found, that in ſpight of all our pains and induſtry, ſome undiſcerned leak or other in the receiver hindered us from being able to empty it altogether) we thought fit to deſiſt for that time, after trial made of what operation the external air, being let in upon the expanded water, would have; and accordingly turned the key to let in the air, we ſaw, as we expected, that the water in the pipe in a moment fell down almoſt to the bottom of it.

Now of this experiment there are two or three circumſtances yet to be mentioned, which are, no leſs than thoſe already recited, pertinent to our preſent purpoſe.

In the firſt place then, when the greater part of the air had been pumped out of the receiver, the riſing bubbles aſcended ſo very ſlow in the pipe, that their progreſs was ſcarce diſcernible; which ſeemed to proceed from this, that their bigneſs was ſuch, that they could not ſufficiently extend themſelves in the cavity of the glaſs, without preſſing on both hands againſt the ſides of it, whereby they became of more difficult extruſion to the water. And though it may ſeem ſtrange that theſe bubbles ſhould be of any conſiderable bulk, ſince it is like they conſiſted of leſſer parcels of the air lurking in the water, than thoſe that were vigorous enough to make their way through, long before them; yet they were commonly much larger than before, ſome of them being equal in quantity to four or five peas; whether this their increaſe of bulk proceeded from the greater decrement of the preſſure of the air, or from the union of two or three of thoſe numerous bubbles which were then generated below the bottom of the pipe, where we could not ſee what was done among them.

Another thing we noted in our bubbles was, that whereas in ordinary ones the air, together with the thin film of water that inveſts and detains it, is wont to ſwell above the ſurface of the water it ſwims on, and commonly to conſtitute hemiſpherical bodies with it, the little parcels of air that came up after the receiver was pretty well emptied, did not make protuberant bubbles, but ſuch whoſe upper ſurface was either level with, or beneath that of the water; ſo that the upper ſurface being uſually ſomewhat convex, the leſs protuberant parts of it had a pretty quantity of water remaining above them.

We alſo farther obſerved, that whereas in the bubbles that firſt appeared in the pipe, the aſcending air did, as in other common bubbles, make its way upwards by dividing the water through which it paſſed, in thoſe bubbles that appeared at the latter end of our experiment, when the preſſure of the little external air, remaining in the receiver, was grown inconſiderable; the aſcending parcels of air having now little more than the weight of the incumbent water to ſurmount, were able both ſo to expand themſelves as to fill up that part of the pipe which they pervaded, and by preſſing every way againſt the ſides of it, to lift upwards with them what water they found above them, without letting any conſiderable quantity glide down along the ſides of the glaſs: ſo that ſometimes we could ſee a bubble thruſt on before it a whole cylinder of water of perhaps an inch high, and carry it up to the top of the pipe; though, as we formerly noted, upon the letting in the external air, theſe tumid bubbles ſuddenly relapſed to their former inconſpicuouſneſs.

ALL theſe things laid together ſeemed ſufficiently to confirm that, which the conſideration of the thing itſelf would eaſily enough perſuade; namely, that the air, and ſuch like bodies being under water, may be preſſed upon as well by the atmoſphere, as by the weight of the incumbent water itſelf.

HENCE likewiſe we may verify what we obſerved at the cloſe of the foregoing experiment; namely, that from the ſole ſwelling of water there recorded, it cannot be ſo ſafely concluded, that water, when freed from compreſſion, is endowed with an elaſtical power of expanding itſelf: ſince thereby it appears that the intumeſcence produced by that experiment, may (at leaſt in great part) be aſcribed to the numerous little bubbles which are wont to be produced in water, from which the preſſure of the atmoſphere is in great meaſure taken off. So apt are we to be miſled, even by experiments themſelves, into miſtakes, when either we conſider not that moſt effects may proceed from various cauſes, or mind only thoſe circumſtances of our experiment, which ſeem to comply with our preconceived hypotheſis or conjectures.

AND hence it ſeems alſo probable, that in the pores or inviſible little receſſes of water itſelf there lie commonly interſperſed many parcels of either air, or at leaſt ſomething analogous thereunto, although ſo very ſmall, that they have not been hitherto ſo much as ſuſpected to lurk there. But if it be demanded how it appears that there is interſperſed through the body of water any ſubſtance thinner than itſelf, and why that which produced the bubbles above mentioned ſhould not be reſolutely ſaid to be nothing elſe than a more active and ſpirituous part of the water; we ſhall, in order to the elucidation of this matter, ſubjoin, to what was formerly delivered, the following experiment.

EXPERIMENT XXII.

WE recited in our nineteenth experiment, how by drawing moſt of the air out of the receiver, we made the water ſubſide by degrees in a glaſs not four foot long: we ſhall now add, that in the like experiment made in ſuch a tube, or a greater, it may be obſerved, that when the water begins to fall, there will appear ſtore of bubbles faſtened all along to the ſides of the glaſs; of which bubbles, by the agitation of the veſſel conſequent upon pumping, there will ariſe good numbers to the top of the water, and thereby break; and as the cylinder of water is brought to be lower and lower, ſo the bubbles will appear more numerous in that part of the tube which the water yet fills; and the nearer the ſurface of the water, in its deſcent, approacheth to theſe bubbles, the greater they will grow, becauſe, having the leſs weight and preſſure upon them, the expanſion of that air which makes them, can be the leſs reſiſted by the preſſure of the incumbent water and air; as ſeems probable from hence, that upon the letting in a little external air, thoſe bubbles immediately ſhrink.

IT may indeed, as we lately intimated, be conjectured, that theſe bubbles proceed not ſo much from any air pre-exiſtent in the water, and lurking in the pores of it, as from the more ſubtle parts of the water itſelf; which, by the expanſion allowed them upon the diminiſhed preſſure of the ambient bodies, may generate ſuch bubbles. And indeed, I am not yet ſo well ſatisfied that bubbles may not (at leaſt ſometimes) have ſuch an origination: but that which makes me ſuſpect that thoſe in our trials contain real air, formerly latitant in the pores of the water, is this, that upon the inletting of the external air, the water was not again impelled to the very top of the tube whence it began to fall, but was ſtopped in its aſcent near an inch beneath the top. And ſince, if the upper part of the tube had been devoid of any other than ſuch ethereal matter as was ſubtle enough freely to penetrate the pores of the glaſs, the external air would have been able to impel the water to the top of a tube ſeven or eight times as long as ours was; the phænomenon under conſideration ſeemed manifeſtly to argue, that the many bubbles that broke at the top of the water, did contain a real air, which being collected into one place, and hindered by the top of the glaſs from receding, was able to withſtand the preſſure of the outward air. As we ſee that if never ſo little air remain in the tube upon the making the experiment *de vacuo* with quickſilver, no inclining of the tube (though a long one) will enable a man to impel the mercury up to the very top, by reaſon (as we formerly noted) of the reſiſtance of the included air, which will not be compreſſed beyond a certain degree.

BUT in order to a farther diſcovery what our bubbles were, we will, on this occaſion, inform your Lordſhip, that we tried the XIXth experiment in one of our ſmall receivers, and found, that upon the drawing down of the water, ſo many bubbles diſcloſed themſelves, and broke into the upper part of the tube, that having afterwards let in the external air, the water was not thereby impelled to the top of the tube (three foot in length) within a little more than half an inch. And whether or no it were air that poſſeſſed that ſpace at the top of the tube, which was not filled with water, we took this courſe to examine. We drew the ſecond time the air out of the receiver, and found, that by reaſon of the body that poſſeſſed the top of the tube, we were able not only to make the water in the tube fall to a level with the ſurface of the water in the veſſel, but alſo (by plying the pump a little longer) a great way beneath it; which, ſince it could not well be aſcribed to the bare ſubſiding of the water, by reaſon of its own weight, argued that the water was depreſſed by the air: which was confirmed by the figure of the ſurface of the water in the tube, which was much more concave than that of water in tubes of that bigneſs uſeth to be. And this farther trial (to add that upon the bye) we made at the ſame time; that when the water in the pipe was drawn down almoſt as low as the water without it, we obſerved, that (though we deſiſted from pumping) by the bare application of a hand moderately warm to the deſerted part of the tube, the remaining water would be ſpeedily and notably depreſſed. And having for a while held a kindled coal to the outſide of the tube, (the pump being ſtill unimployed, becauſe the veſſel chanced to hold extraordinarily well) the air was by the heat ſo far expanded, that it quickly drove the water to the bottom of the tube, which was divers inches beneath the ſurface of the ambient water. Whereby it appears (by the ſame way by which we formerly meaſured the dilatation of the air) that the air, even when it is expanded to between 90 and 100 times its extent, will yet readily admit of a much farther rarefaction by heat.

I conſidered alſo that in caſe the bubbles we have been ſpeaking of, were produced by the parcels of air latitant in the water, that air being now got together to the top of the tube, though the air were again drawn out of the receiver, the taking off its preſſure would not diſcloſe bubbles as before; and accordingly, the air being again pumped out, the water in the tube deſcended as formerly; but for a great while we ſcarce ſaw one bubble appear, only when the receiver had been very much exhauſted, and the water was fallen very low, there appeared near the bottom of the tube, certain little bubbles, which ſeemed to conſiſt of ſuch parcels of air as had not, by reaſon of their ſmallneſs, got up to the top of the water, with the more bulky and vigorous ones. And that which is not inconſiderable, is, that having, by letting in the air, forced up the water into the tube, we could not perceive that it aſcended near the top, though we permitted the engine to remain unimployed for two or three nights together, and watched whether the water would ſwell up and fill the tube. And on this occaſion I remember, that having tried ſuch an experiment as this with quickſilver inſtead of water, in a tube of about a foot and a half long, wherein it might ſeem more hopeful to eſcape bubbles; yet upon the drawing down the quickſilver as low as we could, and letting in the external air upon it, we found that ſome lurking particles of air were got up to the top of the tube, and hindered the quickſilver from being forced up again ſo high. And though the quickſilver were by this means brought to appear a very cloſe and lovely metalline cylinder, not interrupted by interſperſed bubbles as before; yet having cauſed the air to be again drawn out of the receiver, I could perceive ſeveral little bubbles to diſcloſe themſelves, faſtened to the inſide of the tube, near the bottom of it; and having purpoſely watched one or two of the chiefeſt, I had the pleaſure to obſerve, that though they grew bigger and bigger as the ſurface of the mercurial cylinder fell nearer and nearer to them, ſo as that at length they ſwelled into a conſpicuous bulk; yet upon the wary letting in the air upon them, they did not break, but preſently ſhrunk up into a littleneſs that rendered them inconſpicuous.

WHENCE it ſeems very probable, if not certain, that even in the cloſeſt and moſt ponderous liquors, and therefore much more in water, there may lurk undiſcernable parcels of air, capable, upon the removal of the preſſure of the ambient air (though but in part) and that of the liquor wherein it lurks, to produce conſpicuous bubbles. And conſequently, if it ſeem inconvenient to admit an elaſtical power in the water, it may be ſaid that the ſwelling of the compreſſed water in the pewter veſſel lately mentioned, and the ſpringing up of the water at the hole made by the needle, were not the effects of any internal *Elater* of the water, but of the ſpring of the many little particles of air diſperſed through that water, and acting upon it in their ſudden recovering themſelves to a greater extent, than that to which a violent compreſſion had reduced them.

BUT though, from all theſe particulars, it ſeems manifeſt that the bubbles we have been all this while treating of, were produced by ſuch a ſubſtance as may be properly enough called air; yet till we ſhall have had the opportunity of making ſome farther trials concerning the nature of the air, we ſhall not reſolutely determine whether or no air be a primogenial body (if I may ſo ſpeak) that cannot now be generated or turned either into water or any other body. Yet in the mean while (becauſe it is an important queſtion, and, if rightly determined, may much conduce to the knowledge of the nature of the air) we think it not unfit to make a brief mention of ſome of the particulars which at preſent occur to our thoughts in favour of either part of the queſtion.

FIRST then, divers Naturaliſts eſteem the air (as well as other elements) to be ingenerable and incorruptible. And reaſons plauſible enough may be drawn to countenance this opinion, from the conſideration of that permanency that ought to belong to the corporeal principles of other bodies.

NEXT, experience may be pleaded to the ſame purpoſe, for I have read of ſome who in vain attempted to turn air into water, or water into air.

Schottus Mechan. hydraulico-pneumat. Part. 3. Claſſ. 1.

The diligent *Schottus* tells us, that amongſt other rarities to be met with in that great repoſitory of them, the *Muſæum Kircherianum*, there is a round glaſs with a tapering neck near half full (as one may gueſs by the ſcheme he annexeth) of ordinary ſpring water, which having been hermetically ſhut up there by *Clavius* the famous geometrician, the included water is to this day preſerved, not only clear and pure, as if it were but newly put in; but (as it ſeems) without (in the leaſt) turning into air, notwithſtanding its having been kept there theſe fifty years: for he tells us, that the water hath continued there all this while without any diminution.

Nor doth it appear in thoſe glaſſes, which for chymical experiments we uſually cloſe with *Hermes* his ſeal (as they call it) that the included air doth, during its long impriſonment, notwithſtanding the alteration it receiveth from various degrees of heat, diſcernably alter its nature. Whereas we plainly perceive in our digeſtions and diſtillations, that though water may be rarefied into inviſible vapours, yet it is not really changed into air, but only divided by heat, and ſcattered into very minute parts, which meeting together in the alembic or in the receiver, do preſently return into ſuch water as they conſtituted before. And we alſo ſee, that even ſpirit of wine, and other ſubtle and fugitive ſpirits, though they eaſily fly into the air, and mingle with it, do yet in the glaſſes of chymiſts eaſily lay aſide the diſguiſe of air, and reſume the diveſted form of liquors. And ſo volatile ſalts, as of urine, hartſhorn, *&c.* though they will readily diſperſe themſelves through the air, and play up and down in the capacity of an alembic or a receiver; yet will they, after a while, faſten themſelves to the inſides of ſuch glaſſes in the form of ſalts.

Besides, ſince air is confeſſedly endowed with an elaſtical power that probably proceeds from its texture, it appears not what it is, that in ſuch light alterations of water, as are by many preſumed capable of turning it into air, can be reaſonably ſuppoſed ſo to contrive the particles of water, as to give them, (and that permanently) the ſtructure requiſite to a ſpring. I add the word permanently, becauſe the newly mentioned obſervations ſeem to argue the corpuſcles of air to be irreducible into water, whereas the aqueous particles may perhaps for a while be ſo vehemently agitated, as to preſs almoſt like ſprings upon other bodies; yet upon the ceaſing of the agitation, they quickly, by relapſing into water, diſcloſe themſelves to have been nothing elſe whilſt they counterfeited the air.

Lastly, the experiment formerly made in our engine with a piece of match, ſeems to evince, that even thoſe light and ſubtile fumes (for the moſt part not aqueous neither) into which the fire itſelf ſhatters dry bodies, have no ſuch ſpring in them as the air, ſince they were unable to hinder or repreſs the expanſion of the air included in the bladder they ſurrounded.

Natural and Moral Hiſt. of the Indies, Lib. 3. c. 9.

Georg. General. Lib. 1. cap. 19.

I remember indeed, that the learned *Joſephus Acoſta*, in his hiſtory of the *Weſt Indies*, tells us, that he ſaw in thoſe parts ſome grates of iron ſo ruſted and conſumed by the air, that the metal being preſſed between the fingers, diſſolved (to uſe his words) to powder, as if it had been hay or parched ſtraw. And I remember too, that the accurate *Varenius* tells us, that in the iſlands commonly called *Azores*, the air (and wind) is ſo ſharp, that in a ſhort time it frets not only iron plates, but the very tiles upon the roofs of houſes, and reduceth them to duſt. And I have elſewhere mentioned ſome recent obſervations of this kind. But it may be ſaid, that the abovementioned authors aſcribe the recited effects chiefly to the winds, and that, however the corroſion of the iron and the tiles may proceed not from the air itſelf, or any of its genuine parts, but from ſome ſaline corpuſcles diſperſed through the air, and driven by the winds againſt the bodies it is preſumed to fret. And that ſuch volatile ſalts may copiouſly aſcend into the air, and yet retain their nature, as doth the more fixt ſalt in the ſea-water, the ſublimations of ſal-armoniac may ſufficiently evince. Not to mention, that I have ſhewn ſome friends a ſecret kind of ſaline ſubſtance incomparably ſubtler than ſal-armoniac, which did not only eaſily enough aſcend itſelf, but carried up with it (and that in a very great proportion) the ſolid and ponderous body even of uncalcined gold in the form of ſubtile exhalations, which did afterwards faſten themſelves to the upper parts of the veſſels, and yet manifeſt themſelves to continue gold. We remember alſo, that to try whether water could be turned into air, we once took an æolipile, into which we had before conveyed ſome water, and placing it upon kindled coals when the heat forced out a vehement ſtream of aqueous vapours; we tied about the neck of it, that of a bladder, which we had before emptied of air; and finding the æolipile after a while to blow up the bladder, we carefully tied it again, that the included ſubſtance might not get away. Then ſlipping it off from the æolipile, we conveyed it into our receiver, to try whether or no that, which in part diſtended the bladder, would appear by its ſpring to be true air: whereby we found that upon the exſuction of the ambient air, the included ſubſtance expanded itſelf and the bladder to a very much greater bulk than it was of before. And for farther ſatisfaction, having again taken out the bladder, we ſuffered it to remain tied up till next morning, to try whether time, and the coldneſs of the night, would make the contained ſubſtance relapſe into water: but the next morning we found it little leſs tumid than before. I remember, I ſay, that I once made this experiment; but I might ſay in anſwer to it, that the chief reaſon of my mentioning it, is, to let your Lordſhip ſee, how requiſite it is to be circumſpect and conſiderate, when we are to make and to build upon nice experiments. For though I may ſeem to have uſed ſufficient caution, yet afterward conſidering with myſelf that the æolipile I had imployed was a very large one, and that it required much more care than one that hath not tried it would imagine, to drive out all the air from a large æolipile, I eaſily ſuſpected that the diſtenſion of the bladder in our pneumatical veſſel, might proceed not from the watery ſteams that came out at the narrow mouth of the æolipile, and had very much wetted the bladder, but from the rarefied air which in that ſort of veſſels is wont for a good while together to come out with the rarefied water: and accordingly having reiterated the experiment, I found it very difficult (by reaſon of the ſhrinking of the bladders upon their being heated, and of other experiments) to make it ſo accurately as to deduce from it, that water may be rarefied into true air.

AGAINST the other four above-mentioned conſiderations, we cannot ſpend time to frame objections, but muſt forthwith proceed to the mention of thoſe things that ſeem to argue that air (at leaſt ſuch as produced our bubbles) may be generated of water and other bodies.

FIRST then, we have found by experience that a vapid air, or water rarefied into vapour, may at leaſt for a while emulate the elaſtical power of that which is generally acknowledged to be true air. For if you take a good æolipile, with a moderately ſtrong and ſlender neck, and filling it with water, lay it upon quick coals, you may, after a while, obſerve ſo great a preſſure by ſome of the parts contained in the æolipile upon others, that the water will ſometimes be thrown up into the air above three or four foot high; and if you then take the æolipile almoſt red-hot from off the fire, you may perceive that the water will, for a longer time than one would eaſily imagine, continue to be ſpouted out in a violent ſtream. And if there remains but little water in the æolipile when it is taken very hot from the fire, immerſing the neck of it into cold water, you will find, that after it begins to ſuck in ſome water, there will be made from time to time ſtore of large bubbles in that water wherein the neck was plunged. Which bubbles ſeem manifeſtly to proceed from hence, that for a while the heat in the æolipile continues ſtrong enough to rarefy part of the water that is ſucked in, and expel it in the form of vapours through the water incumbent on the pipe. If alſo when the æolipile is almoſt full of water, and therefore can contain but little air; you hold a coal or brand in that ſtream of vapours that iſſues out of the narrow mouth of it, you will find this vapid or rorid air (if I may ſo call it) to blow the fire very ſtrongly, and with a roaring noiſe. And that it be not ſaid that it is by the external air which the aqueous ſteams drive before them, and not by the ſteams themſelves, that the blaſt is made and the flame excited; it hath been obſerved, that by approaching the coal or brand almoſt to the mouth of the æolipile, the wind appeared more vehement, than if the body to be kindled were held ſome inches off.

BUT in regard the elaſtical power of the ſtream, iſſuing out of an æolipile, ſeems manifeſtly due to the heat that expands and agitates the aqueous particles whereof that ſtream conſiſts, and that ſuch rapid winds ſeem to be but water ſcattered into little parts and ſet a-moving; ſince we find, that holding a knife, or any ſolid, ſmooth, and cloſe body againſt the ſtream that iſſues out of the æolipile, the vapours condenſing upon it, will preſently cover it with water: it will be very pertinent to ſubjoin a notable experiment that I remember I have met with, in the deſcription given us by the induſtrious *Kircher*, of ſeveral muſical engines. And (though it may ſeem ſomewhat prolix) we will recite what he delivers in his own words, which are theſe.

Kirch. Art. Mag. Con. & Diſſon. *lib.* 9. *p.* 309. *See the fifteenth figure.*

Cum eodem tempore quo hæc ſcripſi, ſumma Pont. Innocentii X^{mi} *mandato organi hydraulici in horto Quirinali conſtituendi cura mihi commendata eſſet. Æoliam cameram inſigni ſanè ſucceſſu conſtrui juſſimus, eâ quæ ſequitur ratione.*

Erat longitudo ſive altitudo cameræ A H 5 *pedum latitudine* 3 *ferè ex lateribus conſtructa; in medio duo tenebat diaphragmata* C D & E F *in modum cribri pluribus foraminibus pertuſi. Paulo infra canalis* G *aquam advehens inſerebatur in* H *eidem epiſtomium parabat exitum. Aqua itaque per canalem* G *maximo impetu ruens vehementiſſimum ventum mox intus excitabat; qui ventus nimiâ humiditate imbutus, ut purior exiret ſicciórque, diaphragmata illa in cribri modum pertuſa, ordinata ſunt. Intra hæc enim aquæ vehemens agitatio rupta fractâque aërem puriorem per* A *canalem ſubtiliorémque emittebat: verum cum poſtea inventum ſit aërem plus æquo humidum interioribus organi meatibus maximum detrimentum inferre: hinc, ut aër aquoſus ſicciſſimam conſiſtentiam acquireret, ordinavimus canalem plumbeum* Q R *in helicem contortum vaſi* S *aliquantulum capaciori in modum urnæ efformato, inſertum. Intra urnam enim plumbeam & canalem tortuoſum illiſus aër humidus, ita ab omni aquoſitate defæcabatur, ut ex furno in organum derivatus dici potuerit. Urna* S *canalis tortuoſi* Q R *ultimum orificium* Z *inſeritur anemothecæ organi. Et hanc modum organis hydraulicis omnium aptiſſimum reperi.*

Debet autem camera illa ſituari in loco quantum fieri poteſt ſicciori, ita ut longo canali aqua intra eam derivetur, ne locus humiditate ſuâ organis officiat.

THUS far the ingenious *Kircherus*, whom I the rather cite, becauſe although I have been informed of divers vendiducts (as they call them) by very knowing travellers that have obſerved them; yet this relation of our author being very punctual, and delivered upon his own particular experience, hath, I confeſs, made me wiſh I had had the good fortune when I was at *Rome*, to take notice of theſe organs; or that I had now the opportunity of examining of ſuch an experiment. For, if upon a ſtrict enquiry I ſhould find that the breath that blows the organs doth not really upon the ceaſing of its unuſual agitation by little and little relapſe into water, I ſhould ſtrongly ſuſpect that it is poſſible for water to be eaſily turned into air. I remember indeed, that we have formerly taught, that there lurks an interſperſed air in the pores of otdinary water, which may poſſibly be ſtruck out by the breaking of the water in its fall into the Æolian chamber, (as he calls it.) But in regard the ſcheme ſeems to repreſent that chamber as cloſely ſhut, and thereby forbids us to ſuppoſe that any air is carried into it, but what is latitant in the water, it will ſcarce ſeem probable (to him who remembers how ſmall a proportion of air that appeared to be, when its rarefication ceaſed, which was concealed in the water we freed from bubbles in our receiver) that ſo little air as is commonly diſperſed through water, ſhould be able, in ſo little water as was requiſite for ſo ſmall a room, to make ſo vehement a wind as our author here tells us of. I have ſometime therefore ſuſpected, that in this caſe the wind may be produced by ſmall particles of the water itſelf, forcibly expelled out of the chamber into the organs. And to the objection, to which I foreſaw this gueſs to be liable, namely, that, no heat intervening, there appeared nothing that ſhould raiſe the water into exhalations, and give them an impulſe; I thought it might be ſaid, that motion alone, if vehement enough, may, without ſenſible heat, ſuffice to break water into very minute parts, and make them aſcend upwards, if they can no where elſe more eaſily continue their agitation. For I remember that travelling betwixt *Lyons* and *Geneva*, I ſaw, not very far out of the way, a place where the river of *Rhone*, coming ſuddenly to be ſtraitened betwixt two rocks, ſo near each other, that a man may, (if my memory fail me not) ſtand aſtride upon both at once; that rapid ſtream daſhing with great impetuoſity againſt its rocky boundaries, doth break part of its water into ſuch minute corpuſcles, and put them into ſuch a motion, that paſſengers obſerve at a good diſtance off, as it were a miſt ariſing from that place, and aſcending a good way up into the air. Such, I ſay, was my ſuſpicion touching the wind we have been conſidering; but it ſeems ſomething odd that aqueous vapours ſhould, like a dry wind, paſs through ſo long and tortuous a pipe of lead, as that deſcribed by our author, ſince we ſee in the heads of ſtills, and the necks of æolipiles, how quickly ſuch vapours are even by a very little cold recondenſed into water. But to this alſo ſomething may be ſpeciouſly replied; wherefore contenting myſelf to have mentioned our author's experiment as a plauſible, though not demonſtrative proof, that water may be tranſmuted into air, we will paſs on to mention in the third place another experiment, which we tried in order to the ſame enquiry.

We took a clear glaſs bubble (capable of containing by gueſs about three ounces of water) with a neck ſomewhat long and wide, of a cylindrical form; this we filled with oil of vitriol and fair water, of each almoſt a like quantity, and caſting in half a dozen ſmall iron nails, we ſtopt the mouth of the glaſs, (which was top full of liquor) with a flat piece of diapalma provided for the purpoſe, that, accommodating itſelf to the ſurface of the water, the air might be exquiſitely excluded: and ſpeedily inverting the phial, we put the neck of it into a ſmall wide-mouthed glaſs, that ſtood ready, with more of the ſame liquor in it, to receive it. As ſoon as the neck had reached the bottom of the liquor it was dipped into, there appeared at the upper part (which was before the bottom) of the phial a bubble, of about the bigneſs of a pea, which ſeemed rather to conſiſt of ſmall and recent bubbles, produced by the action of the diſſolving liquor upon the iron, than any parcel of the external air that might be ſuſpected to have got in upon the inverſion of the glaſs, eſpecially ſince we gave time to thoſe little particles of air which were carried down with the nails into the liquor to fly up again. But whence this firſt bubble was produced, is not ſo material to our experiment, in regard it was ſo ſmall: for ſoon after we perceived the bubbles produced by the action of the menſtruum upon the metal, aſcending copiouſly to the bubble named, and breaking into it, did ſoon exceedingly increaſe it, and by degrees depreſs the water lower and lower, till at length the ſubſtance contained in theſe bubbles poſſeſſed the whole cavity of the glaſs phial, and almoſt of its neck too, reaching much lower in the neck than the ſurface of the ambient liquor, wherewith the open-mouthed glaſs was by this means almoſt repleniſhed. And becauſe it might be ſuſpected that the depreſſion of the liquor might proceed from the agitation whereinto the exhaling and impriſoned ſteams were put, by that heat which is wont to reſult from that action of corroſive ſalts upon metals, we ſuffered both the phial and the open-mouthed glaſs to remain as they were, in a window, for three or four days and nights together; but looking upon them ſeveral times during that while, as well as at the expiration of it, the whole cavity of the glaſs bubble, and moſt of its neck, ſeemed to be poſſeſſed by air, ſince by its ſpring it was able for ſo long to hinder the expelled and ambient liquor from regaining its former place. And it was remarkable, that juſt before we took the glaſs bubble out of the other glaſs, upon the application of a warm hand to the convex part of the bubble; the impriſoned ſubſtance readily dilated itſelf like air, and broke through the liquor in divers bubbles ſucceeding one another.

Having alſo another time tried the like experiment with a ſmall phial, and with nails diſſolved in aqua fortis, we found nothing incongruous to what we have now delivered. And this circumſtance we obſerved, that the newly generated ſteams did not only poſſeſs almoſt all the whole cavity of the glaſs, but divers times without the aſſiſtance of the heat of my hand, broke away in large bubbles through the ambient liquor into the open air: ſo that theſe experiments with corroſive liquors, ſeemed manifeſtly enough to prove, though not that air may be generated out of the water, yet that in general air may be generated anew.

Lastly, to the foregoing arguments from experience we might eaſily ſubjoin the authority of *Ariſtotle*, and of (his followers) the ſchools, who are known to have taught, that air and water being ſymbolizing elements, (in the quality of moiſture) are eaſily tranſmutable into one another. But we ſhall rather to the foregoing argument add this, drawn from reaſon, that if, as *Leucippus*, *Democritus*, *Epicurus* and others, followed by divers modern Naturaliſts, have taught, that the difference of bodies proceeds but from the various magnitudes, figures, motions, and textures of the ſmall parts they conſiſt of, (all the qualities that make them differ, being deducible from thence) there appears no reaſon why the minute parts of water, and other bodies, may not be ſo agitated or connected as to deſerve the name of air. For if we allow the Carteſian hypotheſis, according to which as we noted at the beginning of this letter, the air may conſiſt of any terrene or aqueous corpuſcles, provided they be kept ſwimming in the interfluent celeſtial matter; it is obvious that air may be as often generated as terreſtrial particles, minute enough to be carried up and down, by the celeſtial matter, aſcend into the atmoſphere. And if we will have the air to be a congeries of little ſlender ſprings, it ſeems not impoſſible, though it be difficult, that the ſmall parts of divers bodies may by a lucky concourſe of cauſes be ſo connected, as to conſtitute ſuch little ſprings, ſince (as we note in another treatiſe) water in the plants it nouriſheth is uſually contrived into ſpringy bodies, and even the bare altered poſition and connection of the parts of a body may ſuffice to give it a ſpring that it had not before, as may be ſeen in a thin and flexible plate of ſilver; unto which, by ſome ſtrokes of a hammer, you may give a ſpring, and by only heating it red-hot, you may make it again flexible as before.

THESE, my Lord, are ſome of the conſiderations at preſent occurring to my thoughts, by which it may be made probable, that air may be generated anew. And though it be not impoſſible to propoſe objections againſt theſe, as well as againſt what hath been repreſented in favour of the contrary doctrine; yet having already almoſt tired myſelf, and I fear more than almoſt tired your Lordſhip with ſo troubleſome an inquiry after the nature of bubbles, I ſhall willingly leave your Lordſhip to judge of the arguments alledged on either ſide; and I ſhould ſcarce have ventured to entertain you ſo long concerning ſuch empty things as the bubbles, which have occaſioned all this diſcourſe, but that I am willing to invite you to take notice with me of the obſcurity of things, or the dimneſs of our created intellects (which yet of late too many ſo far preſume upon, as either to deny or cenſure the Almighty and Omniſcient Creator himſelf) and to learn hence this leſſon, that there are very many things in nature that we diſdainfully overlook as obvious or deſpicable, each of which would exerciſe our underſtandings, if not poſe them too, if we would but attentively enough conſider it, and not ſuperficially contemplate, but attempt ſatisfactorily to explicate the nature of it.

EXPERIMENT XXIII.

SINCE the writing of the twenty-firſt and twenty-ſecond experiments (and notwithſtanding all that hath been on their occaſion delivered concerning bubbles) we made ſome farther trials in proſecution of the ſame inquiry whereto they were deſigned.

WE choſe then, amongſt thoſe glaſſes which chymiſts are wont to call philoſophical eggs, one that containing about nine ounces of water, had a neck of half an inch in diameter at the top, and as we gueſſed, almoſt an inch at the bottom; which breadth we pitched upon for a reaſon that will by and by appear: then filling it up with common water to the height of about a foot and an half, ſo that the upper part remained empty, we ſhut it into the receiver, and watched what would follow upon pumping, which proved that a great part of the air being drawn out, the bubbles began to diſcover themſelves at the bottom and ſides of the glaſs; and increaſing, as the air was more and more drawn away, they did from time to time aſcend copiouſly enough to the top of the water, and there quickly break; but by reaſon that the wideneſs of the glaſs allowed them free paſſage through the water, they did not appear as in the former experiments to make it ſwell: the water ſcarce ever riſing at all above the mark affixed to its upper ſurface, when it was put in; and upon the return permitted to the outward air, and conſequently the ſhrinking in of the remaining bubbles, the water ſeemed to have loſt of its firſt extent, by the avolation of the formerly interſperſed air.

BEING willing likewiſe to try whether diſtilled water were, by having been divided into minute parts, and then reunited, more or leſs diſpoſed to expand itſelf than water not diſtilled; we took out of our laboratory ſome carefully diſtilled rain-water, and put about two ounces of it into a round glaſs-bubble, with a very ſmall neck (not exceeding the ſixth part of an inch in diameter) which we filled half way to the top, and then conveyed it into the receiver; the iſſue was, that though we drew out more air than ordinary, yet there appeared not the leaſt intumeſcence of the water, nor any aſcending bubbles.

BUT ſuſpecting that either the ſmall quantity of the water or the figure of the veſſel might have an intereſt in this odd phænomenon, we took the lately mentioned philoſophical egg, and another not much differing from it; the former we filled up with diſtilled rain-water to the old mark, and into the latter we put a long cylinder or rod of ſolid glaſs to ſtraiten the cavity of the neck by almoſt filling it up; and then pouring ſome diſtilled water into that alſo, till it reached within ſome fingers breadth of the top, the eggs were let down into the receiver. In this experiment the air was ſo far drawn forth, before there appeared any bubble in either of the glaſſes, that the diſparity betwixt this and common water was manifeſt enough. But at length, when the air was almoſt quite pumped out, the bubbles began to diſcloſe themſelves, and to increaſe as the preſſure of the air in the receiver decreaſed. But whereas in the firſt mentioned philoſophical egg the bubbles were very ſmall, and never able to ſwell the water, that we took notice of, at all above the mark; in the other, whoſe neck, as we lately ſaid, was ſtraitned, and their paſſage obſtructed, great numbers of them, and bigger, faſtened themſelves to the lower end of the glaſs-rammer (if we may ſo call it) and gathered in ſuch numbers between that and the ſides of the neck, that the water ſwelled about a finger's breadth above the mark, though upon the admitting of the external air it relapſed to the former mark, or rather fell ſomewhat below it. And although thereupon in the firſt named veſſel all the bubbles preſently diſappeared, yet in the other we obſerved, that divers remained faſtened to the lower part of the glaſs-rammer, and continued there, ſomewhat to our wonder, for above an hour after, but contracted in their dimenſions.

MOREOVER, having ſuffered the glaſſes to remain above twenty-four hours in the receiver, we afterwards repeated the experiment to try what change the exſuction of the external air would produce in the water, after the internal and latitant air had (as is above recited) in great meaſure got away in bubbles; and whether or no the water would, by ſtanding, readmit any new particles of air in the room of thoſe that had forſaken it. But though we exhauſted the receiver very diligently, yet we ſcarce ſaw a bubble in either of the glaſſes; notwithſtanding which, we perceived the water to riſe about the breadth of a barley-corn, or more, in the neck of that glaſs wherein the ſolid cylinder had been put; the liquor in the other glaſs not ſenſibly ſwelling.

AND laſtly, upon the letting in of the air, the water in the ſtraitened neck ſoon ſubſided to the mark above which it had ſwollen; which whether it ought to be aſcribed to the ſame ſmall expanſion of the parts of the water itſelf, or to the rarefaction of ſome yet latitant air broken into ſuch ſmall particles, as to eſcape our obſervation, ſeems not eaſily determinable, without ſuch farther trials, as would perhaps prove tedious to be recited as well as to be made; though I was content to ſet down thoſe already mentioned, that it might appear, how requiſite it is in nice experiments to conſider variety of circumſtances.

EXPERIMENT XXIV.

AFTER having thus diſcovered what operation the exſuction of the ambient air had upon water, we thought good to try alſo what changes would happen in other liquors upon the like taking off the preſſure of the external air. We took then a glaſs egg, ſomewhat bigger than a turkey's egg, which had a long neck or ſtem of about a third part of an inch in diameter; and filling it up with ſallad-oil, until it reached above half way to the top of the neck, we incloſed it in the receiver, together with common water in a reſembling veſſel, that we might the better compare together the operation of the exſuction of the air upon thoſe two liquors. The pump being ſet on work, there began to appear bubbles in the oil much ſooner than in the water, and afterwards they alſo aſcended much more copiouſly in the former liquor than in the latter: nay, and when by having quite tired the pumper, and almoſt our own patience, we gave over, the bubbles roſe almoſt (if not altogether) in as great numbers as ever, inſomnch that none of the various liquors we tried either before or ſince, ſeemed to abound more with aërial particles than did this oil. In which it was farther remarkable, that between the time it was ſet into the receiver, and that at which we could get ready to pump, it ſubſided notably (by gueſs about half an inch) below the mark it reached before it was put in.

After this expreſſed oil, we made trial of a diſtilled one; and for that purpoſe made choice of the common oil or ſpirit (for in the ſhops where it is ſold, the ſame liquor is promiſcuouſly called by either name) of turpentine, becauſe it was only of that chymical oil we had a ſufficient quantity: which being put into a ſmall glaſs-bubble with a ſlender neck, ſo as to fill it about two inches from the top, did, upon the evacuating of the receiver, preſent us with great ſtore of bubbles, moſt of which riſing from the bottom, expanded themſelves exceedingly in their aſcent, and made the liquor in the neck to ſwell ſo much by degrees, that at length it divers times ran over at the top: by which means we were hindered from being able to diſcern upon the letting in of the air, how much the ſubſidence of the oil below the firſt mark was due to the receſs of the bubbles.

Having likewiſe a mind to try, whether as ſtrong a ſolution of ſalt of tartar in fair water as could be made (we having then no oil of tartar *per deliquium* at hand) though it be accounted, quickſilver excepted, the heavieſt of liquors, would afford us any bubbles; we put in a glaſs-egg full of it at the ſame time with other liquors, and found, that they did long yield ſtore of bubbles before any diſcovered themſelves in the liquor of tartar; and having purſued the experiment, it appeared, that of all the liquors we made trial of, this afforded the feweſt and ſmalleſt bubbles.

Spirit of vinegar being tried after the ſame manner, exhibited a moderate number of bubbles, but ſcarce any thing elſe worth the mentioning.

Nor could we in red wine, tried in a glaſs-egg, take notice of any thing very obſervable. For though upon the exſuction of the air the bubbles aſcended in this liquor, as it were in ſhoals, and ſhifted places among themſelves in their aſcent; yet the intumeſcence of the whole bulk of the liquor was ſcarce at all ſenſible, the bubbles moſt commonly breaking very ſoon after their arrival at the top, where during their ſtay they compoſed a kind of ſhallow froth, which alone appeared higher in the neck of the glaſs than was the wine when it was let down. Neither yet did milk, conveyed into our pneumatical veſſel, preſent us with any thing memorable, ſave that (as it ſeemed by reaſon of ſome unctuouſneſs of the liquor) the bubbles not eaſily breaking at the top, and thruſting up one another, made the intumeſcence appear much greater than that of common water.

We likewiſe conveyed hens eggs into the receiver, but after the exſuction of the air, took them out whole again. That which invited us to put them in, was, that (as perhaps we mention in other papers) we had among other experiments of cold, made eggs burſt, by freezing them within doors with ſnow and ſalt; the ice, into which the aqueous parts of the egg were turned by the cold, ſo diſtending (probably by reaſon of the numerous bubbles wont to be obſervable in ice) the outward parts of the egg, that it uſually cracked the ſhell, though the inner membrane, that involved the ſeveral liquors of the egg, becauſe it would ſtretch and yield, remained unbroken. And hereupon we imagined, that in our engine it might appear, whether or no there were any conſiderable ſpring, either in any of the liquors, or in any other more ſpirituous ſubſtance included in the egg.

WE took alſo ſome ſpirit of urine, careleſly enough deflegmed, and put it into the ſame glaſs (firſt carefully ſcoured and cleanſed) wherein we had put the oil olive above mentioned: we took alſo another glaſs, differing from a glaſs egg, only in that its bottom was flat, and filled it up to about $\frac{2}{3}$ of the neck (which was wider than that of the egg) with rectified ſpirit of wine.

WE took alſo another glaſs-egg; and having filled it with common water till it reached to the middle of the neck, we poured to it of the ſame ſpirit of wine, till it reached about an inch higher.

THESE three glaſſes having marks ſet on them, over againſt the edges of the contained liquors, were put into the receiver; and that beginning to be evacuated, the bubbles in all the three liquors began to appear. The mixture of the ſpirit of wine and water diſcloſed a great ſtore of bubbles, eſpecially towards the top; but ſcarce afforded us any thing worth the remembring. The ſpirit of urine appeared to ſwell near an inch and an half above the mark; and beſides that, ſent forth ſtore of bubbles, which made a kind of froth at the upper part of it. And above that ſpume, there appeared eight or ten great bubbles one above another, in a very decent order, each of them conſtituting, as it were, a cylinder of about half an inch high, and as broad as the internal cavity of the neck: ſo that all the upper part part of the neck (for theſe bubbles reached to the top) ſeemed to be divided into almoſt equal parts by certain diaphragms, conſiſting of the coats of the bubbles, whoſe edges appeared like ſo many rings ſuſpended one above another.

In the ſpirit of wine there did ariſe a great multitude of bubbles, even till wearineſs did make us give over the experiment. And in theſe bubbles two or three things were remarkable; as firſt, that they aſcended with a very notable celerity; next, that being arrived at the top, they made no ſtay there: and yet, notwithſtanding the great thinneſs and ſpiritouſneſs of the liquor, did, before they broke, lift up the upper ſurface of it, and for a moment or two form thereof a thin film or ſkin, which appeared protuberant above the reſt of the ſuperficies like a ſmall hemiſphere. Thirdly, that they aſcended ſtreight up; whereas thoſe produced at the lower part of the veſſel, containing the mixture of the water and ſpirit of wine, aſcended with a wavering or wriggling motion, whereby they deſcribed an indented line. Laſtly, it was obſervable in the ſpirit of wine (and we took notice of the like in the oil of turpentine lately mentioned) that not only the bubbles ſeemed to riſe from certain determinate places at the bottom of the glaſs, but that in their aſcenſion they kept an almoſt equal diſtance from each other, and followed one another in a certain order, whereby they ſeemed part of ſmall bracelets, conſiſting of equally little incontiguous beads, the lower end of each bracelet being, as it were, faſtened to a certain point at the bottom of the glaſs.

THE air being ſparingly let into the receiver, the great bubbles formerly mentioned as incumbent upon one another, in that glaſs that contained the ſpirit of urine, were by orderly degrees leſſened till at length they wholly ſubſided. Notwithſtanding the receſs of ſo many bubbles, as broke on the top of the ſpirit of urine, during all the time of the experiment; yet it ſcarcely appeared at all to be ſunk below the mark: nor did the mixture of ſpirit of wine and water conſiderably ſubſide. But that is nothing to what we obſerved in the ſpirit of wine; for not only it conſpicuouſly expanded itſelf in the neck of the veſſel that contained it, notwithſtanding the largeneſs of it, and that the bubbles were wont to break at the top of it almoſt as ſoon as they arrived there; but upon the re-admiſſion of the external air, the ſpirit of wine retained its newly acquired expanſion. And though we let it alone for near an hour together, in expectation that it might ſubſide; yet when we took it out, we found it ſtill ſwelled between a quarter and half an inch above the mark; and although it was not eaſily imaginable, how this phænomenon could proceed from any miſtake in trying the experiment, yet the ſtrangeneſs of it invited me to repeat it with freſh ſpirit of wine. Which, ſwelling in the neck as formerly, I left all night in the receiver, allowing free acceſs to the external air at the ſtop-cock; and the next day found it ſtill expanded as before, ſave that it ſeemed a little lower: which decrement perhaps proceeded from the avolation of ſome of the fugitive parts of ſo volatile a liquor. And for better ſatisfaction having taken out the glaſs, and conſidered it in the open air, and at a window, I could not find, that there were any remaining bubbles, that could occaſion the perſevering and admired expanſion.

EXPERIMENT XXV.

BEING deſirous to diſcover what difference there might be, as to gravity and levity, between air expanded under water, and itſelf before ſuch expanſion; we took two very ſmall phials, ſuch as chymical eſſences (as they call them) are wont to be kept in, and of the ſize and ſhape expreſſed by the eighth figure. Into one of theſe we put ſo much of a certain ponderous mercurial mixture (happening to be then at hand) that the mouth being ſtopt with a little ſoft wax, the glaſs would juſt ſink in water and no more. This we let fall to the bottom of a wide-mouthed cryſtal jar, filled with about half a pint of common water, and into the ſame veſſel we ſunk the other eſſence-glaſs unſtopped, with as much water in it as was more than ſufficient to make it ſubſide. Both theſe ſunk with their mouths downwards, the former being about three quarters full of air, the latter containing in it a bubble of air, that was gueſſed to be of the bigneſs of half a pea: this done, the wide-mouthed glaſs was let down into the receiver, and the way of employing the engine was carefully made uſe of.

THE ſucceſs was, that having drawn out a pretty quantity of air, the bubbles began to diſcloſe themſelves in the water, as in the former experiments; and though for a good while after the bubbles aſcended in ſwarms from the lower parts of the water, and haſtily broke at the top; yet we proſecuted the experiment ſo long without ſeeing any effect wrought upon the eſſence-bottles, that we began to deſpair of ſeeing of them riſe. But continuing to ply the pump, that little glaſs, whoſe mouth was opened, came to the top of the water, being, as it were, buoyed up thither by a great number of bubbles, that had faſtened themſelves to the ſides of it; ſwimming thus with the mouth downward, we could eaſily perceive, that the internal air above mentioned had much dilated itſelf, and thereby ſeemed to have contributed to the emerging of the glaſs, which remained floating, notwithſtanding the breaking and vaniſhing of moſt of the contiguous bubbles. Being hereby encouraged to perſiſt in pumping, we obſerved, with ſome pleaſure, that at each time we turned the key, the air in the little glaſs did manifeſtly expand itſelf and thruſt out the water, generally retaining a very protuberant ſurface, where it was contiguous to the remaining water. And when after divers exſuctions of the air in the receiver, that in the phial ſo dilated itſelf as to expel almoſt all the water, it turned up its mouth towards the ſurface of the water in the jar, and there delivered a large bubble, and then relapſed into its former floating poſture. And this experiment taught us, among other things, that it was a work of more time and labour than we imagined, to exhauſt our engine as much as it may be exhauſted: for although before the emerging of the ſmall phial, we did (as hath been touched already) think we had very conſiderably emptied the receiver, becauſe there ſeemed to come out but very little or almoſt no ſenſible air at each exſuction into, and out of the cylinder; yet afterwards, at each drawing down the ſucker, the air included in the phial did manifeſtly dilate itſelf ſo long, that it did no leſs than nine times turn its mouth upwards, and diſcharge a bubble by conjecture about the bigneſs of a pea, after the manner newly recited. But as for that phial, which had the weight in it, it roſe not at all. So that being not able by quick pumping to gain another bubble from the air in the ſwimming glaſs, which proceeded from ſome ſmall leak in the veſſel, though it held in this experiment more ſtanch than was uſual, we thought fit to let in leiſurely the air from without, upon whoſe admiſſion that within the phial ſhrinking into a very narrow compaſs, the glaſs did, as we expected, fall down to the bottom of the jar.

But being deſirous, before we proceed to any new experiment, to try once more whether the little glaſs, that had the weight in it, might not alſo be raiſed; after we had ſuffered the engine to remain cloſed as it was, for five or ſix hours, the pump was again plied with ſo much obſtinacy, that not only about the upper part of the jar there appeared a good number of bubbles (but very much ſmaller than thoſe we ſaw the firſt time) but afterwards, there came from the bottom of the jar bubbles about the bigneſs of ſmall peas; which, the pump being ſtill kept going, followed one another to the number of forty, coming from the ſtopt phial; whoſe mouth, it ſeems, had not been ſhut ſo ſtrongly and cloſely, but that the included air, dilating itſelf by its own ſpring, made itſelf ſome little paſſage betwixt the wax and the glaſs, and got away in theſe bubbles: after which, the unſtopt glaſs began to float again, the air ſhut up in it being manifeſtly ſo dilated as to expel a good part of the water, but not ſo much as to break quite thorough. And at length, when our expectation of it was almoſt tired out, the heavier of the two phials began to come aloft, and immediately to ſubſide again; which appeared to be occaſioned by the air within it, whoſe bulk and ſpring being weakened by the receſs of the forty bubbles before mentioned, it was no longer able, as formerly, to break forcibly through the incumbent water; but forming a bubble at the mouth of the glaſs, buoyed it up towards the top, and there getting away, left it to ſink again, till the preſſure of the air in the receiver being farther taken off, the air in the phial was permitted to expand itſelf farther, and to create another bubble. by which it was again for a while carried up. And it was remarkable, that though after having emptied the receiver as far as well we could, we ceaſed from pumping, yet the veſſel continuing more ſtanch than it was wont, this aſſent and fall of the phial was repeated to the ninth time; the included air, by reaſon of the ſmallneſs of the vent at which it muſt paſs out, being not able to get away otherwiſe than by little and little; and conſequently, in divers ſuch parcels as were able to conſtitute bubbles, each of them big enough to raiſe the phial, and keep it aloft until the avolation of that bubble. Whereby it may appear, that the grand rule in Hydroſtaticks, that a body will ſwim in the water, in caſe it be lighter than as much of the water as equals it in bulk, will hold likewiſe, when the preſſure of the atmoſphere is in very great meaſure, if not when it is totally taken off from the liquor and the body: though it were worth inquiring, what it is, that ſo plentifully concurs to fill the bubbles made in our experiment by the ſo much expanded air. For to ſay with the old Peripatetic ſchools, that the air in rarefaction

tion may acquire a new extent, without the admiſſion of any new ſubſtance, would be an account of the phænomenon very much out of date, and which, I ſuppoſe, our modern naturaliſts would neither give nor acquieſce in.

I KNOW not whether it may be requiſite to add, that in this experiment, as in the former, the outward air, being let in, did ſoon precipitate the floating phial. But I think it will not be amiſs to note, that (congruouſly to what hath been above recorded of the vaſt expanſion of the air) the water, which in the heavier phial ſucceeded in the room of thoſe forty odd, if not fifty great bubbles of air, which at ſeveral times got out of it, amounted but to a very inconſiderable bigneſs.

EXPERIMENT XXVI.

IT having been obſerved by thoſe that have conſidered what belongs to pendulums (a ſpeculation that may, in my poor judgment, be highly uſeful to the Naturaliſts) that their vibrations are more ſlowly made, and that their motion laſts leſs in a thicker, than in a thinner medium; we thought it not amiſs to try, if a pendulum would ſwing faſter, or continue ſwinging longer in our receiver, in caſe of exſuction of the air, than otherwiſe. Wherefore we took a couple of round and poliſhed pendulums of iron or ſteel, of equal bigneſs, as near as we could get the artificer to make them; and weighing each of them twenty drachms, wanting as many grains: one of theſe we ſuſpended in the cavity of the receiver by a very ſlender ſilken ſtring, of about ſeven inches and a half in length, from the cover of the receiver to which it is faſtened. Then (by inclining the engine) we made the pendulum ſwing to and fro in it, and deſcribe as long arches, as in the capacity of ſo brittle a veſſel we thought ſafe and convenient. And one of the aſſiſtants telling the recurſions of the other pendulum hanging in the free air, by a ſtring of about the ſame length, we ſhortened and lengthened this other pendulum till it appeared to keep the ſame pace in its vibrations with that ſhut up in the receiver. Then having carefully drawn away the air, we did again ſet the pendulum in the receiver a vibrating; and giving the other ſuch a motion as made it deſcribe an arch, according to one's gueſs, equal to that of the included pendulum, we reckoned, one of us, the recurſions of that pendulum which was ſwinging within the receiver; and another of us that which was moving in (that which one would think a much more reſiſting medium) the air. But once, one of us reckoned near two and twenty recurſions of the included pendulum, whilſt the other reckoned but twenty of the pendulum that vibrated without. And another time alſo, the former of theſe pendula was reckoned to have made one and twenty recurſions, wherein the other made but twenty: yet this experiment ſeemed to teach us little, ſave that the difference betwixt the motion of ſuch a pendulum in the common air, and in one exceedingly rarefied, is ſcarce ſenſible in veſſels no bigger than our receiver; eſpecially, ſince though during this experiment it held very well, yet we could not ſuppoſe it to be altogether devoid of air. We obſerved alſo, that when the receiver was full of air, the included pendulum continued its recurſions about fifteen minutes (or a quarter of an hour) before it left off ſwinging; and that after the exſuction of the air, the vibration of the ſame pendulum (being freſh put into motion) appeared not (by a minute watch) to laſt ſenſibly longer. So that the event of this experiment being other than we expected, ſcarce afforded us any other ſatisfaction, than that of our not having omitted to try it. And whether, in caſe the trial be made with a pendulum much leſs diſproportionate to the air than ſteel is, the event will much better anſwer expectation, experience may be conſulted.

EXPERIMENT XXVII.

THAT the air is the medium, whereby ſounds are conveyed to the ear, hath been for many ages, and is yet the common doctrine of the ſchools. But this received opinion hath been of late oppoſed by ſome philoſophers upon the account of an experiment made by the induſtrious *Kircher*, and other learned men; who have (as they aſſure us) obſerved, that if a bell, with a ſteel clapper, be ſo faſtened to the to the inſide of a tube, that upon the making the experiment *de vacuo* with that tube, the bell remained ſuſpended in the deſerted ſpace at the upper end of the tube: and if alſo a vigorous load-ſtone be applied on the outſide of the tube to the bell, it will attract the clapper, which, upon the removal of the load-ſtone falling back, will ſtrike againſt the oppoſite ſide of the bell, and thereby produce a very audible ſound; whence divers have concluded, that it is not the air, but ſome more ſubtle body, that is the medium of ſounds. But becauſe we conceived, that, to invalidate ſuch a conſequence from this ingenious experiment, (though the moſt luciferous that could well be made without ſome ſuch engine as ours) ſome things might be ſpeciouſly enough alledged; we thought fit to make a trial or two, in order to the diſcovery of what the air doth in conveying of ſounds, reſerving divers other experiments triable in our engine concerning ſounds, till we can obtain more leiſure to proſecute them. Conceiving it then the beſt way to make our trial with ſuch a noiſe, as might not be loud enough to make it difficult to diſcern ſlighter variations in it, but rather might be, both laſting (that we might take notice by what degrees it decreaſed) and ſo ſmall, that it could not grow much weaker without becoming imperceptible; we took a watch, whoſe caſe we opened, that the contained air might have free egreſs into that of the receiver. And this watch was ſuſpended in the cavity of the veſſel only by a pack-thread, as the unlikelieſt thing to convey a ſound to the top of the receiver; and then cloſing up the veſſel with melted plaiſter, we liſtened near the ſides of it, and plainly enough heard the noiſe made by the balance. Thoſe alſo of us, that watched for that circumſtance, obſerved, that the noiſe ſeemed to come directly in a ſtreight line from the watch unto the ear. And it was obſervable to this purpoſe, that we found a manifeſt diſparity of noiſe, by holding our ears near the ſides of the receiver, and near the cover of it: which difference ſeemed to proceed from that of the texture of the glaſs, from the ſtructure of the cover (and the cement) through which the ſound was propagated from the watch to the ear. But let us proſecute our experiment. The pump after this being employed, it ſeemed, that from time to time the ſound grew fainter and fainter; ſo that when the receiver was emptied as much as it uſed to be for the foregoing experiments, neither we, nor ſome ſtrangers, that chanced to be then in the room, could, by applying our ears to the very ſides, hear any noiſe from within; though we could eaſily perceive, that by the moving of the hand, which marked the ſecond minutes, and by that of the balance, that the watch neither ſtood ſtill, nor remarkably varied from its wonted motion. And to ſatisfy ourſelves farther, that it was indeed the abſence of the air about the watch, that hindered us from hearing it, we let in the external air at the ſtop-cock; and then though we turned the key and ſtopt the valve, yet we could plainly hear the noiſe made by the balance, though we held our ears ſometimes at two foot diſtance

ſtance from the outſide of the receiver; and this experiment being reiterated into another place, ſucceeded after the like manner. Which ſeems to prove, that whether or no the air be the only, it is at leaſt the principal medium of ſounds. And by the way it is very well worth noting, that in a veſſel ſo well cloſed as our receiver, ſo weak a pulſe as that the balance of a watch, ſhould propagate a motion to the air in a phyſically ſtreight line, notwithſtanding the interpoſition of ſo cloſe a body as glaſs, eſpecially glaſs of ſuch thickneſs as that of our receiver; ſince by this it ſeems the air impriſoned in the glaſs muſt, by the motion of the balance, be made to beat againſt the concave part of the receiver, ſtrongly enough to make its convex part beat upon the contiguous air, and ſo propagate the motion to the liſtner's ears. I know this cannot but ſeem ſtrange to thoſe, who, with an eminent modern philoſopher, will not allow, that a ſound, made in the cavity of a room, or other place ſo cloſed, that there is no intercourſe betwixt the external and internal air, can be heard by thoſe without, unleſs the ſounding body do immediately ſtrike againſt ſome part of the incloſing body. But not having now time to handle controverſies, we ſhall only annex, that after the foregoing experiment, we took a bell of about two inches in diameter at the bottom, which was ſupported in the midſt of the cavity of the receiver by a bent ſtick, which by reaſon of its ſpring preſſed with its two ends againſt the oppoſite parts of the inſide of the veſſel: in which, when it was cloſed up, we obſerved, that the bell ſeemed to ſound more dead than it did when juſt before it ſounded in the open air. And yet, when afterwards we had (as formerly) emptied the receiver, we could not diſcern any conſiderable change (for ſome ſaid they obſerved a ſmall one) in the loudneſs of the ſound. Whereby it ſeemed, that though the air be the principal medium of ſound, yet either a more ſubtle matter may be alſo a medium of it, or elſe an ambient body, that contains but very few particles of air, in compariſon of thoſe it is eaſily capable of, is ſufficient for that purpoſe. And this, among other things, invited us to conſider, whether in the above-mentioned experiment made with the bell and the load-ſtone, there might not in the deſerted part of the tube remain air enough to produce a ſound; ſince the tubes for the experiment *de vacuo* (not to mention the uſual thinneſs of the glaſs) being ſeldom made greater than is requiſite, a little air might bear a not inconſiderable proportion to the deſerted ſpace: and that alſo, in the experiment *de vacuo*, as it is wont to be made, there is generally ſome little air, that gets in from without, or at leaſt ſtore of bubbles, that ariſe from the body of the quickſilver, or other liquor itſelf, obſervations heedfully made have frequently informed us; and it may alſo appear, by what hath been formerly delivered concerning the Torricellian experiment.

ON the occaſion of this experiment concerning ſounds, we may add in this place, that when we tried the experiment formerly mentioned, of firing gun-powder with a piſtol in our evacuated receiver, the noiſe made by the ſtriking of the flint againſt the ſteel was exceeding languid, in compariſon of what it would have been in the open air. And on divers other occaſions it appeared, that the ſounds created within our exhauſted glaſs, if they were not loſt before they reached the ear, ſeemed at leaſt to arrive there very much weakened. We intended to try, whether or no the wire-ſtring of an inſtrument ſhut up into our receiver would, when the ambient air was ſucked out, at all tremble, if in another inſtrument held cloſe to it, but without the receiver, a ſtring tuned (as muſicians ſpeak, how properly I now examine not) to an uniſon with it, were briſkly touched, and ſet a vibrating. This, I ſay, we purpoſed to try, to ſee how the motion made in the air without would be propagated through the cavity of our evacuated receiver. But when the inſtrument, wherewith the trial was to be made, came to be employed, it proved too big to go into the pneumatical veſſel: and we have not now the conveniency to have a fitter made.

We thought likewiſe to convey into the receiver a long and ſlender pair of bellows, made after the faſhion of thoſe uſually employed to blow organs, and furniſhed with a ſmall muſical inſtead of an ordinary pipe. For we hoped, that by means of a ſtring faſtened to the upper part of the bellows, and to the moveable ſtopple, that makes a part of the cover of our receiver, we ſhould, by frequently turning round that ſtopple, and the annexed ſtring, after the manner already often recited, be able to lift up and diſtend the bellows; and by the help of a competent weight faſtened to the ſame upper part of the bellows, we ſhould likewiſe be able at pleaſure to compreſs them, and, by conſequence, try whether, that ſubtler matter than air (which, according to thoſe that deny a vacuum, muſt be ſuppoſed to fill the exhauſted receiver) would be able to produce a ſound in the muſical pipe; or in a pipe like that of ordinary bellows, to beget a wind capable to turn or ſet on moving ſome very light matter, either ſhaped like the ſails of a wind-mill, or of ſome other convenient form, and expoſed to its orifice. This experiment, I ſay, we thought to make; but have not yet actually made it for want of an artificer to make us ſuch a pair of bellows as it requires.

We had thoughts alſo of trying, whether or no, as ſounds made by the bodies in our receiver become much more languid than ordinary, by reaſon of the want of air; ſo they would grow ſtronger, in caſe there were an unuſual quantity of air crouded and ſhut up in the ſame veſſel. Which may be done (though not without ſome difficulty) by the help of the pump, provided the cover and ſtopple be ſo firmly faſtened (by binding and cement, or otherwiſe) to the glaſs and to each other, that there be no danger of the condenſed air's blowing of either of them away, or its breaking through the junctures. Theſe thoughts, my Lord, as I was ſaying, we entertained; but for want of leiſure, as, of as good receivers as ours, to ſubſtitute in its place, in caſe we ſhould break it before we learned the ſkill of condenſing the air in it, we durſt not put them in practice. Yet on this occaſion give me leave to advertiſe your Lordſhip once for all, that though for the reaſons newly intimated, we have only in the ſeventeenth experiment, taken notice, that by the help of our engine the air may be condenſed as well as rarefied; yet there are divers other of our experiments, whoſe phænomena it were worth while to try to vary, by means of the compreſſion of the air.

EXPERIMENT XXVIII.

We taught, among divers other things, when we diſcourſed of our firſt experiment, that the air ſhut up in our receiver preſſeth as ſtrongly upon the bodies ſhut up with it, as if they were expoſed to the preſſure of the whole atmoſphere. That this was not inconſiderately propounded, we hope your Lordſhip hath gathered from divers of the things already recited: but yet perhaps it will not be amiſs to ſubjoin, by way of farther confirmation of the ſame truth, the following experiment; which would have accompanied the 20th, but the paper, wherein the one was written, chanced not to be at hand, when the other was ſent away.

We conveyed into the receiver a new glaſs phial, capable of holding about 6 or 7 ounces of water, into which we had before put 2 or 3 ſpoonfuls of that liquor, and ſtopt it cloſe with a fit cork. The pneumatical veſſel being emptied, there appeared not any change in the incloſed water, the air impriſoned with it, not having the force to blow out the ſtopple. Which event, though it were no other than we expected, was differing from what we deſired. For we would gladly have ſeen, what change would have appeared in the water upon the bottle's being ſuddenly unſtopped, in a place where the ambient body was ſo differing from our common air. Wherefore we did again put in the phial, but leſs ſtrongly cloſed than formerly, though as ſtrongly ſtopt as ſeemed requiſite on ordinary occaſions: but when the air was pumped out of the receiver, that within the phial did quickly, as we expected, find or make itſelf little paſſages to get out at: as we argued, from this, that whereas when the phial was put in the time before, the water remained all the while perfectly free from bubbles; at this time the bottom of the glaſs appeared all covered with them, and they, upon the egreſs of the excluded air into the receiver, did preſently flag and ſhrink up.

FROM theſe trials it ſeemed deducible enough, that whilſt the phial continued to be well ſtopped, the included water did, from the air ſhut up with it, ſuſtain a preſſure equal to that of the atmoſphere; ſince till the air could get out of the glaſs, there appeared no bubbles in the water, notwithſtanding the want of preſſure in the ambient body.

BUT to be ſure to reach the chief end of our experiment, we made uſe of this other expedient. We cauſed a convenient quantity of water to be put, and hermetically ſhut up into a glaſs egg, whoſe long neck (which was purpoſely made of an unequal thickneſs) was faſtened to one end of a ſtring, whoſe other end was tied to the cover of our receiver, after the manner elſewhere mentioned already. Then the egg being conveyed into the pneumatical veſſel, and that being evacuated, we did, by turning the braſs-ſtopple formerly deſcribed amongſt the parts of our engine, ſo ſhorten the ſtring as to break the glaſs; whereby liberty being given to the air impriſoned in the egg to paſs into the cavity of the receiver, the ſudden receſs of the air made the bubbles in a trice appear ſo numerous, and aſcend ſo ſwiftly in the water, that their motion looked like that of a violent ſhower of rain; ſave that the bubbles did not, like the drops of rain, tend downwards, but upwards. Which made me reſemble this phænomenon to what I have ſeen happen in the diſſolution of ſeed-pearl in ſome acid menſtruum; in which, if a good quantity of the little pearls be caſt whole, they will at firſt, if the menſtruum be ſharp enough, be carried in ſwarms from the bottom to the top of the liquor. We will add, that without ſealing up the glaſs, this experiment may be tried in one of our ſmalleſt receivers. For there the exſuction of the ambient air may be performed ſo nimbly, that immediately the bubbles lurking in the water are allowed to diſplay themſelves, and aſcend in throngs; inſomuch, as having in ſuch a receiver tried the experiment with wine (as a more ſpirituous liquor) inſtead of water, the red-wine appeared all covered with a copious, but vaniſhing white froth, almoſt as if a veſſel full of bottled drink had been unwarily opened.

EXPERIMENT XXIX.

IT may not a little conduce to the clearer explication of divers points in the doctrine of meteors, and perhaps of ſome other phyſiological difficulties, to diſcover what the air doth to the motion of thoſe ſtreams or exhalations that aſcend into it; namely, whether they mount upwards by virtue of any ſuch poſitive levity (as ſome Peripateticks ſpeak) acquired together with their aërial nature, as enables them to pierce through part of the atmoſphere, and overcome its reſiſtance. Or elſe, whether theſe ſteams being once raiſed above the earth by their agitation, have their aſcent and ſuſtentation aloft, rather promoted than hindered by the air: as the inferior parts of that, being thicker and heavier than the ſuperior, the ſteams can more eaſily continue for a while their agitation upwards than downwards; and afterwards are by the ſame fluidity and thickneſs of the air carried to and fro in it, and kept from relapſing to the earth: as in the ſea-water the ſaline parts are kept from ſubſiding by thoſe aqueous ones, wherewith they are aſſociated.

We hoped to illuſtrate this matter, by obſerving the motion of the ſmoke, proceeding from kindled or flaming bodies in our exhauſted receiver. But as we formerly noted, upon the exſuction of the air, the ſmoking of thoſe bodies preſently ceaſed. We had thoughts alſo of conveying into our pneumatical glaſs a hot iron, with ſome body eaſy to be diſſipated into ſmoke ſet upon it; but conſidered, that neither was that way free from inconveniencies; eſpecially this, that the hot body would make the impriſoned air circulate within the receiver, and conſequently make it queſtionable, whether the aſcent of the ſteams would not be due to the new and acquired motion of the air.

Wherefore I bethought myſelf of another way to ſatisfy in ſome meaſure my curioſity, to wit, by means of a certain liquor, which I called to mind, that ſome years ago I had (for a deſign that belongs not to our preſent purpoſe) prepared; which, I ſuppoſe, I ſhewed your Lordſhip, and which had the luck to be taken notice of by divers very ingenious and famous men. For this liquor, though moſt of its ingredients be metals, and all of them ponderous enough, is yet of that nature, that whilſt the phial wherein it is kept is ſtopped (how ſlight a cover ſoever) both the liquor and the glaſs are tranſparent; and ſo is that upper half of the glaſs, to which the liquor reacheth not: but as ſoon as ever the ſtopple is taken out, and full acceſs is given to the external air, both in the inward part of the cork, and the liquor itſelf, do preſently ſend upwards, and ſcatter abroad a fume as thick and white, as if there were a quantity of alabaſter-duſt thrown up into the air. And this ſmoking of the liquor laſts till my unwillingneſs to waſte it invites me to ſtop it again; and then the aſcenſion of the fumes ſuddenly ceaſeth, till the phial be again unſtopped.

This fuming liquor then, I thought, would much conduce to the diſcovery I deſired to make, ſince it ſaved me the need of conveying any hot body with it into the receiver, and would not darken it with fumes before the time. Wherefore having tied to the phial a great weight of lead, to keep it from being lifted up by the drawing out of the cork: and having tied to the ſtopple one end of a ſtring, of which the other end was made faſt to the cover of the pneumatical glaſs, the liquor was carefully cloſed up after the wonted manner: then the air being diligently pumped out, the phial was unſtopped in the emptied receiver. And though immediately, upon the drawing out of the cork, there appeared to be as it were thrown up ſome white fumes, which ſeemed to proceed from the air before impriſoned in the phial, and diffuſing itſelf ſuddenly into the capacity of the receiver; yet we afterward obſerved, as we expected, that the fumes did not mount and diſperſe themſelves as they uſed to do in the open air; but that, when by reaſon of the agitation of the corpuſcles of the liquor, which could not continue their motion in ſo narrow a ſpace as the phial afforded them, and were therefore reduced to thruſt one another out of it; when, I ſay, by theſe aſſiſtances the fumes were aſcended to the lip of the phial, they mounted no higher, but ran down along the outſide of the phial to the bottom of it; and thence along a long and inclining piece of lead, on which the phial reſted, like a little ſtream (not very much bigger than a ſwan's quill) whoſe nature they ſeemed to emulate ſo well, that it quitted not the phial till it was come to the bottom of it, and then forſook it in ſuch a manner, as a ſtream of water of the ſame bigneſs would have done. And this ſtream laſted a pretty while, and would probably have laſted longer, but that being loth to waſte my liquor, I let in at the ſtop-cock a pretty deal of the external air; notwithſtanding which, finding after a while that the ſtream did run afreſh, though as it ſeemed not altogether ſo copious as before, I let as much more air as would come in; and found (ſomewhat to my wonder) that though the ſtream formerly mentioned diſappeared, yet there appeared not any white fumes to ariſe, either from the cork, or out of the phial itſelf, no not when the cover was removed from the receiver: though not only after a while there aſcended white fumes from the receiver, but having forthwith taken out the phial into the open air, it emitted white exhalations as before. And having preſently after unſtopped it in an open window, we found both it and the cork immediately to ſend forth a yet much more plentiful ſmoke. Though it be now divers years ſince this numerical liquor was prepared, after the manner mentioned either by *Carneades* or *Eleutherius* (for I do not well remember which) in thoſe *Dialogues concerning heat and flame* that have been above mentioned.

MORE circumſtances concerning theſe fumes we might have obſerved, had we not been deterred by an indiſpoſition in point of health, from having much to do with ſteams of ſo dangerous a nature, as by that of the ingredients of this liquor theſe ſeem likely to be of.

THE reflections, that may be made upon this experiment, we have not now the leiſure to proſecute; and therefore ſhall content ourſelves to recommend the ſeveral circumſtances of it to your Lordſhip's ſerious conſideration; and to take notice *(en paſſant)* that ſteams in an ambient body, or a medium thinner than themſelves, may both tend downwards, and otherwiſe emulate the nature of a liquor: which I therefore point at, that it may appear the leſs ſtrange, if we ſometimes ſpeak of the atmoſphere as of a kind of liquor, in compariſon of that more thin and ſubtile celeſtial matter, that ſurrounds it.

AND though it might perchance ſuffice to have on this occaſion intimated thus much; yet, leſt this way of ſpeaking of the atmoſphere ſhould be thought too bold and extravagant, I am content to borrow an experiment of the diſcourſe formerly mentioned (touching *fluidity and firmneſs*,) and ſubjoin it here with alterations ſuitable to the contrivance of our engine; and this the rather, becauſe I hope it may conduce to the diſcovery of the nature of the atmoſphere: for which reaſon it might have been annexed to what hath been noted either upon the firſt, or eighteenth experiment, but that when they were written and ſent away, it came not into my mind. The experiment then, as we tried in our engine, was as follows:

EXPERIMENT XXX.

WE took one of the ſmall receivers, often mentioned already, and into it we conveyed a piece of well-lighted match; and letting it remain there, till it had filled the receiver with ſmoke, we took it out and haſtily cloſed again the receiver, that the ſmoke might not get away. Then ſtaying a while to let theſe fumes leiſurely ſubſide, we found, as we expected, that after ſome time they ſettled themſelves in the lower half of the receiver, in a darkiſh body, leaving the upper half of pierce through part of the atmoſphere, and overcome its reſiſtance. Or elſe, whether theſe ſteams being once raiſed above the earth by their agitation, have their aſcent and ſuſtentation aloft, rather promoted than hindered by the air: as the inferior parts of that, being thicker and heavier than the ſuperior, the ſteams can more eaſily continue for a while their agitation upwards than downwards; and afterwards are by the ſame fluidity and thickneſs of the air carried to and fro in it, and kept from relapſing to the earth: as in the ſea-water the ſaline parts are kept from ſubſiding by thoſe aqueous ones, wherewith they are aſſociated.

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WHEREFORE I bethought myſelf of another way to ſatisfy in ſome meaſure my curioſity, to wit, by means of a certain liquor, which I called to mind, that ſome years ago I had (for a deſign that belongs not to our preſent purpoſe) prepared; which, I ſuppoſe, I ſhewed your Lordſhip, and which had the luck to be taken notice of by divers very ingenious and famous men. For this liquor, though moſt of its ingredients be metals, and all of them ponderous enough, is yet of that nature, that whilſt the phial wherein it is kept is ſtopped (how ſlight a cover ſoever) both the liquor and the glaſs are tranſparent; and ſo is that upper half of the glaſs, to which the liquor reacheth not: but as ſoon as ever the ſtopple is taken out, and full acceſs is given to the external air, both in the inward part of the cork, and the liquor itſelf, do preſently ſend upwards, and ſcatter abroad a fume as thick and white, as if there were a quantity of alabaſter-duſt thrown up into the air. And this ſmoking of the liquor laſts till my unwillingneſs to waſte it invites me to ſtop it again; and then the aſcenſion of the fumes ſuddenly ceaſeth, till the phial be again unſtopped.

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THE reflections, that may be made upon this experiment, we have not now the leiſure to proſecute; and therefore ſhall content ourſelves to recommend the ſeveral circumſtances of it to your Lordſhip's ſerious conſideration; and to take notice *(en paſſant)* that ſteams in an ambient body, or a medium thinner than themſelves, may both tend downwards, and otherwiſe emulate the nature of a liquor: which I therefore point at, that it may appear the leſs ſtrange, if we ſometimes ſpeak of the atmoſphere as of a kind of liquor, in compariſon of that more thin and ſubtile celeſtial matter, that ſurrounds it.

AND though it might perchance ſuffice to have on this occaſion intimated thus much; yet, leſt this way of ſpeaking of the atmoſphere ſhould be thought too bold and extravagant, I am content to borrow an experiment of the diſcourſe formerly mentioned (touching *fluidity and firmneſs*,) and ſubjoin it here with alterations ſuitable to the contrivance of our engine; and this the rather, becauſe I hope it may conduce to the diſcovery of the nature of the atmoſphere: for which reaſon it might have been annexed to what hath been noted either upon the firſt, or eighteenth experiment, but that when they were written and ſent away, it came not into my mind. The experiment then, as we tried in our engine, was as follows:

EXPERIMENT XXX.

WE took one of the ſmall receivers, often mentioned already, and into it we conveyed a piece of well-lighted match; and letting it remain there, till it had filled the receiver with ſmoke, we took it out and haſtily cloſed again the receiver, that the ſmoke might not get away. Then ſtaying a while to let theſe fumes leiſurely ſubſide, we found, as we expected, that after ſome time they ſettled themſelves in the lower half of the receiver, in a darkiſh body, leaving the upper half of the receiver tranſparent, and, as to ſight, full of nought but clear air. Now to manifeſt, that this ſmoke thus ſettled emulated a liquor, we inclined the engine that contained it ſometimes to one ſide, and ſometimes to the other; and obſerved the ſmoke to keep its ſurface almoſt horizontal, notwithſtanding the ſtooping of the veſſel that held it, as water, or another liquor, would in the like caſe have done. And if by a quicker rocking of the engine the ſmoke were more ſwiftly ſhaken, it would, like water, either vibrate to and fro from one ſide to the other of the glaſs, or elſe have its ſurface manifeſtly curled with waves, but preſerve itſelf in an entire and diſtinct body from the incumbent air; and being permitted to reſt a while, would ſoon recover its former ſmooth and level ſuperficies. If alſo the key were turned, and the valve unſtopped, ſo that there was a free, though but a narrow paſſage opened betwixt the internal air and the cavity of the receiver, then would ſome of this ſmoke fall down, as it were, in a ſtream into the ſubjacent cylinder, and a proportionate quantity of the outward air would manifeſtly aſcend through it into the incumbent air, much after the ſame manner as if you invert a phial with a long neck, and well filled with red-wine, into a glaſs full of fair water, you ſhall ſee the water and wine by degrees mingle with one another; the one falling down as it were in little coloured ſtreams, and the other aſcending in its room in the like curled ſtreams, ſometimes preceded by round parcels of water, which, by reaſon of their tranſparency, look almoſt like bubbles. The other circumſtances of this experiment, belonging not all of them to our preſent purpoſe, we ſhall content ourſelves with taking notice of one, which ſeems the moſt important, and may illuſtrate and confirm ſome things formerly delivered. And it was, that if, when the ſuperficies of our ſmoke lay ſmooth and horizontal, a hot iron were held near the outſide of the receiver, the neighbouring part of the included fumes (for the reſt did not very much alter their former ſuperficies) being rarefied by the heat, would readily aſcend in a large pillar of ſmoke to the very top of the receiver, yet without ſeeming to loſe a diſtinct ſuperficies, or to be confounded with air; below which, upon the receſs of the adventitious heat, that by agitating it impelled it upward, it would again ſubſide.

ALL which being added to the late experiment of the ſmoking liquor, and to what may be from that, which hath been elſewhere ſaid, gathered to the ſame purpoſe, will, I hope, keep it at leaſt from appearing abſurd; if (ſince we ſee, that there is ſo great an inequality in the denſity and weight of liquors, that water is near fourteen times thinner or lighter than quickſilver of the ſame bulk, and well dephlegmed ſpirit of wine, yet much lighter than water) we venture to ſpeak ſometimes of the atmoſphere, as if it were a peculiar kind of thin and halitious liquor (if I may ſo call it) much lighter than ſpirit of wine.

To theſe things I know not whether it will be requiſite to add, that as we lately took notice of conſpicuous waves, that appeared upon the ſuperficies of our agitated ſmoke, ſo ſome ſuch thing may not abſurdly be conjectured to happen on the ſuperficies of the atmoſphere, by thoſe ſtrange ruggedneſſes, that appear (eſpecially in the ſpring and fall, when exhalations and vapours are wont to aſcend moſt plentifully) upon the limb or edge of the riſing and ſetting ſun. I ſpeak thus diffidently upon this occaſion, becauſe I know, that by the fluctuation or boiling of the ſun's own ſuperficies, divers eminent mathematicians have plauſibly enough (but how truly, I leave your Lordſhip to judge) endeavoured to give an account of it. But if we will join with thoſe, that have aſcribed of late this phænomenon to the refraction the ſunbeams ſuffer in our vapid air, we may, as hath been intimated, promote their doctrine,

trine, by deducing from it, that probably the ſurface of the atmoſphere is oftentimes (if not always) exceedingly curled or waved. And certainly it is ſomewhat wonderful, as well as very pleaſant to behold, how, to him that looks upon the ſetting ſun through a long and excellent teleſcope, there will not only appear ſtrange inequalities in the edge of it (inſomuch that I have often ſeen it more indented than a ſaw); but thoſe inequalities will vaniſh in one place, and preſently appear in another, and ſeem perfectly to move like waves ſucceeding and deſtroying one another, ſave that their motion oftentimes ſeems to be quickeſt, as if in that vaſt ſea they were carried on by a current, or at leaſt by a tide. And this (as we elſewhere note) appears to the eye, not only when it looks directly through the teleſcope upon the ſun, but alſo when a large and well-defined image of the ſun is by the ſame teleſcope brought into a room and caſt upon a ſheet of white paper. But to inſiſt on this were to digreſs; and therefore I will proceed to experiments of another kind.

EXPERIMENT XXXI.

IT hath been admired by very ingenious men, that if the exquiſitely poliſhed ſurfaces of two flat pieces of marble be ſo congruous to each other, that from their mutual application there will reſult an immediate contact, they will ſtick ſo faſt together, that he, that lifts up the uppermoſt, ſhall, if the undermoſt be not exceeding heavy, lift up that too, and ſuſtain it aloft in the free air. A probable cauſe of this ſo cloſe adheſion we have elſewhere endeavoured to deduce from the unequal preſſure of the air upon the undermoſt ſtone; for the lower ſuperficies of that ſtone being freely expoſed to the air, is preſſed upon by it, whereas the uppermoſt ſurface, being contiguous to the ſuperior ſtone, is thereby defended from the preſſure of the air; which conſequently preſſing the lower ſtone againſt the upper, hinders it from falling, as we have elſewhere more fully declared. Upon theſe grounds we conjectured, that in caſe we could procure two marbles exactly ground to one another, and in caſe we could alſo ſufficiently evacuate our receiver; the lower ſtone would, for want of the wonted and ſuſtaining preſſure of the air, fall from the upper. But the farther trial of this experiment we muſt, unleſs your Lordſhip think it worth your making at *Paris*, put off till a fitter opportunity. For where we now are, we cannot procure marbles ſo exactly ground, that they will ſuſtain one another in the air above a minute or two, which is a much ſhorter time than the emptying of our receiver requires. We did indeed try to make our marbles ſtick cloſe together, by moiſtening their poliſhed ſurfaces with rectified ſpirit of wine, in regard that liquor, by its ſudden avolation from marble, if poured thereon, without leaving it moiſt or leſs ſmooth, ſeemed unable to ſuſtain them together after the manner of a glutinous body, and yet ſeemed ſufficient to exclude and keep out the air. But this we tried to little purpoſe, for having conveyed into the receiver two black ſquare marbles (the one of two inches and a third in length or breadth, and ſomewhat more than half an inch in thickneſs; the other of the ſame extent, but not much above half ſo thick) faſtened together by the intervention of pure ſpirit of wine; and having ſuſpended the thicker by a ſtring from the cover, we found not, that the exſuction of the ambient air would ſeparate them, though a weight amounting to four ounces were faſtened to the lowermoſt marble to facilitate its falling off.

I WOULD gladly have the experiment tried with marble ſo well poliſhed, as to need no liquor whatſoever to make them cohere, and in a veſſel, out of which the air may be more perfectly drawn than it was out of ours. But in the mean time, though we will not determine whether the ſpirit of wine did contribute to the ſtrong coheſion of theſe ſtones, otherwiſe than by keeping even the ſubtleſt parts of the air from getting in between them; yet it ſeemed, that the not falling down of the lowermoſt marble might, without improbability, be aſcribed to the preſſure of the air remaining in the receiver: which, as we formerly noted, having been able to keep a cylinder of water, of above a foot in height, from falling to the bottom of the tube, may well enough be ſuppoſed capable of keeping ſo broad a flat marble from deſcending. And though this may ſeem a ſtrange proof of the ſtrength of the ſpring of the air, even when rarefied, yet it will ſcarce ſeem incredible to him, that hath obſerved, how exceeding ſtrong a coheſion may be made betwixt broad bodies, only by their immediate touching one another. A notable inſtance of which I met with in this ſhort narrative of the learned *Zucchius*: *Juveni* (ſaith he) *lacertorum ſuorum robur jactanti propoſita ſemel eſt lamina ærea, per anſam in medio extantem apprehenſam elevanda è tabula marmorea, cui optime congruebat: qui primo tanquam rem ludicram puero committendam contempſit; tum inſtantibus amicis manum utramque admovens, cum luctatus diu hærentem non removiſſet, excuſavit impotentiam, objecta peregrini & potentiſſimi glutinis interpoſitione, quo fortiſſime copulante nequiret divelli; donec vidit ab alio per tabulam facillimè laminam deduci, & ad extrema productam, & actam in tranſverſum inde deportari.* But that we may learn from our own engine, that two bodies, though they touch each other but in a ſmall part of their ſurfaces, may be made to cohere very ſtrongly, only by this, that the air preſſes much more forcibly upon the inferior ſuperficies of the lowermoſt body, than upon the upper ſurface of the ſame; we will hereunto annex the following experiment, though out of the order, wherein they were made.

P. Nic. Zucchius apud Schot. part 1. Mec. Hydraulo-pneum.

EXPERIMENT XXXII.

I Remember I have, in a diſcourſe touching fluidity and firmneſs, made mention of my having, by the exſuction of the air out of a glaſs veſſel, made that veſſel take up, or ſuck up (to ſpeak in the common language) a body weighing divers ounces; but our engine affording us the opportunity of making conſiderabler experiments of that kind, we thought fit to make a further trial of the force of the atmoſphere's preſſure upwards, after the following manner:

THE receiver having been exquiſitely cloſed, as we have often taught already, and the air being in a good meaſure drawn out of it, it was removed from off the pump: and to the lower branch of the ſtop-cock, there was ſpeedily applied a tapering valve of braſs, ſuch as is deſcribed in the 9th figure, made fit to go with its narrower end into the cavity of the branch, and to fill the orifice of that cavity with its broader part. And that the air might not get in at the little intervals, left here and there between the convex ſurface of the ſtopple and the internal edge of the branch, thoſe intervals were ſtopt with a little diachylon. And to the door, or (if you pleaſe) that part of the valve, which was to move to and fro, and in this experiment hung perpendicular to the horizon, there was, at a button of braſs belonging to the valve, faſtened a broad ſcale, wherein weights were to be put. This done, the key of the ſtop-cock was turned, and the external air beating like a forcible ſtream upon the valve to get in there, it did ſuddenly both ſhut the valve, and keep it ſhut ſo ſtrongly, that we had time to caſt in divers weights one after another into the ſcale; till at length the weight overpowering the preſſure of the atmoſphere, drew down the valve by the ſtrings that tied the ſcale to it, and gave liberty to the outward air to ruſh into the receiver. Though another time, when the valve had but little weight hanging at it, being, by I know not what accident, drawn down beneath its former place, it was by the impetuous current of the outward air ſuddenly impelled up into it again, and kept there. But in the former experiment it is remarkable, that though the receiver were not well exhauſted, and though it leaked, whilſt the reſt of the experiment was in proſecution; and though the valve, whereon the cylinder of the atmoſphere could preſs, were not above an inch and a half in diameter, yet the weight kept up by ſuction, or rather ſupported by the air, (namely the valve, the ſcale, and what was caſt out of it) being ſent to be weighed, amounted to about ten of our common pounds, conſiſting of ſixteen ounces a piece; ſo that we doubted not, but that, had the experiment been made with favourable circumſtances, the air endeavouring to preſs in at the orifice of the ſtop-cock, would have kept a very much greater weight from falling out of it; I ſay the air, becauſe we found, by trial purpoſely made, that neither the imperfect contact of the valve and the ſtop-cock, nor the diachylon, that was employed to fill up the little crannies left betwixt them, were conſiderable in this experiment. By which it may, among other things, appear, that I did not without cauſe, in the above-named diſcourſe touching fluidity and firmneſs, aſcribe a great force, even to ſuch pillars of air, as may be ſuppoſed to begin at the top of the atmoſphere, and recoiling from the ground, to terminate on the bodies on which they preſs; ſince in the preſent experiment ſuch a weight was ſupported by ſo ſlender a cylinder of air, rebounding from the earth to the valve, whereon it did bear.

EXPERIMENT XXXIII.

BUT in regard we have not yet been able to empty ſo great a veſſel as our receiver, ſo well as we can the cylinder itſelf; our pump alone may afford us a nobler inſtance of the force of the air we live in, inſomuch, that by help of this part of our engine, we may give a pretty near gueſs at the ſtrength of the atmoſphere, computed as a weight. And the way may be this; firſt, the ſucker being brought to move eaſily up and down the cylinder, is to be impelled to the top of it: then the receiver muſt be taken off from the pump, that the upper orifice of the cylinder remaining open, the air may freely ſucceed the ſucker, and therefore readily yield to its motion downwards. This done, there muſt be faſtened to one of the iron teeth of the ſucker ſuch a weight as may juſt ſuffice to draw it to the bottom of the cylinder. And having thus examined, what weight is neceſſary to draw down the ſucker, when the atmoſphere makes no other than the ordinary reſiſtance of the air againſt its deſcent; the ſucker muſt be again forced to the top of the cylinder, whoſe upper orifice muſt now be exactly cloſed; and then (the firſt weight remaining) we eaſily may, by hanging a ſcale to the above mentioned iron (that makes part of the ſucker) caſt in known weights ſo long, till in ſpite of the reluctancy of the atmoſphere the ſucker may be drawn down. For to theſe weights in the ſcale that of the ſcale being added, the ſum will give us the weight of a column of air, equal in diameter to the ſucker, or to the cavity of the cylinder, and in length to the height of the atmoſphere.

ACCORDING to this method we did, ſince the writing of the laſt experiment, attempt to meaſure the preſſure of the atmoſphere, but found it more difficult than we expected to perform it with any accurateneſs; for though by the help of the manubrium the ſucker moved up and down with ſo much eaſe, that one would have thought, that both its convex ſurface, and the concave one of the cylinder, were exquiſitely ſmooth, and as it were ſlippery; yet when the ſucker came to be moved only with a dead weight or preſſure (that was not, like the force of him that pumped, intended as occaſion required) we found, that the little roughneſſes or other inequalities, and perhaps too the unequal preſſure of the leather againſt the cavity of the cylinder, were able, now and then, to put a ſtop to the deſcent or aſcent of the ſucker, though a very little external help would eaſily ſurmount that impediment; and then the ſucker would, for a while, continue its formerly interrupted motion, though that aſſiſtance were withdrawn. But this diſcouragement did not deter us from proſecuting our experiment, and endeavouring, by a careful trial, to make it as inſtructive as we could. We found then, that a leaden weight, of 28 pounds (each conſiſting of ſixteen ounces) being faſtened to one of the the teeth of the ſucker, drew it down cloſely enough, when the upper orifice of the cylinder was left open, though by the help of oil and water, and by the frequent moving the ſucker up and down with the manubrium, its motion in the cylinder had been before purpoſely facilitated. This done, the upper orifice of the cylinder was very carefully and cloſely ſtopt, the valve being likewiſe ſhut with its wonted ſtopple well oiled, after the ſucker had been again impelled up to the top of the cylinder. Then to the precedent twenty-eight pound we added a hundred and twelve pounds more; which forcing down the ſucker, though but leiſurely, we took off the twenty-eight pound weight; and being unable to procure juſt ſuch weights as we would have had, we hung on, inſtead of it, one of fourteen pound; but found that, with the reſt, unable to carry down the ſucker. And to ſatisfy ourſelves, and the ſpectators, that it was the reſiſtance of the ambient air, that hindered the deſcent of ſo great a weight, after that we had tried, that upon unſtopping the valve, and thereby opening an acceſs to the external air, the ſucker would be immediately drawn down; after this, I ſay, we made this farther experiment, that having by a man's ſtrength forcibly depreſſed the ſucker to the bottom of the cylinder, and then faſtened weights to the above-named iron, that makes part of that ſucker, the preſſure of the external air finding little or nothing in the cavity of the evacuated cylinder to reſiſt it, did preſently begin to impel the ſucker, with the weights that clogged it, towards the upper part of the cylinder; till ſome ſuch accidental impediment, as we formerly mentioned, checked its courſe. And when that rub (which eaſily might be) was taken out of the way, it would continue its aſcent to the top, to the no ſmall wonder of thoſe byſtanders, that could not comprehend, how ſuch a weight could aſcend, as it were, of itſelf; that is, without any inviſible force, or ſo much as ſuction to lift it up. And indeed it is very conſiderable, that though poſſibly there might remain ſome particles of air in the cylinder, after the drawing down of the ſucker; yet the preſſure of a cylinder of the atmoſphere, ſomewhat leſs than three inches in diameter (for, as it was ſaid in the deſcription of our engine, the cavity of the cylinder was no broader) was able, uncompreſſed, not only to ſuſtain, but even to drive up a weight of a hundred and odd pounds: for beſides the weight of the whole ſucker itſelf, which amounts to ſome pounds, the weights annexed to it made up an hundred and three pounds, beſides an iron bar, that by conjecture weighed two pounds more; and yet all theſe together fall ſomewhat ſhort of the weight, which we lately mentioned the reſiſtance of the air to have held ſuſpended in the cavity of the cylinder.

AND though (as hath been already acknowledged) we cannot peradventure obtain by the recited means ſo exact an account as were to be wiſhed, of what we would diſcover;

cover; yet, if it ſerve us to ground conjectures more approaching to the truth, than we have hitherto met with, I hope it will be conſidered (which a famous poet judiciouſly ſays,)

Eſt quoddam prodire tenus, ſi non datur ultra.

PERADVENTURE it will not be impertinent to annex to the other circumſtances, that have been already ſet down concerning this experiment, that it was made in winter, in weather neither froſty nor rainy, about the change of the moon, and at a place whoſe latitude is near about 51 degrees and a half; for perhaps the force or preſſure of the air may vary, according to the ſeaſons of the year, the temperature of the weather, the elevation of the pole, or the phaſes of the moon; all, or even any of them ſeeming capable to alter either the height or conſiſtence of the incumbent atmoſphere: and therefore it would not be amiſs, if this experiment were carefully tried at ſeveral times and places, with variety of circumſtances. It might alſo be tried with cylinders of ſeveral diameters, exquiſitely fitted with ſuckers, that we might know, what proportion ſeveral pillars of the atmoſphere bear to the weights they are able to ſuſtain or lift up; and conſequently, whether the increaſe or decrement of the reſiſtance of the ambient air can be reduced to any regular proportion to the diameters of the ſuckers. Theſe, and divers other ſuch things, which may be tried with this cylinder, might moſt of them be more exactly tried by the Torricellian experiments, if we could get tubes ſo accurately blown and drawn, that the cavity were perfectly cylindrical.

To dwell upon all the ſeveral reflexions, that a ſpeculative wit might make upon this and the foregoing experiment (I mean the thirty-third and thirty-ſecond) would require almoſt a volume; whereas our occaſions will ſcarce allow us time to touch upon three or four of the chief inferences, that ſeem deducible from them, and therefore we ſhall content ourſelves to point at thoſe few.

AND firſt, as many other phænomena of our engine, ſo eſpecially the two lately mentioned experiments ſeem very much to call in queſtion the received opinion of the nature or cauſe of ſuction. For it is true indeed, that when men ſuck, they commonly uſe ſome manifeſt endeavour, by a peculiar motion of their mouths, cheſts, and ſome other conſpiring parts, to convey to them the body to be ſucked in. And hence perhaps they have taken occaſion to think, that in all ſuction there muſt be ſome endeavour or motion in the ſucking to attract the ſucked body. But in our laſt experiment it appears not at all, how the upper part of the emptied cylinder, that remains moveleſs all the while, or any part of it, doth at all endeavour to draw to it the depreſſed ſucker and the annexed weights. And yet thoſe, that behold the aſcenſion of the ſucker, without ſeriouſly conſidering the cauſe of it, do readily conclude it to be raiſed by ſomething, that powerfully ſucks or attracts it, though they ſee not what that may be, or where it lurks. So that it ſeems not abſolutely neceſſary to ſuction, that there be in the body, which is ſaid to ſuck, an endeavour or motion in order thereunto; but rather that ſuction may be at leaſt for the moſt part reduced to pulſion, and its effects aſcribed to ſuch a preſſure of the neighbouring air upon thoſe bodies (whether aërial, or of other natures) that are contiguous to the body, that is ſaid to attract them, as is ſtronger, than that ſubſtance, which poſſeſſeth the cavity of that ſucking body, is able to reſiſt. To object here, that it was ſome particles of air remaining in the emptied cylinder, that attracted this weight to obviate a vacuum, will ſcarce be ſatisfactory; unleſs it can be clearly made out, by what little hooks, or other grappling inſtruments, the internal air could take hold of the ſucker; how ſo little of it obtained the force to lift up ſo great a weight; and why alſo, upon the letting in of a little more air into one of our evacuated veſſels, the attraction is, inſtead of being ſtrengthened, much weakened; though, if there were danger of a vacuum before, it would remain, notwithſtanding this ingreſs of a little air. For that ſtill there remained in the capacity of the exhauſted cylinder ſtore of little rooms, or ſpaces empty or devoid of air, may appear by the great violence, wherewith the air ruſheth in, if any way be opened to it. And that it is not ſo much the decrement of the vacuum within the cavity of the veſſel, that debilitates the attraction, as the ſpring of the included air (whoſe preſence makes the decrement) that doth it by reſiſting the preſſure of the external air, ſeems probable, partly from the diſability of vacuities, whether greater or leſſer, to reſiſt the preſſure of the air; and partly by ſome of the phænomena of our experiments, and particularly by this circumſtance of the three and thirtieth, that the ſucker was, by the preſſure of the ambient air, impelled upwards with its weight hanging at it, not only when it was in the bottom of the cylinder, and conſequently left a great vacuum in the cavity of it; but when the ſucker had been already impelled almoſt to the top of the cylinder, and conſequently, when the vacuum that remained was become very little in compariſon of that, which preceded the beginning of the ſucker's aſcenſion.

In the next place, theſe experiments may teach us, what to judge of the vulgar axiom received for ſo many ages as an undoubted truth in the Peripatetic ſchools, that nature abhors and flieth a vacuum, and that to ſuch a degree, that no human power (to go no higher) is able to make one in the univerſe; wherein heaven and earth would change places, and all its other bodies rather act contrary to their own nature, than ſuffer it. For, if by a vacuum we will underſtand a place perfectly devoid of all corporeal ſubſtance, it may indeed then, as we formerly noted, be plauſibly enough maintained, that there is no ſuch thing in the world. But that the generality of the Pleniſts (eſpecially till of late years ſome of them grew more wary) did not take a vacuum in ſo ſtrict a ſenſe, may appear by the experiments formerly, and even to this day employed by the denyers of a vacuum, to prove it impoſſible, that there can be any made. For when they alledge (for inſtance) that when a man ſucks water through a long pipe, that heavy liquor, contrary to its nature, aſcends into the ſucker's mouth, only to fill up that room made by the dilatation of his breaſt and lungs, which otherwiſe will in part be empty: and when they tell us, that the reaſon, why if a long pipe exactly cloſed at one end be filled top-full of water, and then inverted, no liquor will fall out of the open orifice; or, to uſe a more familiar example, when they teach, that the cauſe, why in a gardener's watering pot ſhaped conically or like a ſugar-loaf, filled with water, no liquor falls down through the numerous holes at the bottom, whilſt the gardener keeps his thumb upon the orifice of the hole at the top, and no longer; muſt be, that if in the caſe propoſed the water ſhould deſcend, the air being unable to ſucceed it, there would be left at the upper and deſerted part of the veſſel a vacuum, that would be avoided, if the hole at the top were opened: When (I ſay) they alledge ſuch experiments, the tendency of them ſeems plainly to import, that they mean, by a vacuum, any ſpace here below, that is not filled with a viſible body, or at leaſt with air, though it be not quite devoid of all body whatſoever. For why ſhould nature, out of her deteſtation of a vacuum, make bodies act contrary to their own tendency, that a place may be filled with air, if its being ſo were not neceſſary to the avoiding of a vacuum?

TAKING then a vacuum in this vulgar and obvious ſenſe, the common opinion about it ſeems liable to ſeveral exceptions, whereof ſome of the chief are ſuggeſted to us by our engine.

IT will not eaſily then be intelligibly made out, how hatred or averſation, which is a paſſion of the ſoul, can either for a vacuum, or any other object, be ſuppoſed to be in water, or ſuch like inanimate body, which cannot be preſumed to know, when a vacuum would enſue, if they did not beſtir themſelves to prevent it; nor to be ſo generous as to act contrary to what is moſt conducive to their own particular preſervation for the public good of the univerſe. As much then of intelligible and probable truth, as is contained in this metaphorical expreſſion, ſeems to amount but to this; that by the wiſe author of nature (who is juſtly ſaid to have made all things in number, weight and meaſure) the univerſe, and the parts of it, are ſo contrived, that it is as hard to make a vacuum in it, as if they ſtudiouſly conſpired to prevent it. And how far this itſelf may be granted, deſerves to be farther conſidered.

FOR, in the next place, our experiments ſeem to teach, that the ſuppoſed averſation of nature to a vacuum is but accidental, or in conſequence, partly of the weight and fluidity, or, at leaſt, fluxility of the bodies here below; and partly, and perhaps principally, of the air, whoſe reſtleſs endeavour to expand itſelf every way makes it either ruſh in itſelf, or compel the interpoſed bodies into ſmall ſpaces, where it finds no greater reſiſtance than it can ſurmount. And that in thoſe motions which are made *ob fugam vacui* (as the common phraſe is) bodies act without ſuch generoſity and conſideration, as is wont to be aſcribed to them, is apparent enough in our 32d experiment, where the torrent of air, that ſeemed to ſtrive to get into the emptied receiver, did plainly prevent its own deſign, by ſo impelling the valve, as to make it ſhut the only orifice the air was to get out at. And if afterwards, either nature, or the internal air had a deſign the external air ſhould be attracted, they ſeemed to proſecute it very unwiſely by continuing to ſuck the valve ſo ſtrongly, when they found, that by that ſuction the valve itſelf could not be drawn in; whereas by forbearing to ſuck, the valve would by its own weight have fallen down, and ſuffered the excluded air to return freely, and to fill again the exhauſted veſſel.

AND this minds me to take notice of another deficiency, pointed at by our experiments in the common doctrine of thoſe Pleniſts we reaſon with; for many of thoſe unuſual motions in bodies, that are ſaid to be made to eſcape a vacuum, ſeem rather made to fill it. For why, to inſtance in our newly mentioned experiment, as ſoon as the valve was depreſſed by the weight we hung at it, ſhould the air ſo impetuouſly and copiouſly ruſh into the cavity of the receiver, if there were before no vacant room there to receive it? And if there were, then all the while the valve kept out the air, thoſe little ſpaces in the receiver, which the corpuſcles of that air afterwards filled, may be concluded to have remained empty. So that the ſeeming violence, employed by nature on the occaſion of the evacuating of the veſſel, ſeems to have come too late to hinder the making of vacuities in the receiver, and only to have, as ſoon as we permitted, filled up with air thoſe that were already made.

AND as for the care of the public good of the univerſe aſcribed to dead and ſtupid bodies, we ſhall only demand, why in our 19th experiment, upon the exſuction of the ambient air, the water deſerted the upper half of the glaſs tube; and did not aſcend to fill it up, till the external air was let in upon it. Whereas by its eaſy and ſudden regaining that upper part of the tube, it appeared both that there was there much ſpace devoid of air, and that the water might with ſmall or no reſiſtance have aſcended into it, if it could have done ſo without the impulſion of the re-admitted air; which, it ſeems, was neceſſary to mind the water of its formerly neglected duty to the univerſe.

NAY, for aught appeareth, even when the excluded air, as ſoon as it was permitted, ruſhed violently into our exhauſted receiver, that flowing in of the air proceeded rather from the determinate force of the ſpring of the neighbouring air, than from any endeavour to fill up, much leſs to prevent vacuities. For though, when as much air as will, is gotten into our receiver, our preſent opponents take it for granted, that it is full of air; yet if it be remembered, that when we made our 17th experiment, we crouded in more air to our receiver than it uſually holds; and if we alſo conſider (which is much more) that the air of the ſame conſiſtence with that in our receiver may in wind-guns, as is known, and as we have tried, be compreſſed at leaſt into half its wonted room (I ſay, at leaſt, becauſe ſome affirm, that the air may be thruſt into an 8th, or a yet ſmaller part of its ordinary extent) it ſeems neceſſary to admit either a notion of condenſation and rarefaction, that is not intelligible, or that in the capacity of our receiver, when preſumed to be full of air, there yet remained as much of ſpace as was taken up by all the aërial corpuſcles, unpoſſeſſed by the air. Which ſeems plainly to infer, that the air, that ruſhed into our emptied veſſel, did not do it preciſely to fill up the vacuities of it, ſince it left ſo many unfilled, but rather was thruſt in by the preſſure of the contiguous air: which as it could not but be always ready to expand itſelf, where it found leaſt reſiſtance, ſo was it unable to fill the receiver any more, than until the air within was reduced to the ſame meaſure of compactneſs with that without.

WE may alſo from our two already often-mentioned experiments farther deduce, that (ſince nature's hatred of a vacuum is but metaphorical and accidental, being but a conſequence or reſult of the preſſure of the air and of the gravity, and partly alſo of the fluxility of ſome other bodies) the power ſhe makes uſe of to hinder a vacuum, is not (as we have elſewhere alſo noted) any ſuch boundleſs thing, as men have been pleaſed to imagine. And the reaſons, why in the former experiments, mentioned in favour of the Pleniſts, bodies ſeem to forget their own natures to ſhun a vacuum, ſeems to be but this; that in the alledged caſes the weight of that water, that was either kept from falling or impelled up, was not great enough to ſurmount the preſſure of the contiguous air; which, if it had been, the water would have ſubſided, though no air could have ſucceeded. For not to repeat that experiment of Monſieur *Paſchal* (formerly mentioned to have been tried in a glaſs exceeding 32 feet) wherein the inverted pipe being long enough to contain a competent weight of water, that liquor freely ran out at the lower orifice: not to mention this (I ſay) we ſaw in our nineteenth experiment, that when the preſſure of the ambient air was ſufficiently weakened, the water would fall out apace at the orifice even of a ſhort pipe, though the air could not ſucceed into the room deſerted by it. And it were not amiſs, if trial were made on the tops of very high mountains, to diſcover with what eaſe a vacuum could be made near the confines of the atmoſphere, where the air is probably but light in compariſon of what it is here below. But our preſent (three and thirtieth) experiment ſeems to manifeſt, not only that the power, exerciſed by nature, to ſhun or repleniſh a vacuum is limited, but that it may be determined even to pounds and ounces: inſomuch that we might ſay, ſuch a weight nature will ſuſtain or will lift up to reſiſt a vacuum in our engine; but if an ounce more be added to that weight, it will ſurmount her ſo much magnified deteſtation of vacuities. And thus, my Lord, our experiments may not only anſwer thoſe of the Pleniſts, but enable us to retort their arguments againſt themſelves; ſince, if that be true, which they alledge, that when water falls not down according to its nature, in a body, wherein no air can ſucceed to fill up the place it muſt leave, the ſuſpenſion of the liquor is made, *ne detur vacuum* (as they ſpeak) it will follow, that if the water can be brought to ſubſide in ſuch a caſe, that deſerted ſpace may be deemed empty, according to their own doctrine; eſpecially, ſince nature (as they would perſuade us) beſtirs herſelf ſo mightily to keep it from being deſerted.

I hope I ſhall not need to remind your Lordſhip, that I have all this while been ſpeaking of a vacuum, not in the ſtrict and philoſophical ſenſe, but in that more obvious and familiar one, that hath been formerly declared.

And therefore I ſhall now proceed to obſerve in the laſt place, that our 33d experiment affords us a notable proof of the unheeded ſtrength of that preſſure, which is ſuſtained by the corpuſcles of what we call the free air, and preſume to be uncompreſſed. For, as fluid and yielding a body as it is, our experiment teacheth us, that even in our climate, and without any other compreſſion than what is (at leaſt here below) natural, or (to ſpeak more properly) ordinary to it, it bears ſo ſtrongly upon the bodies, whereunto it is contiguous, that a cylinder of this free air, not exceeding three inches in diameter, is able to raiſe and carry up a weight, amounting to between ſixteen and ſeventeen hundred ounces. I ſaid, even in our climate, becauſe that is temperate enough; and as far as my obſervations aſſiſt me to conjecture, the air in many other more northern countries may be much thicker, and able to ſupport a greater weight: which is not to be doubted of, if there be no miſtake in what is recorded concerning the * Hollanders, that were forced by the ice to winter in *Nova Zembla*, namely, that they found there ſo condenſed an air, that they could not make their clock go; even by a very great addition to the weights, that were wont to move it.

I suppose your Lordſhip will readily take notice, that I might very eaſily have diſcourſed much more fully and accurately than I have done, againſt the common opinion touching ſuction, and touching nature's hatred of a vacuum. But I was willing to keep myſelf to thoſe conſiderations touching theſe matters, that might be verified by our engine itſelf; eſpecially ſince, as I ſaid at firſt, it would take up too much time to inſiſt particularly upon all the reflections, that may be made even upon our two laſt experiments. And therefore paſſing to the next, I ſhall leave it to your Lordſhip to conſider, how far theſe trials of ours will either confirm or diſfavour the new doctrine of ſeveral eminent Naturaliſts, who teach, that in all motion there is neceſſarily a circle of bodies, as they ſpeak, moving together; and whether the circles in ſuch motion be an accidental or conſequential thing or no.

EXPERIMENT XXXIV.

It is a known thing to thoſe, that are converſant in the hydroſtatics, that two bodies, which in the air are of equal weight, but of unequal bulk, as gold for inſtance, and iron, being afterwards weighed in water, will loſe their æquilibrium upon the change of the ambient body: ſo that the gold will ſink lower than the iron; which, by reaſon of its greater bulk, hath more water to lift or diſplace, that it may ſink. By analogy to this experiment, it ſeemed probable, that if two weights did in our engine balance each other, when the glaſs was full of air; upon the exſuction of a great part of that air, ſo notable a change in the conſiſtence of the ambient body would make them loſe their æquilibrium.

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* *Aëre frigido exiſtente tardius moventur Automata quam aëre calido, adeò quidem ut Automaton, quod Belgæ in Nova Zembla agentes in ædibus ſuis collocaverunt, omnino à motu ceſſaverit etſi multo majus pondus ei addidiſſent quam antea ferre ſolebat.* Varenius Geog. General. Lib. III. Propo. 7. p. 648.

BUT being deſirous at the ſame time to make a trial, for a certain deſign, that needs not here be mentioned, we took for one of our weights a dry bladder, ſtrongly tied at the neck, and about half filled with air (that being a weight both ſlight, and that would expand itſelf in the evacuated glaſs) and faſtening that to one part of our formerly mentioned exact balance (which turns with the 32d part of a grain) we put a metalline counterpoiſe into the oppoſite ſcale; and ſo the two weights being brought to an æquilibrium, the balance was conveyed into the receiver, and ſuſpended from the cover of it.

BUT before we proceed farther, we muſt note, that preſently after the laying on of the cover, the bladder appeared to preponderate; whereupon the ſcales being taken out, and reduced very near to an æquilibrium, yet ſo, that a little advantage remained on that ſide, to which the metalline weight belonged; they were again let down into the receiver, which was preſently made faſt with plaiſter, and a hot iron: ſoon after which, before the pump was employed, the bladder ſeemed again a little to preponderate. Afterwards the air in the glaſs being begun to be drawn out, the bladder began (according to the formerly mentioned obſervations) to expand itſelf and manifeſtly to outweigh the oppoſite weight, drawing down the ſcale, to which it was faſtened, very much beneath the other, eſpecially when the air had ſwelled to its full extent.

THIS done, we very leiſurely let in the external air; and obſerved, that upon the flagging of the bladder, the ſcale, whereto it was faſtened, not only by degrees returned to an æquilibrium with the other, but at length was a little outweighed by it.

BUT becauſe we ſuſpected there might have intervened ſome unheeded circumſtance in this laſt part of the experiment, we would not preſently take out the ſcales, nor meddle with the cover; but leaving things as they were, we perceived, that after a little while, the bladder began again to preponderate, and by degrees to ſink lower and lower for divers hours; wherefore, leaving the veſſel cloſed up all night, we repaired to it next morning, and found the bladder fallen yet lower; as if the very ſubſtance of it had imbibed ſome of the moiſture wherewith the air (the ſeaſon being very rainy) did then abound; as lute-ſtrings, which are made likewiſe of the membraneous parts of guts, ſtrongly wreathed, are known to ſwell ſo much oftentimes, as to break in rainy and wet weather. Which conjecture is the more to be regarded, becauſe congruouſly unto it one of the company having a little warmed the bladder, found it then lighter than the oppoſite weight. But this muſt be looked upon as a bare conjecture, till we can gain time to make farther trials about it. In the mean while, we ſhall add, that without removing the ſcales or the cover of the receiver, we again cauſed the air to be drawn out, (the weather continuing very moiſt) but found not any manifeſt alteration in the balance; whether becauſe the æquilibrium was too far loſt to let a ſmall change appear, we determine not.

BUT to make the experiment with a body leſs apt to be altered by the temperature of the air, than was the bladder; we brought the ſcales again to an æquilibrium with two weights, whereof the one was of lead, the other of cork. And having evacuated the receiver, we obſerved, that both upon the exſuction, and after the return of the air, the cork did manifeſtly preponderate, and much more, a while after the air had been let in again, than whilſt it was kept out. Wherefore, in the room of the cork, we ſubſtituted a piece of charcoal, as leſs likely to imbibe any moiſture from the air, but the event proved much the ſame with that newly related: ſo that this experiment ſeems more liable to caſualties than any, excepting one we have made in our engine. And as it is difficult to prevent them, ſo it ſeems not very eaſy to diſcover the cauſes of them, whereof we ſhall therefore at preſent forbear mentioning our conjectures.

EXPERIMENT XXXV.

SOME learned Mathematicians have of late ingeniouſly endeavoured to reduce filtres to ſiphons; but ſtill the true cauſe of the aſcenſion of water, and other liquors both in ſiphons and in filtration, needing (for aught we have yet found) a clearer diſcovery and explication, we were deſirous to try, whether or no the preſſure of the air might reaſonably be ſuppoſed to have either the principal, or at leaſt a conſiderable intereſt in the raiſing of thoſe liquors. But becauſe we found, that we could not yet ſo evacuate our receiver, but that the remaining air, though but little in compariſon of the exhauſted, would be able to impel the water to a greater height than is uſual in ordinary filtrations; we reſolved, inſtead of a liſt of cotton, or the like filtre, to make uſe of a ſiphon of glaſs, delineated in the third figure; conſiſting of three pieces, two ſtraight, and the third crooked to join them together; whoſe junctures were diligently cloſed, that no air might find entrance at them. One of the legs of this ſiphon was (as it ſhould be) ſomewhat longer than the other, and was pervious at the bottom of it only, by a hole almoſt as ſlender as a hair, that the water might but very leiſurely drop out of it, leſt it ſhould all run out before the experiment were compleated. The other and ſhorter leg of the ſiphon was quite open at the end, and the ſame wideneſs with the reſt of the pipe, whoſe bore was about a quarter of an inch. The whole ſiphon made up of theſe ſeveral pieces put together was deſigned to be about a foot and a half long; that the remaining air, when the veſſel was exhauſted after the wonted manner, might not be able to impel the water to the top of the ſiphon; which being inverted, was filled with water, and of which the ſhorter leg being let down two or three inches deep into a glaſs veſſel full of water, and the upper parts of it being faſtened to the inſide of the cover of the receiver, we proceeded to cloſe firſt, and then to empty the veſſel.

THE effect of the trial was this, that till a pretty quantity of air had been drawn out, the water dropped freely out at the lower end of the lower leg of the ſiphon, as if the experiment had been performed in the free air. But afterwards, the bubbles (as had been apprehended) began to diſcloſe themſelves in the water, and aſcending to the top of the ſiphon, imbodied themſelves there into one, which was augmented by little and little, by the riſing of other bubbles, that from time to time broke into it, but much more by its own dilatation, which increaſed proportionably to the exſuction, that was made of the air out of the receiver. So that at length the water in the ſhorter leg of the ſiphon was reduced, partly by the extraction of the ambient air, and partly by the expanſion of the great bubble at the upper part of the ſiphon, to be about a foot high, if ſo much; whereby it came to paſs, that the courſe of the water in the ſiphon was interrupted, and that, which remained in the longer leg of it, continued ſuſpended there without dropping any longer. But upon the turning of the ſtop-cock, the outward air (being let into the receiver) got into the ſiphon, by the little hole, at which the water formerly dropped out; and traverſing all the incumbent cylinder of water, in the form of bubbles, joined itſelf with that air, that before poſſeſſed the top of the ſiphon.

To prevent the inconveniences ariſing from theſe bubbles, two glaſs pipes, like the former, were ſo placed, as to terminate together in the midſt of the belly of a glaſs phial, into whoſe neck they were carefully faſtened with cement; and then both the phial and the pipes being (which was not done without difficulty) totally filled with water, the ſiphon deſcribed in the fifth figure was placed with its ſhorter leg in the glaſs of water, as formerly: and the experiment being proſecuted after the ſame manner, much more air than formerly was drawn out, before the bubbles, diſcloſing themſelves in the water, were able to diſturb the experiment; becauſe that in the capacity of the phial there was room enough for them to ſtretch themſelves, without depreſſing the water below the ends of the pipes: and during this time, the water continued to drop out of the propending leg of the ſiphon. But at length the receiver being very much emptied, the paſſage of the water through the ſiphon ceaſed, the upper ends of the pipes beginning to appear a little above the remaining water in the phial, whoſe dilated air appeared likewiſe to preſs down the water in the pipes, and fill the upper part of them.

AND hereby the continuity of the water, and ſo the experiment itſelf being interrupted, we were invited to let in the air again, which, according to its various proportions of preſſure to that of the air in the phial and the pipes, did for a good while exhibit a pleaſing variety of phænomena, which we have not now the leiſure to recite. And though upon the whole matter there ſeemed little or no cauſe to doubt, but that, if the bubbles had not diſturbed the experiment, it would manifeſtly enough have appeared, that the courſe of water through ſiphons depends upon the preſſure of the air; yet we reſolved, at our next leiſure and conveniency, to try the experiment again, with a quantity of water before freed from bubbles by the help of the ſame engine.

THIS occaſion I have had to take notice of ſiphons, puts me in mind of an odd kind of ſiphon, that I cauſed to be made a pretty while ago; and which hath been ſince, by an ingenious man of your acquaintance, communicated to divers others. The occaſion was this: an eminent Mathematician told me one day, that ſome inquiſitive Frenchmen (whoſe names I know not) had obſerved, that in caſe one end of a ſlender and perforated pipe of glaſs be dipped in water, the liquor will aſcend to ſome height in the pipe, though held perpendicular to the plain of the water. And, to ſatisfy me, that he miſrelated not the experiment, he ſoon after brought two or three ſmall pipes of glaſs, which gave me the opportunity of trying it; though I had the leſs reaſon to diſtruſt it, becauſe I remember I had often, in the long and ſlender pipes of ſome weather-glaſſes, which I had cauſed to be made after a ſomewhat peculiar faſhion, taken notice of the like aſcenſion of the liquor, though (preſuming it might be caſual) I had made but little reflection upon it. But after this trial, beginning to ſuppoſe, that though the water in theſe pipes, that were brought me, riſe not above a quarter of an inch (if near ſo high) yet, if the pipes were made ſlender enough, the water might riſe to a very much greater height; I cauſed ſeveral of them to be, by a dexterous hand, drawn out at the flame of a lamp, in one of which, that was almoſt incredibly ſlender, we found, that the water aſcended (as it were of itſelf) five inches by meaſure, to the no ſmall wonder of ſome famous Mathematicians, who were ſpectators of ſome of theſe experiments. And this height the water reached to, though the pipe were held in as erected a poſture as we could; for if it were inclined, the water would fill a greater part of it, though not riſe higher in it. And we alſo found, that when the inſide of the pipe was wetted beforehand, the water would riſe much better than otherways. But we cauſed not all our ſlender pipes to be made ſtreight, but ſome of them crooked, like ſiphons: and having immerſed the ſhorter leg of one of theſe into a glaſs, that held ſome fair water, we found, as we expected, that the water ariſing to the top of the ſiphon, though that were high enough, did of itſelf run down the longer leg, and continue running like an ordinary ſiphon. The cauſe of this aſcenſion of the water appeared to all that were preſent ſo difficult, that I muſt not ſtay to enumerate the various conjectures that were made at it, much leſs to examine them; eſpecially having nothing but bare conjectures to ſubſtitute in the room of thoſe I do not approve. We tried indeed, by conveying a very ſlender pipe and a ſmall veſſel of water into our engine, whether or no the exſuction of the ambient air would aſſiſt us to find the cauſe of the aſcenſion we have been ſpeaking of: but though we employed red wine inſtead of water, yet we could ſcarce certainly perceive thorough ſo much glaſs, as was interpoſed betwixt our eyes and the liquor, what happened in a pipe ſo ſlender, that the redneſs of the wine was ſcarce ſenſible in it. But, as far as we could diſcern, there happened no great alteration to the liquor; which ſeemed the leſs ſtrange, becauſe the ſpring of that air, that might depreſs the water in the pipe, was equally debilitated with that, which remained to preſs upon the ſurface of the water in the little glaſs. Wherefore, in favour of his ingenious conjecture, who aſcribed the phænomenon under conſideration to the greater preſſure made upon the water by the air without the pipe, than by that within it, (where ſo much of the water, conſiſting perhaps of corpuſcles more pliant to the internal ſurfaces of the air, was contiguous to the glaſs) it was ſhown, that in caſe the little glaſs-veſſel, that held the water, of which a part aſcended into the ſlender pipe, were ſo cloſed, that a man might with his mouth ſuck the air out of it, the water would immediately ſubſide in the ſmall pipe. And this would indeed infer, that it aſcended before only by the preſſure of the incumbent air; but that it may (how juſtly I know not) be objected, that peradventure this would not happen, in caſe the upper end of the pipe were in a vacuum; and that it is very probable the water may ſubſide, not becauſe the preſſure of the internal air is taken off by the exſuction, but by reaſon of the ſpring of the external air, which impels the water, it finds in its way to the cavity deſerted by the other air, and would as well impel the ſame water upwards, as make it ſubſide, if it were not for the accidental poſture of the glaſſes. However, having not now leiſure to examine any farther this matter, I ſhall only mind your Lordſhip, that if you will proſecute this ſpeculation, it will be pertinent to find out likewiſe, why the ſurface of water (as is manifeſt in pipes) uſeth to be concave, being depreſſed in the middle, and higher on every ſide: and why in quickſilver, on the contrary, not only the ſurface is wont to be very convex, or ſwelling, in the middle; but if you dip the end of a ſlender pipe in it, the ſurface of the liquor (as it is called) will be lower within the pipe, than without. Which phænomena, whether, and how far, they may be deduced from the figure of the mercurial corpuſcles, and the ſhape of the ſpringy particles of the air, I willingly leave to be conſidered.

EXPERIMENT XXXVI.

SEVERAL ways we have met with propoſed, partly by the excellent *Galileo*, and partly by other ingenious writers, to manifeſt, that the air is not devoid of weight; ſome of theſe require the previous abſence of the air to be weighed; and others, the violent condenſation of it. But if we could lift a pair of ſcales above the atmoſphere, or place them in a vacuum, we might there weigh a parcel of air itſelf, as here we do other bodies in the air, becauſe it would there be heavier than that which ſurrounds it, as are groſſer bodies we commonly weigh, than the medium or ambient air. Wherefore, though we have above declined to affirm, that our receiver, when emptied, deſerves the name of a true vacuum; and though we cannot yet perfectly free it from air itſelf; yet we thought fit to try how far the air would manifeſt its gravity in ſo thin a medium, as we could make in our receiver, by evacuating it. We cauſed then to be blown at the flame of a lamp a glaſs bubble of about the bigneſs of a ſmall hen-heg, and of an oval form, ſave that at one end there was drawn out an exceeding ſlender pipe, that the bubble might be ſealed up, with as little rarefaction as might be of the air included in the great or oval cavity of it. This glaſs being ſealed, was faſtened to one of the ſcales of the exact pair of balances formerly mentioned; and being counterpoiſed with a weight of lead, was conveyed into the receiver, and cloſed up in it. The beam appearing to continue horizontal, the pump was ſet on work; and there ſcarce paſt above two or three exſuctions of the air, before the balance loſt its æquilibrium, and began to incline to that ſide on which the bubble was; which, as the air was farther and farther drawn out, did manifeſtly more and more preponderate, till he that pumped began to grow weary of his employment: after which the air being leiſurely let in again, the ſcales by degrees returned to their former æquilibrium. After that we took them out, and caſting into that ſcale to which the lead belonged three quarters of a grain, we conveyed the balance into the receiver, which being cloſed up, and exhauſted as before, we obſerved, that as the air was drawn out more and more, ſo the glaſs bubble came nearer and nearer to an æquilibrium with the other weight, till at length the beam was drawn to hang horizontal; which (as we had found by another trial) we could not bring it to do, when a quarter of a grain more was added to the ſcale, to which the lead belonged: though it ſeemed queſtionleſs, that if we could have perfectly emptied the receiver of the contained air, that included in the bubble would have weighed above a grain, notwithſtanding its having been probably ſomewhat rarefied by the flame, by the help of which the bubble was ſealed up. Let us add, that on the regreſs of the excluded air, the lead, and the weight caſt into the ſame ſcale, did again very much preponderate.

We likewiſe conveyed into the receiver the ſame bubble, opened at the end of the ſlender pipe above mentioned; but having drawn out the air, after the accuſtomed manner, we found not, as before, the bubble to outweigh the oppoſite lead: ſo that by the help of our engine we can weigh the air, as we weigh other bodies, in its natural or ordinary conſiſtence, without at all condenſing it: nay, which is remarkable, having conveyed a lamb's bladder about half full of air into the receiver, we obſerved, that though upon the drawing out of the ambient air, the impriſoned air ſo expanded itſelf, as to diſtend the bladder ſo, as to ſeem ready to break it; yet this rarefied air did manifeſtly depreſs the ſcale, whereunto it was annexed.

Another thing we muſt not forget to mention, that happened to us, whilſt we were making trials concerning the weight of the air; namely, that having once cauſed the pump to be ſomewhat obſtinately plied, to diſcover the better what may be expected from the thinneſs of the medium in this experiment; the impriſoned air broke its brittle priſon, and throwing the greateſt part of it againſt the ſide of the receiver, daſhed it againſt that thick glaſs into a multitude of pieces. Which accident I mention, partly, that it may confirm what we delivered in our reflexions upon the firſt experiment, where we conſidered what would probably be done by the ſpring of the air impriſoned in ſuch glaſſes, in caſe the balancing preſſure of the ambient air were withdrawn; and partly, that we may thence diſcern of how cloſe a texture glaſs is, ſince ſo very thin a film of glaſs (if I may ſo call it) proved ſo impervious to the air, that it could not get away through the pores, but was forced to break the glaſs in pieces to free itſelf; and this, notwithſtanding the time and advantage it had to try to get out at the pores. And this I mention, that neither our experiments, nor thoſe of divers learned men, might receive any prejudice from an experiment, which I happened to make divers years ago, and which, having been ſo much taken notice of by curious men, may be drawn to countenance their erroneous opinion, who would fain perſuade us, that glaſs is penetrable by air properly ſo called. Our experiment was briefly this: We were diſtilling a certain ſubſtance, that much abounded with ſubtile ſpirits and volatile ſalt, in a ſtrong earthen veſſel of an unuſual ſhape, to which was luted a large receiver, made of the coarſer ſort of glaſs, (which the tradeſmen are wont to call green glaſs;) but in our abſence, the fire, though it were to be very ſtrong, was, by the negligence or miſtake of thoſe we appointed to attend it, ſo exceſſively increaſed, that when we came back to the furnace, we found the ſpirituous and ſaline corpuſcles poured out (if I may ſo call it) ſo hot, and ſo copiouſly into the receiver, that they made it all opacous, and more likely to fly in pieces, than fit to be touched. Yet, being curious to obſerve the effects of a diſtillation, proſecuted with ſo intenſe and unuſual a degree of heat, we ventured to come near; and obſerved, among other things, that on the outſide of the receiver, at a great diſtance from the juncture, there was ſettled a round whitiſh ſpot or two, which at firſt we thought might be ſome ſtain upon the glaſs; but after, finding it to be in divers qualities like the oil and ſalt of the concrete we were diſtilling, we began to ſuſpect, that the moſt ſubtile and fugitive parts of the impetuouſly aſcending ſteams had penetrated the ſubſtance (as they ſpeak) of the glaſs, and by the cold of the ambient air were condenſed on the ſurface of it. And though we were very backward to credit this ſuſpicion, and therefore called in an ingenious perſon or two, both to aſſiſt us in the obſervation, and have witneſs of its event, we continued a while longer to watch the eſcape of ſuch unctuous fumes; and upon the whole matter unanimouſly concluded, that (all things conſidered) the ſubtile parts of the diſtilled matter being violently agitated by the exceſſive heat, had paſſed through the pores of the glaſs, widened by the ſame heat. But this having never happened but once in any of the diſtillations we have either made or ſeen, though theſe be not a few, it is much more reaſonable to ſuppoſe, that the perviouſneſs of our receiver to a body much more ſubtile than air proceeded partly from the looſer texture of that particular parcel of glaſs the receiver was made of (for experience hath taught us, that all glaſs is not of the ſame compactneſs and ſolidity) and partly from the enormous heat, which, together with the vehement agitation of the penetrant ſpirits, opened the pores of the glaſs; than to imagine that ſuch a ſubſtance as air ſhould be able to permeate the body of glaſs, contrary to the teſtimony of a thouſand chymical and mechanical experiments, and of many of thoſe made in our engine, eſpecially that newly recited: nay, by our fifth experiment it appears, that a thin bladder will not at its pores give paſſage even to rarefied air. And on this occaſion we will annex an experiment, which hath made ſome of thoſe we have acquainted with it doubt, whether the corpuſcles of the air be not leſs ſubtile than thoſe of water.

BUT without examining here the reaſonableneſs of that doubt, we will proceed to recite the experiment itſelf, which ſeems to teach, that though air, when ſufficiently compreſſed, may perchance get entrance into narrower holes and crannies than water; yet unleſs the air be forced in at ſuch very little holes, it will not get in at them, though they may be big enough to let water paſs through them.

THE experiment then was this: I took a fair glaſs ſiphon, the lower end of whoſe longeſt leg was drawn by degrees to ſuch a ſlenderneſs, that the orifice, at which the water was to fall out, would hardly admit a very ſmall pin. This ſiphon being inverted, the matter was ſo ordered, that a little bubble of air was intercepted in the ſlendereſt part of the ſiphon, betwixt the little hole newly mentioned and the incumbent water; upon which it came to paſs, that the air being not to be forced through ſo narrow a paſſage, by ſo light a cylinder of water (though amounting to the length of divers inches) as leaned upon it, hindered the farther efflux of the water, as long as I pleaſed to let it ſtay in that narrow place: whereas, when by blowing a little at the wider end of the ſiphon, that little parcel of air was forced out with ſome water, the remaining water, that before continued ſuſpended, began freely to drop down again as formerly. And if you take a glaſs pipe, whether it be in the form of a ſiphon, or no, that being for the moſt part of the thickneſs of a man's finger, is yet towards one end ſo ſlender, as to terminate in a hole almoſt as ſmall as a horſe-hair; and if you fill this pipe with water, you will find that liquor to drop down freely enough thorough the ſlender extreme: but if you then invert the pipe, you will find, that the air will not eaſily get in at the ſame hole, through which the water paſſed. For in the ſharp end of the pipe, ſome inches of water will remain ſuſpended, which, it is probable, would not happen, if the air could get in to ſucceed it; ſince if the hole were a little wider, the water would immediately ſubſide. And though it be true, that if the pipe be of the length of many inches, a great part of the water will run down at the wider orifice; yet that ſeems to happen for ſome other reaſon, than becauſe the air ſucceeds it at the upper and narrow orifice, ſince all the ſlender part of the pipe, and perhaps ſome inches more, will continue full of water.

AND on this occaſion I remember, that whereas it appears by our fifth experiment, that the aërial corpuſcles (except perhaps ſome, that are extraordinary fine) will not paſs thorough the pores of a lamb's bladder, yet particles of water will, as we have long ſince obſerved, and as may be eaſily tried, by very cloſely tying a little alcalizate ſalt (we uſed the calx of tartar made with nitre) in a fine bladder, and dipping the lower end of the bladder in water: for if you hold it there for a competent while, you will find, that there will ſtrain thorough the pores of the bladder water enough to diſſolve the ſalt into a liquor.

BUT I ſee I am ſlipped into a digreſſion; wherefore I will not examine, whether the experiment I have related proceeded from hence, that the ſpringy texture of the corpuſcles of the air makes them leſs apt to yield and accommodate themſelves eaſily to the narrow pores of bodies than the more flexible particles of water; or whether it may more probably be aſcribed to ſome other cauſe. Nor will I ſtay to conſider, how far we may hence be aſſiſted to gueſs at the cauſe of the aſcenſion of water in the ſlender pipes and ſiphons formerly mentioned; but will return to our bubble, and take notice, that we thought fit alſo to endeavour to meaſure the capacity of the bubble we had made uſe of, by filling it with water, that we might the better know how much water anſwered in weight to $\frac{3}{4}$ of a grain of air. But notwithſtanding all the diligence, that was uſed to preſerve ſo brittle a veſſel, it broke before we could perfect that we were about, and we were not then provided of another bubble fit for our turn.

THE haſte I was in, my Lord, when I ſent away the laſt ſheet, made me forget to take notice to you of a problem, that occurred to my thoughts, upon the occaſion of the ſlow breaking of the glaſs bubble in our evacuated receiver. For it may ſeem ſtrange, ſince by our ſixth experiment it appears, that the air, when permitted, will by its own internal ſpring expand itſelf twice as much as *Merſennus* was able to expand it, by the heat even of a candent æolipile; yet the elater of the air was ſcarce able to break a very thin glaſs bubble, and utterly unable to break one ſomewhat thicker, within whoſe cavity it was impriſoned. Whereas air penned up and agitated by heat is able to perform ſo much more conſiderable effects, that (not to mention thoſe of rarefaction, that are more obvious) the learned Jeſuit *Cabæus* (he that writ of the load-ſtone) relates, that he ſaw a marble pillar (ſo vaſt, that three men together with diſplayed arms could not embrace it, and that 1000 yoke of oxen, drawing it ſeveral ways with all their ſtrength, could not have torn it aſunder) quite broken off in the midſt, by reaſon of ſome wood, which happening to be burnt juſt by the pillar, the heat proceeding from the neighbouring fire ſo rarefied ſome air or ſpirituous matter, which was ſhut up in the cavities of the marble, that it broke through the ſolid body of the ſtone to obtain room to expand itſelf.

P. Nichol. Cab. lib. 4. Meteor. Ariſtot.

I REMEMBER I have taken notice, that probably the reaſon, why the included air did not break the hermetically ſealed bubbles, that remained intire in our emptied receiver, was, that the air, being ſomething rarefied by the flame employed to cloſe the glaſs, its ſpring, upon the receſs of the heat, grew weaker than before. But though we reject not that gueſs, yet it will not in the preſent caſe ſerve the turn; becauſe that much ſmaller glaſs bubbles exactly cloſed will, by the included air (though agitated by the heat of a very moderate fire) be made to fly in pieces. Whether we may be aſſiſted to ſolve this problem, by conſidering, that the heat doth from within vehemently agitate the corpuſcles of the air, and add its aſſiſtance to the ſpring they had before, I ſhall not now examine; ſince I here but propoſe a problem, and that chiefly, that by this memorable ſtory of *Cabæus* notice may be taken of the prodigious power of rarefaction, which hereby appears capable of performing ſtranger things, than any of our experiments have hitherto aſcribed to it.

WE ſhould hence, my Lord, immediately proceed to the next experiment, but that we think it fit, on this occaſion, to acquaint you with what ſome former trials (though not made in our engine) have taught us, concerning what we would have diſcovered by the newly mentioned bubble that broke. And this the rather, becauſe (a great part of this letter ſuppoſing the gravity of the air) it will not be impertinent to determine more particularly than hitherto we have done, what gravity we aſcribe to it.

WE took then an æolipile made of copper, weighing ſix ounces, five drachms, and eight and forty grains: this being made as hot as we durſt make it, (for fear of melting the metal, or at leaſt the ſoder) was removed from the fire, and immediately ſtopped with hard wax, that no air at all might get in at the little hole, wont to be left in æolipiles for the fumes to iſſue out at: then the æolipile being ſuffered leiſurely to cool, was again weighed together with the wax that ſtopt it, and was found to weigh (by reaſon of the additional weight of the wax) ſix ounces, ſix drachms, and 39 grains. Laſtly, the wax being perforated without taking any of it out of the ſcale, the external air was ſuffered to ruſh in (which it did with ſome noiſe) and then the æolipile and wax, being again weighed, amounted to ſix ounces, ſix drachms, and 50 grains. So that the æolipile freed, as far as our fire could free it, from its air, weighed leſs than itſelf when repleniſhed with air, full eleven grains: that is, the air containable within the cavity of the æolipile amounted to eleven grains and ſomewhat more; I ſay ſomewhat more, becauſe of the particles of air, that were not driven by the fire out of the æolipile. And by the way (if there be no miſtake in the obſervations of the diligent *Merſennus*) it may ſeem ſtrange, that it ſhould ſo much differ from 2 or 3 of ours; in none of which we could rarefy the air in our æolipile (though made red-hot almoſt all over, and ſo immediately plunged into cold water) to half that degree, which he mentions, namely to 70 times its natural extent, unleſs it were, that the æolipile he employed was able to ſuſtain a more vehement heat than ours (which yet we kept in ſo great an one, that once the ſoder melting, it fell aſunder into the two hemiſpheres it conſiſts of).

The forementioned way of weighing the air by the help of an æolipile ſeems ſomewhat more exact than that, which *Merſennus* uſed, in that in ours the æolipile was not weighed till it was cold; whereas in his, being weighed red-hot, it is ſubject to loſe of its ſubſtance in the cooling, for (as we have elſewhere noted on another occaſion) copper heated red-hot is wont in the cooling to throw off little thin ſcales in ſuch plenty, that having purpoſely watched a copper æolipile during its refrigeration, we have ſeen the place round about it almoſt covered with thoſe little ſcales it had every way ſcattered: which, however they amount not to much, ought not to be overlooked, when it is ſo light a body as air that is to be weighed. We will not examine, whether the æolipile in cooling may not receive ſome little increment of weight, either from the vapid or ſaline ſteams, that wander up and down in the air; but we will rather mention, that (for the greater exactneſs) we employed to weigh our æolipile, both when filled only with air and when repleniſhed with water, a pair of ſcales, that would turn (as they ſpeak) with the fourth part of a grain.

As to the proportion of weight betwixt air and water, ſome learned men have attempted it by ways ſo unaccurate that they ſeem to have much miſtaken it. For (not to mention the improbable accounts of *Kepler* and others) the learned and diligent *Ricciolus*, having purpoſely endeavoured to inveſtigate this proportion by means of a thin bladder, eſtimates the weight of the air to that of the water to be as one to ten thouſand, or thereabouts. And indeed I remember, that having formerly, on a certain occaſion, weighed a large bladder full of air, and found it, when the air was all ſqueezed out, to have contained fourteen grains of air; I found the ſame bladder afterwards filled with water to contain very near 14 pound of that liquor: according to which account, the proportion of air to water was almoſt as a grain to a pound, that is, as 1 to above 7600. To this we may add, that on the other ſide, *Galileo* himſelf uſing another, (but an unaccurate way too) defined the air to be in weight to water but as one to 4 hundred. But the way formerly propoſed of weighing the air by an æolipile ſeems by great odds more exact; and (as far as we could gueſs) ſeemed to agree well enough with the experiment made in our receiver. Wherefore it will be beſt to truſt our æolipile in the inquiry we are about. And according to our obſervations, the water it contained amounting to one and twenty ounces and an half, and as much air as was requiſite to fill it weighing eleven grains, the proportion in gravity of air to water of the ſame bulk will be as one to 938. And though we could not fill the æolipile with water ſo exactly as we would, yet in regard we could not neither as perfectly as we would drive the air out of it by heat, we think the proportion may well enough hold: but thoſe, that are delighted with round numbers (as the phraſe is) will not be much miſtaken, if they reckon water to be near a thouſand times heavier than air. And (for farther proof, that we have made the proportion betwixt theſe two bodies rather greater than leſſer than indeed it is, and alſo to confirm our former obſervation of the weight of the air) we will add, that, having another time put ſome water into the æolipile before we ſet it on the fire, that the copious vapours of the rarefied liquor might the better drive out the air, we found, upon trial carefully made, that when the æolipile was refrigerated, and the included vapours were by the cold turned again into water (which could not have happened to the air, that the preceding ſteams expelled) the air, when it was let in, increaſed the weight of the æolipile as much as before, namely, eleven grains; though there were already in it twelve drachms and a half, beſides a couple of grains of water, which remained of that we had formerly put into it to drive out the air.

Merſennus indeed tells us, that by his account air is in weight to water, as 1 to 1356. And adds, that we may, without any danger, believe, that the gravity of water to that of air of a like bulk is not leſs than of 1300 to 1. And conſequently, that the quantity of air to a quantity of water equiponderant thereto is as 1300 to 1. But why we ſhould relinquiſh our own carefully repeated trials, I ſee not. Yet I am unwilling to reject thoſe of ſo accurate and uſeful a writer: and therefore ſhall propoſe a way of reconciling our differing obſervations, by preſenting, that the diſcrepance between them may probably ariſe from the differing conſiſtence of the air at *London* and at *Paris:* for our air being more cold and moiſt than that which your Lordſhip now breathes, may be ſuppoſed alſo to be a fourth or fifth part more heavy. I leave it to be conſidered, whether it be of any moment, that our obſervations were made in the midſt of winter; whereas his were perhaps made in ſome warmer time of the year. But I think it were not amiſs, that, by the method formerly propoſed, the gravity of the air were obſerved both in ſeveral countries, and in the ſame country, in the ſeveral ſeaſons of the year and differing temperatures of the weather. And I would give ſomething of value to know the weight of ſuch an æolipile as ours full of air, in the midſt of winter in *Nova Zembla*; if that be true, which we formerly took notice of, namely, that the Hollanders, who wintered there, found that air ſo thick, that their clock would not go.

If your Lordſhip ſhould now aſk me, if I could not by the help of theſe, and our other obſervations, decide the controverſies of our modern mathematicians about the height of the air or atmoſphere, by determining how high it doth indeed reach; I ſhould anſwer, that though it ſeems eaſy enough to ſhew, that divers famous and applauded writers have been miſtaken in aſſigning the height of the atmoſphere, yet it ſeems very difficult preciſely to define of what height it is. And becauſe we have hitherto but lightly touched upon a matter of ſuch importance, we preſume it will not be thought impertinent, upon this occaſion, to annex ſomething towards the elucidation of it.

What we have already tried and newly ſet down allows us to take it for granted, that (at leaſt about *London)* the proportion of gravity betwixt water and air, of equal bulk, is as of a thouſand to one.

The next thing therefore, that we are to inquire after, in order to our preſent deſign, is the difference in weight betwixt water and quickſilver: and though this hath been defined already by the illuſtrious *Verulam*, and ſome other inquiſitive perſons, that have compared the weight of ſeveral bodies, and caſt their obſervations into tables, yet we ſhall not ſcruple to annex our own trials about it; partly, becauſe we find authors conſiderably to diſagree; partly, becauſe we uſed exacter ſcales, and a ſomewhat more wary method than others ſeem to have done; and partly alſo, becauſe, having proſecuted our inquiry by two or three ſeveral ways, the ſmall difference between the events may aſſure us, that we were not much miſtaken.

We took then a glaſs pipe, of the form of an inverted ſiphon, whoſe ſhape is delineated in the ſixteenth figure; and pouring into it a quantity of quickſilver, we held it ſo, that the ſuperficies of the liquor, both in the longer and ſhorter leg, lay in a horizontal line, denoted in the ſcheme by the pricked line E F: then pouring water into the longer leg of the ſiphon, till that was almoſt filled, we obſerved the ſurface of the quickſilver in that leg to be, by the weight of the water, depreſſed, as from E to B; and in the ſhorter leg, to be as much impelled upwards as from F to C. Whereupon having formerly ſtuck marks, as well at the point B, as at the oppoſite point D, we meaſured both the diſtance D C to have the height of the cylinder of quickſilver, which was raiſed above the point D (level with the ſurface of the quickſilver in the other leg) by the weight of the water, and the diſtance B A, which gave us the height of the cylinder of water. So that the diſtance D C amounting to $2\frac{13}{54}$ inches, and the height of the water amounting to $30\frac{45}{54}$ inches; and the whole numbers on both ſides, with the annexed fractions, being reduced to improper fractions of the ſame denomination, the proportion appeared to be (the denominators being left out as equal on both ſides) as 121 to 1665; or by reduction, as one to $\frac{92}{121}$.

Besides this unuſual way of determining the gravity of ſome things, we meaſured the proportion betwixt quickſilver and water, by the help of ſo exact a balance, as loſeth its æquilibrium by the hundredth part of a grain. But becauſe there is wont to be committed an overſight in weighing quickſilver and water, eſpecially if the orifice of the veſſel wherein they are put be any thing wide, in regard that men heed not that the ſurface of water in veſſels will be concave, but that of quickſilver notably convex or protuberant; to avoid this uſual overſight (I ſay) we made uſe of a glaſs bubble, blown very thin at the flame of a lamp, that it might not be too heavy for the balance, and terminating in a very ſlender neck wherein the concavity or convexity of a liquor could not be conſiderable; this glaſs, weighing $23\frac{1}{2}$ grains, we filled almoſt with quickſilver, and faſtening a mark over againſt the middle of the protuberant ſuperficies as near as our eyes could judge, we found that the quickſilver alone weighed $299\frac{7}{32}$ grains; then the quickſilver being poured out, and the ſame glaſs being filled as full of common water, we found the liquor to weigh $21\frac{7}{8}$ grains. Whereby it appeared the weight of water to quickſilver, is as one to $13\frac{19}{28}$; though our illuſtrious *Verulam* (queſtionleſs not for want of judgment or care, but of exact inſtruments) makes the proportion betwixt thoſe two liquors to be greater than of 1 to 17. And to add, that upon the bye, ſince quickſilver and well rectified ſpirit of wine, are (how juſtly I ſay not) accounted, the one the heavieſt, and the other the lighteſt of liquors; we thought to fill the ſame glaſs, and with the ſame ſcales to obſerve the difference betwixt them, which we found to be as of 1 to $16\frac{641}{1084}$; whereby it appeared, that the difference betwixt ſpirit of wine, that may be made to burn all away (ſuch as was ours) and common water, is as betwixt 1 and $1\frac{44}{171}$.

We might here take occaſion to admire, that though water (as appeared by the experiment formerly mentioned of the pewter veſſel) ſeems not capable of any conſiderable condenſation, and ſeems not to have interſperſed into it any ſtore of air; yet quickſilver, of no greater bulk than water, ſhould weigh near fourteen times as much. But having only pointed at this as a thing worthy of conſideration, we will proceed in our inquiry after the height of the atmoſphere: and to avoid the trouble of fractions, we will aſſume, that quickſilver is fourteen times as heavy as water, ſince it wants ſo little of being ſo.

Wherefore, having now given us the proportion of air to water, and water to quickſilver, it will be very eaſy to find the proportion betwixt air and quickſilver, in caſe we will ſuppoſe the atmoſphere to be uniformly of ſuch a conſiſtence as the air we weighed here below. For ſince our engine hath ſufficiently manifeſted that it is the æquilibrium with the external air, that in the Torricellian experiment keeps the quickſilver from ſubſiding; and ſince, by our accurate experiment formerly mentioned, it appears that a cylinder of mercury, able to balance a cylinder of the whole atmoſphere, amounted to near about thirty inches; and ſince, conſequently we may aſſume the proportion of quickſilver to air to be as fourteen thouſand to one; it will follow, that a cylinder of air, capable to maintain an æquilibrium, with a mercurial cylinder of two feet and a half in height, muſt amount to 35,000 feet of our Engliſh meaſure; and conſequently (reckoning five feet to a geometrical pace, and one thouſand ſuch paces to a mile) to ſeven full miles.

BUT this (as we lately intimated) proceeds upon the ſuppoſition, that the air is every where of the ſame conſiſtence that we found it near the ſurface of the earth; but that cannot with any ſafety be concluded, not only for the reaſon I find to have been taken notice of by the ancients, and thus expreſſed in *Seneca: Omnis aër* (he ſays) *quo propior eſt terris, hoc craſſior*; *quemadmodum in aqua & in omni humore fæx ima eſt, ita in aëre ſpiſſiſſima quæque deſidunt.* But much more, becauſe the ſpringy texture of the aërial corpuſcles makes them capable of a very great compreſſion, which the weight of the incumbent part of the atmoſphere is very ſufficient to give thoſe that be undermoſt and near the ſurface of the earth. And if we recal to mind thoſe former experiments, whereby we have manifeſted that air, much rarefied without heat, may eaſily admit a farther rarefaction from heat, and that the air, even without being expanded by heat, is capable of being rarefied to above one hundred and fifty times the extent it uſually poſſeſſeth here below; how can it be demonſtrated that the atmoſphere may not, for aught we know, or at leaſt for aught can be determined by our ſtatical and mechanical experiments, riſe to the height of five and twenty German leagues, if not of ſome hundred of common miles?

Senec. *Nat. quæſt. Lib.* 4. *cap.* 10.

AND this conjecture it ſelf may appear very injurious to the height whereunto exhalations may aſcend, if we will allow that there was no miſtake in that ſtrange obſervation made at *Tolouſe* in a clear night in *Auguſt*, by the diligent Mathematician *Emanuel Magnan*, and thus recorded by *Ricciolus*, for I have not at hand the author's own book: *Vidit* (ſays he) *ab hora undecima poſt meridiem uſque ad mediam noctem lunâ infra horizontem poſitâ, nubeculam quandam lucidam prope meridianum fere uſque ad Zenith diffuſam, quæ conſideratis omnibus non poterat niſi à ſole illuminari*; *ideoque altior eſſe debuit tota umbra terræ. Addit* (continues *Ricciolus*) *ſimile quid eveniſſe Michaeli Angelo Riccio apud Sabinos verſanti, nempe viro in matheſi eruditiſſimo.*

Ricciol. *Alma. Nov. Tom.* 2. *lib.* 10. *ſect.* 6. *prop.* 50. *Ex Magnan. lib.* 1. *Perſpectivæ horariæ prop.* 38.

VARIOUS obſervations made at the feet, tops, and interjacent parts of high mountains, might perchance ſomewhat aſſiſt us to make an eſtimate in what proportion, if in any certain one, the higher air is thicker than the lower, and gueſs at the difform conſiſtence, as to laxity and compactneſs of the air, at ſeveral diſtances from us. And if the difficulties about the refractions of the celeſtial lights were ſatisfactorily determined, that might alſo much conduce to the placing due limits to the atmoſphere (whoſe dimenſions thoſe obſervations about refractions ſeem hitherto much to contract). But for the preſent we dare not pronounce any thing peremptorily concerning the height of it, but leave it to farther enquiry: contenting ourſelves to have manifeſted the miſtakes of divers eminent modern writers, who will not allow the atmoſphere to exceed above two or three miles in height (as the famous *Kepler* will not the *Aër refractivus*) and to have rendered a reaſon why, in the mention we made in the notes upon the firſt experiment touching the height of the atmoſphere, we ſcrupled not to ſpeak of it, as if it might be many miles high.

EXPERIMENT XXXVII.

WE will now proceed to recite a phænomenon, which, though made amongſt the firſt, we thought fit not to mention till after many others, that we might have the opportunity to obſerve as many circumſtances of it as we could, and ſo preſent your Lordſhip at once, moſt of what we at ſeveral times have taken notice of concerning ſo odd a phænomenon.

OUR engine had not been long finiſhed, when, at the firſt leiſure we could ſteal from our occaſions to make trial of it, we cauſed the air to be pumped out of the receiver; and whilſt I was buſied in entertaining a learned friend that juſt then came to viſit me, an ingenious byſtander thought he perceived ſome new kind of light in the receiver, of which giving me haſtily notice, my friend and I preſently obſerved, that when the ſucker was drawn down, immediately upon the turning of the key, there appeared a kind of light in the receiver, almoſt like a faint flaſh of lightning in the day-time, and almoſt as ſuddenly did it appear and vaniſh. Having, not without ſome amazement, obſerved divers of theſe apparitions of light, we took notice that the day was clear, the hour about ten in the morning, that the only window in the room faced the north; and alſo, that by interpoſing a cloke, or any opacous body between the receiver and the window, though the reſt of the room were ſufficiently enlightened, yet the flaſhes did not appear as before, unleſs the opacous body were removed. But not being able on all theſe circumſtances to ground any firm conjecture at the cauſe of this ſurprizing phænomenon, as ſoon as night was come, we made the room very dark; and plying the pump, as in the morning, we could not, though we often tried, find, upon the turning of the key, ſo much as the leaſt glimmering of light; whence we inferred, that the flaſh appearing in the receiver, did not proceed from any new light generated there, but from ſome reflections of the light of the ſun, or other luminous bodies placed without it; though whence the reflection ſhould proceed, it poſed us to conjecture.

WHEREFORE the next morning, hoping to inform ourſelves better, we went about to repeat the experiment; but though we could as well as formerly exhauſt the receiver, though the place wherein we made the trial was the very ſame, and though other circumſtances were reſembling; yet we could not diſcover the leaſt appearance of light all that day, nor on divers others, on which trial was again fruitleſly made; nor can we to this very time be ſure a day before-hand that theſe flaſhes will be to be ſeen in our great receiver. Nay, having once found the engine in a good humour (if I may ſo ſpeak) to ſhew this trick, and ſent notice of it to our learned friend Dr. *Wallis*, who expreſſed a great deſire to ſee this phænomenon, though he were not then above a bow-ſhoot off, and made haſte to ſatisfy his curioſity; yet by that time he was come, the thing he came for was no longer to be ſeen: ſo that having vainly endeavoured to exhibit again the phænomenon in his preſence, I began to apprehend what he might think of me, when unexpectedly the engine preſented us a flaſh, and after that a ſecond, and as many more as ſufficed to ſatisfy him that we might very well confidently relate, that we have ourſelves ſeen this phænomenon, though not confidently promiſe to ſhew it others.

AND this unſucceſsfulneſs whereto our experiment is liable, being ſuch, that by all our watchfulneſs and trials we could never reduce it to any certain rules or obſervations, ſince in all conſtitutions of the weather, times of the day, *&c.* it will ſometimes anſwer, and ſometimes diſappoint our expectations; we are much diſcouraged from venturing to frame an hypotheſis to give an account of it: which if the experiment did conſtantly ſucceed, might the more hopefully be attempted, by the help of the following phænomena laid together; ſome of them produced upon trials purpoſely made to examine the validity of the conjectures other trials had ſuggeſted.

FIRST then we obſerved, that the apparition of light may be made as well by candle-light, as by day-light; and in whatever poſition the candle be held, in reference to the receiver, as on this or that hand of it, above it, beneath it, or any other way, provided the beams of light be not hindered from falling upon the veſſel.

NEXT, we noted that the flaſh appears immediately upon the turning of the key, to let the air out of the receiver into the emptied cylinder, inſomuch that I remember not that when at any time in our great receiver, the ſtop-cock was opened before the cylinder was exhauſted (whereby it came to paſs that the air did rather deſcend, than ruſh into the cylinder) the often mentioned flaſh appeared to our eyes.

YET, we farther obſerved, that when inſtead of the great receiver we made uſe of a ſmall glaſs, not containing above a pound and a half of water, the phænomenon might be exhibited though the ſtop-cock were open, provided the ſucker were drawn nimbly down.

WE noted too, that when we began to empty the receiver, the appearances of light were much more conſpicuous than towards the latter end, when little air at a time could paſs out of the receiver.

WE obſerved alſo, that when the ſucker had not been long before well oiled, and, inſtead of the great receiver, the ſmaller veſſel above mentioned was emptied; we obſerved, I ſay, that then, upon the opening of the ſtop-cock, as the air deſcended out of the glaſs into the emptied cylinder, ſo at the ſame time there aſcended out of the cylinder into the veſſel a certain ſteam, which ſeemed to conſiſt of very little bubbles, or other minute corpuſcles thrown up from the oil, rarefied by the attrition it ſuffered in the cylinder. For at the ſame time that theſe ſteams aſcended into the glaſs, ſome of the ſame kind manifeſtly iſſued out like a little pillar of ſmoke at the orifice of the valve, when that was occaſionally opened. And theſe ſteams frequently enough preſenting themſelves to our view, we found, by expoſing the glaſs to a clear light, that they were wont to play up and down in it, and ſo by their whitiſhneſs to emulate, in ſome meaſure, the apparition of light.

FOR we likewiſe ſometimes found, by watchful obſervation, that when the flaſh was great, not only at the very inſtant the receiver loſt of its tranſparency, by appearing full of ſome kind of whitiſh ſubſtance; but that for ſome ſhort time after, the ſides of the glaſs continued ſomewhat opacous, and ſeemed to be darkened, as if ſome whitiſh ſteam adhered to the inſide of them.

HE that would render a reaſon of the phænomenon, whereof all theſe are not all the circumſtances, muſt do two things; whereof the one is difficult, and the other little leſs than impoſſible: for he muſt give an account not only whence the appearing whiteneſs proceeds, but wherefore that whiteneſs doth ſometimes appear, and ſometimes not.

FOR our part, we freely confeſs ourſelves at a loſs about rendering a reaſon of the leſs difficult part of the problem; and though your Lordſhip ſhould even preſs us to declare what conjecture it was, that the above-recited circumſtance ſuggeſted to us, we ſhould propoſe the thoughts we then had, no otherwiſe than as bare conjectures.

IN caſe then our phænomenon had conſtantly and uniformly appeared, we ſhould have ſuſpected it to have been produced after ſome ſuch manner as follows:

FIRST, we obſerved that, though that which we ſaw in our receiver ſeemed to be ſome kind of light, yet it was indeed but a whiteneſs which did (as hath already been noted) opacate (as ſome ſpeak) the inſide of the glaſs.

NEXT, we conſidered, that our common air abounds with particles, or little bodies, capable to reflect the beams of light. Of this we might eaſily give divers proofs, but we ſhall name but two: the one, that vulgar obſervation of the motes that appear in multitudes ſwimming up and down in the air, when the ſun-beams ſhooting into a room, or any other ſhady place, diſcover them, though otherwiſe the eye cannot diſtinguiſh them from the reſt of the air; the other proof we will take from what we (and no doubt very many others) have obſerved, touching the illumination of the air in the night. And we particularly remember, that, being at ſome diſtance from *London* one night, that the people, upon a very welcome occaſion, teſtified their joy by numerous bonfires; though, by reaſon of the interpoſition of the houſes, we could not ſee the fires themſelves, yet we could plainly ſee the air all enlightened over and near the city; which argued, that the lucid beams ſhot upwards from the fires, met in the air with corpuſcles opacous enough to reflect them to our eyes.

A third thing that we conſidered, was, that white may be produced (without excluding other ways, or denying inviſible pores in the ſolideſt bodies) when the continuity of a diaphanous body happens to be interrupted by a great number of ſurfaces, which, like ſo many little looking-glaſſes, do confuſedly repreſent a multitude of little and ſeemingly contiguous images of the lucid body. We ſhall not inſiſt on the explanation of this, but refer you for it to what we have ſaid in another paper (touching colours). But the inſtances that ſeem to prove it are obvious: for water or whites of eggs beaten to froth, do loſe their tranſparency, and appear white. And having out of one of our leſſer receivers carefully drawn out the air, and ſo ordered it, that the hole by which the water was to get in, was exceeding ſmall, that the liquor might be the more broken in its paſſage thorough it, we obſerved with pleaſure, that the neck being held under water, and the little hole newly mentioned being opened, the water that ruſhed in was ſo broken, and acquired ſuch a multitude of new ſurfaces, that the receiver ſeemed to be full rather of milk than water. We have likewiſe found out, that by heating a lump of cryſtal to a certain degree, and quenching it in fair water, it would be diſcontinued by ſuch a multitude of cracks (which created new ſurfaces within it) that though it would not fall aſunder, but retain its former ſhape, yet it would loſe its tranſparency, and appear white.

Upon theſe conſiderations, my Lord, and ſome others, it ſeemed not abſurd to imagine, that upon the ruſhing of the air out of the receiver into the emptied cylinder, the air in the receiver being ſuddenly and vehemently expanded, the texture of it was as ſuddenly altered, and the parts made ſo to ſhift places (and perhaps ſome of them to change poſtures) as during their new and vehement motion, and their varied ſituation, to diſturb the wonted continuity, and ſo the diaphaneity of the air; which (as we have already noted) upon its ceaſing to be a tranſparent body, without the interpoſition of coloured things, muſt eaſily degenerate into white.

Several things there were that made this conjecture ſeem the leſs improbable. As firſt, that the whiteneſs always appeared greater when the exſuction began to be made, whilſt there was ſtore of air in the receiver, than when the air was in great part drawn out. And next, that, having exhauſted the receiver, and applied to the hole in the ſtop-cock a large bubble of clear glaſs, in ſuch a manner, that we could at pleaſure let the air paſs out at the ſmall glaſs into the great one, and eaſily fill the ſmall one with air again, we obſerved with pleaſure, that, upon the opening the paſſage betwixt the two glaſſes, the air in the ſmaller having ſo much room in the greater to receive it, the diſſilition of that air was ſo great, that the ſmall phial ſeemed to be full of milk; and this experiment we repeated ſeveral times. To which we may add, that, having provided a ſmall receiver, whoſe upper orifice was ſo narrow that I could ſtop it with my thumb, I obſerved, that when, upon the exſuction of the air, the capacity of the glaſs appeared white, if, by a ſudden removal of my thumb, I let in the outward air, that whiteneſs would immediately vaniſh. And whereas it may be objected, that in the inſtance formerly mentioned, water turning from perſpicuous to white, there intervenes the air, which is a body of a heterogeneous nature, and muſt turn it into bubbles to make it loſe its tranſparency; we may borrow an anſwer from an experiment we deliver in another treatiſe, where we teach, how to make two very volatile liquors, which being gently put together, are clear as rock-water, and yet will almoſt in a moment, without the ſubingreſſion of air to turn them into bubbles, ſo alter the diſpoſition of their inſenſible parts, as to become a white and conſiſtent body. And this happens not as in the precipitation of benjamin, and ſome other reſinous bodies, which being diſſolved in ſpirit of wine, may, by the effuſion of fair water, be turned into a ſeemingly milky ſubſtance. For this whiteneſs belongs not to the whole liquor, but to the corpuſcles of the diſſolved gum, which, after a while ſubſiding, leave the liquor tranſparent, themſelves only remaining white; whereas in our caſe, it is from the varied texture of the whole formerly tranſparent fluid body, and not from this or that part, that this whiteneſs reſults: for the body is white throughout, and will long continue ſo; and yet may, in proceſs of time, without any addition, be totally reduced into a tranſparent body as before.

BUT beſides the conjecture inſiſted on all this while, we grounded another upon the following obſervation; which was, that having conveyed ſome ſmoke into our receiver placed againſt a window, we obſerved, that upon the exſuction of the air, the corpuſcles that were ſwimming in it, did manifeſtly enough make the receiver ſeem more opacous at the very moment of the ruſhing out of the air: for conſidering that the whiteneſs, whoſe cauſe we inquire of, did but ſometimes appear, it ſeemed not impoſſible but that at ſuch times the air in the receiver might abound with particles, capable of reflecting the light in the manner requiſite to exhibit a white colour, by their being put into a certain unuſual motion: as may be in ſome meaſure illuſtrated by this, that the new motion of the freſhly mentioned fumes made the inſide of the receiver appear ſomewhat darker than before; and partly by the nature of our formerly mentioned ſmoking liquor, whoſe parts, though they ſeemed tranſparent whilſt they compoſed a liquor, yet when the ſame corpuſcles, upon the unſtopping of the glaſs, were put into a new motion, and diſpoſed after a new manner, they did opacate that part of the air they moved in, and exhibited a greater whiteneſs than that which ſometimes appears in our pneumatical veſſel. Nor ſhould we content ourſelves with this ſingle inſtance, to manifeſt, that little bodies, which being ranged after one manner are diaphanous and colourleſs, may, by being barely agitated, diſperſed, and conſequently otherways ranged, exhibit a colour, if we were not unwilling to rob our collection of experiments concerning colours.

BUT, my Lord, I foreſee you may make ſome objections againſt our propoſed gueſs, which perhaps I ſhall ſcarce be able to anſwer; eſpecially, if you inſiſt upon having me render a reaſon why our phænomenon appears not conſtantly.

I MIGHT indeed anſwer, that probably it would do ſo, if inſtead of our great receiver we uſe ſuch a ſmall phial as we have lately mentioned, wherein the diſſilition of the air being much greater, is like to be the more conſpicuous: ſince I remember not that we ever made our trial with ſuch ſmall veſſels, without finding the expected whiteneſs to appear. But it would remain to be explicated, why in our great receiver the phænomenon ſhould ſometimes be ſeen, and oftentimes not appear. And though that conjecture which we laſt made ſhould not be rejected, yet if were farther preſſed to aſſign a reaſon why the air ſhould abound with ſuch particles, as we there ſuppoſe, more at one time than another, we are not yet provided of any better anſwer than this general one, that the air about us (and much more that within the receiver) may be much altered by ſuch cauſes as few are aware of: for, not to repeat thoſe probable arguments of this aſſertion which we have

occaſionally

Joſeph Acoſta's *Nat. and Mor. Hiſt. of the* Indies, lib. 3. cap. 9.

occaſionally mentioned here and there in the former part of this epiſtle, we will here ſet down two or three inſtances to verify the ſame propoſition. Firſt, I find that the learned *Joſephus Acoſta*, among other judicious obſervations he made in *America*, hath this concerning the effects of ſome winds: *There are*, ſaith he, *winds which naturally trouble the water of the ſea, and make it green, and black*; *others, clear as cryſtal.* Next, we have obſerved, that though we conveyed into the receiver our ſcales, and the pendula formerly mentioned, clean and bright; yet after the receiver had been emptied, and the air let in again, the gloſs or luſtre both of the one, and of the other, appeared tarniſhed by a beginning ruſt. And in the laſt place, we will ſubjoin an obſervation we made ſome years ago, which hath been heard of by divers ingenious men, and ſeen by ſome of them: we had, with pure ſpirit of wine, drawn a tincture out of a certain concrete, which uſeth to be reckoned among mineral bodies; and this tincture being very pure and tranſparent, we did, becauſe we put a great value upon it, put into a cryſtal phial, which we carefully ſtopped, and locked up in a preſs among ſome other things that we ſpecially prized. This liquor being a chymical rarity, and beſides very defecate, and of a pleaſing golden colour; we had often occaſion to look upon it, and ſo to take notice, that one time it ſeemed to be very much troubled, and not clear as it was wont to be: whereupon we imagined, that though it would be ſomething ſtrange, yet it was not impoſſible that ſome precipitation of the mineral corpuſcles was then happening, and that thence the liquor was opacated. But, finding after ſome days, that though the expected precipitation had not been made, yet the liquor, retaining its former vivid colour, was grown clear again as before, we ſomewhat wondered at it; and locking it up again in the ſame preſs, we reſolved to obſerve, both whether the like changes would again appear in our tincture, and whether in caſe they ſhould appear, they would be aſcribable to the alterations of the weather. But though, during the greateſt part of a winter and a ſpring, we took pleaſure to obſerve, how the liquor would often grow turbid, and after a while clear again; yet we could not find that theſe mutations depended upon any that were manifeſt in the air, which would be often dark and clouded, when the tincture was clear and tranſparent; as, on the other ſide, in clear weather the liquor would appear ſometimes troubled, and more opacous. So that being unable to give an account of theſe odd changes in our tincture (which we ſuppoſe we have not yet loſt, though we know not whether it hath loſt its fickle nature) either by thoſe of the air, or any thing elſe that occurred to our thoughts; we could not but ſuſpect, that there may be in divers bodies, as it were ſpontaneous mutations, that is, ſuch changes as depend not upon manifeſt cauſes. But, my Lord, what hath beenall this while ſaid concerning our phænomenon, is offered to you, not as containing a ſatiſfactory account of it, but to aſſiſt you to give yourſelf one.

EXPERIMENT XXXVIII.

WE took a glaſs veſſel, open at the top, and into it we put a mixture of ſnow and common ſalt, (ſuch a mixture as we have in another treatiſe largely diſcourſed of;) and into the midſt of this mixture we ſet a glaſs of a cylindrical form, cloſely ſtopped at the lower end with plaiſter, and open at the upper, at which we filled it with common water. Theſe things being let down into the receiver, and the pump being ſet on work, the ſnow began to melt ſomewhat faſter than we expected; whether upon the account of the exſuction of the air, or becauſe there was but little of the ſnow, or whether for any other reaſon, it appeared doubtful. But however, by that time the receiver had been conſiderably exhauſted, which was done in leſs than $\frac{1}{4}$ of an hour, we perceived the water near the bottom of the glaſs cylinder to freeze, and the ice, by a little longer ſtay, ſeemed to increaſe, and to riſe ſomewhat higher than the ſurface of the ſurrounding liquor, whereinto almoſt all the ſnow and ſalt were reſolved. The glaſs being taken out, it appeared that the ice was as thick as the inſide of the glaſs it filled, though into that I could put my thumb. The upper ſurface of the ice was very concave, which, whether it were due to any unheeded accident, or to the exſuction of the air, we leave to be determined by farther trial. And laſtly, the ice held againſt the light, appeared not deſtitute of bubbles, though ſome byſtanders thought they were fewer than would have been found if the water had been frozen in the open air. The like experiment we tried alſo another time in one of our ſmall receivers, with not unlike ſucceſs.

AND on this occaſion, my Lord, give me leave to propoſe a problem, which ſhall be this: whence proceeds that ſtrange force that we may ſometimes obſerve in frozen water, to break the bodies that impriſon it, though hard and ſolid? That there is ſuch a force in water expoſed to congelation, may be gathered not only from what may be often obſerved in winter, of the burſting of glaſſes too cloſe ſtopped, filled with water or aqueous liquors, but by inſtances as much more conſiderable as leſs obvious. For I remember, that an ingenious ſtone-cutter not long ſince complained to me, that ſometimes, through the negligence of ſervants, the rain being ſuffered to ſoak into marble ſtones, the ſupervening violent froſts would burſt the ſtones, to the poſſeſſor's no ſmall damage. And I remember another tradeſman, in whoſe houſe I had lodgings, was laſt winter complaining, that even implements made of bell-metal, being careleſsly expoſed to the wet, have been broken and ſpoiled by the water; which, having gotten into the little cavities and crannies of the metal, was there afterwards frozen and expanded into ice. And to theſe relations, we can add one of the formerly mentioned *Cabæus*'s, whereby they not only may be confirmed, but are ſurpaſſed: for he tells us, that he ſaw a huge veſſel of exceeding hard marble ſplit aſunder by congealed water, whoſe rarefaction, ſaith our author, proved ſo vehement, that the hardneſs of the ſtone yielded to it; and ſo a veſſel was broken, which would not have been ſo by 100 yoke of oxen drawing it ſeveral ways. I know, my Lord, that to ſolve this problem, it will be ſaid, that congelation doth not (as is commonly, but erroneouſly preſumed) reduce water into leſs room than it poſſeſſed before, but rather makes it take up more. And I have elſewhere proved by particular experiments, that whether or no ice may be truly ſaid to be water rarefied (for that ſeems queſtionable) it may be ſaid to take up more room than the water did before glaciation. But though we grant that freezing makes water ſwell, yet, how cold (which in weather-glaſſes manifeſtly condenſeth the air) ſhould expand either the water, or the intercepted air ſo forcibly, as to perform ſuch things as we have newly related, will yet remain a problem.

In lib. 4. Meteor. Ariſt.

EXPERIMENT XXXIX.

WE took an oval glaſs, clear and (leſt it ſhould break) pretty ſtrong, with a ſhort neck at the obtuſer end; through this neck, we thruſt almoſt to the bottom, a pipe of glaſs, which was cloſely cemented to the newly mentioned neck; the upper part of which pipe was drawn in ſome places more ſlender than a crow's quill, that the changes of the air in that glaſs egg might be the more conſpicuous.

Then there was conveyed into the glaſs five or ſix ſpoonfuls of water, part of which, by blowing air into the egg, was raiſed into the abovementioned ſlender part of the pipe, ſo that the water was interpoſed between the external air, and that included in the egg. This weather glaſs (delineated in the fourteenth figure) was ſo placed, and cloſed up in the cavity of one of our ſmall receivers, that only the ſlender part of the pipe, to the height of four or five inches, paſſing thorough a hole in the cover, remained expoſed to the open air.

The pump being ſet at work, upon the exſuction of the air, the water in the pipe deſcended about a quarter of an inch, and this upon two or three reiterated trials; which ſeemed ſufficiently to argue, that there was no heat produced in the receiver upon the exſuction of the air: for even a little heat would probably have been diſcovered by that weather-glaſs, ſince upon the bare application of my hand to the outſide of the receiver, the warmth having after ſome time been communicated or propagated through both the glaſſes, and the interval betwixt them, to the impriſoned air, did ſo rarefy that, as to enable it, by preſſing upon the ſubjacent water, to impel that in the pipe very many times as far as it had fallen downwards upon the exſuction of the air.

Yet ſhall not we conclude, that in the cavity of the receiver the cold was greater after the exſuction of the air than before.

For if it be demanded what then could cauſe the forementioned ſubſiding of the water? it may be anſwered, that probably it was the reaching of the glaſs egg, which, upon the exſuction of the ambient air, was unable to reſiſt altogether as much as formerly the preſſure of the included air, and of the atmoſphere, which, by the intervention of the water, preſſed upon its concave ſurface. Which ſeemed probable, as well by what was above delivered, in the experiment about the breaking of the glaſs by the force of the atmoſphere, as by this notable circumſtance (which we divers times obſerved) that when by drawing the air out of the receiver, the water in the pipe was ſubſided, upon the readmiſſion of the external air, to preſs againſt the convex ſurface of the egg, the water was preſently reimpelled to its former height: which would perhaps appear leſs ſtrange to your Lordſhip, if you had yet ſeen, what we have heretofore taught in another treatiſe, concerning the ſpring that may be diſcovered in glaſs, as rigid and inflexible a body as it is generally eſteemed. And in the mean while it may ſerve the turn, to cauſe a glaſs egg to be blown exceeding thin; and then, having broken it, try how far you can by degrees bend ſome narrow parts of it; and how readily, upon the removal of what kept it bent, it will reſtore itſelf to its former ſtate or poſture. But to return to our experiment: from thence it ſeems probable, either that there ſucceeds no body in the room of the air drawn out of our receiver; or that it is not every matter that is ſubtile enough readily to paſs through the pores of glaſs, that is always agitated enough to produce heat wherever it is plentifully found. So that if no vacuum be to be admitted, this experiment ſeems to invite us to allow a great diſparity, either as to bulk, or as to agitation, or as to both, betwixt ſome parts of the ethereal ſubſtance, and thoſe that are wont here below to produce heat and fire.

We tried alſo what operation the drawing out of the air would have upon camphire, that being a body, which, though not a liquor, conſiſts of ſuch volatile or fugitive parts, that without any greater agitation than that of the open air itſelf, they will copiouſly fly away. But we found not that even this looſe body was ſenſibly altered by the exſuction of the ambient air.

EXPERIMENT XL.

IT may ſeem well worth trying, whether or no in our exhauſted glaſs the want of an ambient body, of the wonted thickneſs of air, would diſable even light and little animals, as bees, and other winged inſects, to fly. But though we eaſily foreſaw how difficult it would be to make ſuch an experiment, yet not to omit our endeavours, we procured a large fleſh-fly, which we conveyed into a ſmall receiver. We alſo another time ſhut into a great receiver a humming bee, that appeared ſtrong and lively, though we had rather have made the trial with a butterfly, if the cold ſeaſon would have permitted us to find any *. The fly, after ſome exſuctions of the air, dropped down from the ſide of the glaſs whereon ſhe was walking. But, that the experiment with the bee might be the more inſtructive, we conveyed in with her a bundle of flowers, which remained ſuſpended by a ſtring near the upper part of the receiver; and having provoked the bee, we excited her to fly up and down the capacity of the veſſel, till at length, as we deſired, ſhe lighted upon the flowers: whereupon we preſently began to draw out the air, and obſerved, that though for ſome time the bee ſeemed to take no notice of it, yet within a while after ſhe did not fly, but fall down from the flowers, without appearing to make any uſe of her wings to help herſelf. But whether this fall of the bee, and the other inſect, proceeded from the medium's being too thin for them to fly in, or barely from the weakneſs, and as it were ſwooning of the animals themſelves, you will eaſily gather from the following experiment.

EXPERIMENT XLI.

TO ſatisfy ourſelves in ſome meaſure about the account upon which reſpiration is ſo neceſſary to the animals that nature hath furniſhed with lungs, we took (being then unable to procure any other lively bird, ſmall enough to be put into the receiver) a lark, one of whoſe wings had been broken by a ſhot of a man that we had ſent to provide us ſome birds for our experiment; but notwithſtanding this hurt, the lark was very lively, and did, being put into the receiver, divers times ſpring up in it to a good height. The veſſel being haſtily, but carefully cloſed, the pump was diligently plied, and the bird for a while appeared lively enough; but upon a greater exſuction of the air, ſhe began manifeſtly to droop and appear ſick, and very ſoon after was taken with as violent and irregular convulſions, as are wont to be obſerved, in poultry, when their heads are wrung off: for the bird threw herſelf over and over two or three times, and died with her breaſt upward, her head downwards, and her neck awry. And though upon the appearing of theſe convulſions, we turned the ſtop-cock, and let in the air upon her, yet it came too late; whereupon caſting our eyes upon one of thoſe accurate dials that go with a pendulum, and were of late ingeniouſly invented by the noble and learned *Hugenius*, we found that the whole tragedy had been concluded within ten minutes of an hour, part of which time had been employed in cementing the cover to the receiver. Soon after we got a henſparrow, which being caught with bird-lime was not at all hurt; when we put her into the receiver, almoſt to the top of which ſhe would briſkly raiſe herſelf, the experiment being tried with this bird, as it was with the former, ſhe ſeemed to be dead within ſeven minutes, one of which were employed in cementing on the cover: but upon the ſpeedy turning of the key, the freſh air flowing in, began ſlowly to revive her, ſo that after ſome pantings ſhe opened her eyes, and regained her feet, and in about ¼ of an hour after, threatned to make an eſcape at the top of the glaſs, which had been unſtopped to let in the freſh air upon her: but the receiver being cloſed the ſecond time, ſhe was killed with violent convulſions within five minutes from the beginning of the pumping.

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* Since the writing of this XLth experiment, we procured a white butterfly, and incloſed it one of our ſmaller receivers, where, though at firſt he fluttered up and down, yet preſently, upon the exſuction of the air, he fell down as in a ſwoon, retaining no other motion than ſome little trembling of the wings.

A WHILE after we put in a mouſe, newly taken, in ſuch a trap as had rather affrighted than hurt him; whilſt he was leaping up very high in the receiver, we faſtened the cover to it, expecting that an animal uſed to live in narrow holes with very little freſh air, would endure the want of it better than the lately mentioned birds: but though, for a while after the pump was ſet a work, he continued leaping up as before; yet, it was not long ere he began to appear ſick and giddy, and to ſtagger: after which he fell down as dead, but without ſuch violent convulſions as the bird died with. Whereupon, haſtily turning the key, we let in ſome freſh air upon him, by which he recovered, after a while, his ſenſes and his feet, but ſeemed to continue weak and ſick: but at length, growing able to ſkip as formerly, the pump was plied again for eight minutes, about the middle of which ſpace, if not before, a little air by a miſchance got in at the ſtop cock; and about two minutes after that, the mouſe divers times leaped up lively enough, though after about two minutes more he fell down quite dead, yet with convulſions far milder than thoſe wherewith the two birds expired. This alacrity ſo little before his death, and his not dying ſooner than at the end of the eighth minute, ſeemed aſcribable to the air (how little ſoever) that ſlipt into the receiver. For the firſt time, thoſe convulſions (that, if they had not been ſuddenly remedied, had immediately diſpatched him) ſeized on him in ſix minutes after the pump began to be ſet a work. Theſe experiments ſeemed the more ſtrange, in regard that during a great part of thoſe few minutes the engine could but inconſiderably rarefy the air (and that too, but by degrees) and at the end of them there remained in the receiver no inconſiderable quantity; as may appear by what we have formerly ſaid of our not being able to draw down water in a tube, within much leſs than a foot of the bottom: with which we likewiſe conſidered, that by the exſuction of the air and interſperſed vapours, there was left in the receiver a ſpace ſome hundreds of times exceeding the bigneſs of the animal, to receive the fuliginous ſteams, from which exſpiration diſcharges the lungs; and which, in the other caſes hitherto known, may be ſuſpected, for want of room, to ſtifle thoſe animals that are cloſely penned up in too narrow receptacles.

I FORGOT to mention, that having cauſed theſe three creatures to be opened, I could, in ſuch ſmall bodies, diſcover little of what we ſought for, and what we might poſſibly have found in larger animals; for though the lungs of the birds appeared very red, and as it were inflamed, yet that colour being uſual enough in the lungs of ſuch winged creatures, deſerves not ſo much our notice, as it doth, that in almoſt all the deſtructive experiments made in our engine, the animals appeared to die with violent convulſive motions: from which, whether phyſicians can gather any thing towards the diſcovery of the nature of convulſive diſtempers, I leave to them to conſider.

HAVING proceeded thus far, though (as we have partly intimated already) there appeared not much cauſe to doubt, but that the death of the forementioned animals proceeded rather from the want of air, than that the air was overclogged by the ſteams of their bodies, exquiſitely penned up in the glaſs; yet I, that love not to believe any thing upon conjectures, when by a not over-difficult experiment I can try whether it be true or no, thought it the ſafeſt way to obviate objections, and remove ſcruples, by ſhutting up another mouſe as cloſe as I could in the receiver; wherein it lived about three quarters of an hour, and might probably have done ſo, much longer, had not a Virtuoſo of quality, who in the mean while chanced to make me a viſit, deſired to ſee whether or no the mouſe could be killed by the exſuction of the ambient air: whereupon we thought fit to open, for a little while, an intercourſe betwixt the air in the receiver, and that without it, that the mouſe might thereby (if it were needful for him) be refreſhed; and yet we did this without uncementing the cover at the top, that it might not be objected, that perhaps the veſſel was more cloſely ſtopped for the exſuction of the air than before.

THE experiment had this event, that after the mouſe had lived ten minutes (which we aſcribed to this, that the pump, for want of having been lately oiled, could move but ſlowly, and could not by him that managed it be made to work as nimbly as it was wont) at the end of that time he died with convulſive fits, wherein he made two or three bounds into the air, before he fell down dead.

NOR was I content with this, but for your Lordſhip's farther ſatisfaction, and my own, I cauſed a mouſe, that was very hungry, to be ſhut in all night, with a bed of paper for him to reſt upon: and to be ſure that the receiver was well cloſed, I cauſed ſome air to be drawn out of it, whereby, perceiving that there was no ſenſible leak, I preſently readmitted the air at the ſtop-cock, leſt the want of it ſhould harm the little animal; and then I cauſed the engine to be kept all night by the fire-ſide, to keep him from being deſtroyed by the immoderate cold of the froſty night. And this care ſucceeded ſo well, that the next morning I found that the mouſe was not only alive, but had devoured a good part of the cheeſe that had been put in with him. And having thus kept him alive full twelve hours, or better, we did, by ſucking out part of the air, bring him to droop, and to appear ſwelled; and by letting in the air again, we ſoon reduced him to his former livelineſs.

A Digreſſion containing ſome Doubts touching RESPIRATION.

I FEAR your Lordſhip will now expect, that to theſe experiments I ſhould add my reflections on them, and attempt, by their aſſiſtance, to reſolve the difficulties that occur about reſpiration; ſince at the beginning I acknowledged a farther enquiry into the nature of that, to have been my deſign in the related trials. But I have yet, becauſe of the inconvenient ſeaſon of the year, made ſo few experiments, and have been ſo little ſatisfied by thoſe I have been able to make, that they have hitherto made reſpiration appear to me rather a more, than a leſs myſterious thing, than it did before. But yet, ſince they have furniſhed me with ſome ſuch new conſiderations, concerning the uſe of the air, as confirms me in my diffidence of the truth of what is commonly believed touching that matter; that I may not appear ſullen or lazy, I am content not to decline employing a few hours in ſetting down my doubts, in preſenting your Lordſhip ſome hints, and in conſidering whether the trials made in our engine will at leaſt aſſiſt us to diſcover wherein the deficiency lies that needs to be ſupplied.

AND this, my Lord, being all my preſent deſign, I ſuppoſe you will not expect that (as if you knew not, or had forgotten what Anatomiſts are wont to teach) I ſhould entertain you with a needleſs diſcourſe of the organs of reſpiration, and the variety of their ſtructure in ſeveral animals; though if it were neceſſary, and had not been performed by others, I ſhould think, with *Galen*, that by treating of the fabrics of living bodies, I might compoſe hymns to the wiſe author of nature, who, in the excellent contrivance of the lungs, and other parts of (thoſe admirable engines) animals, manifeſts himſelf to be indeed what the eloquent prophet moſt juſtly ſpeaks him, *wonderful in counſel, and excellent in working*.

Nor ſhall we any farther meddle with thoſe controverſies ſo much agitated among the moderns, namely, whether the motion of the lungs in reſpiration be their own, or but conſequent to the motion of the thorax, diaphragm, and (as ſome learned men would have it) the abdomen; and, whence it is that the air ſwells the lungs in inſpiration, any farther than they may receive light from our engine: but that it may appear what kind of ſervice it is that may be expected from it on this occaſion, we muſt premiſe a few words to ſhew wherein the ſtrength of the objection we are to anſwer, lies. In favour then of thoſe that would have the lungs rather paſſive than active in the buſineſs of reſpiration, it may againſt the common opinion be alledged, that as the lungs being deſtitute of muſcles and of fibres, are unfit to dilate themſelves; ſo it appears, that without the motion of the thorax they would not be filled with air. Since, as our learned friend Dr. *Highmore* hath well (and congruouſly to what ourſelves have purpoſely tried) obſerved, if a live dog have a great wound made in his cheſt, the lobes of the lungs on that ſide of the mediaſtinum will ſubſide and lie ſtill; the thorax and the lobes on the other ſide of the mediaſtinum continuing their former motion. And if ſuddenly at once the muſcles of the cheſt be on both ſides diſſected, upon the ingreſs of the air, the whole lungs, though untouched, will remain moveleſs, at leaſt, as to any expanſion or contraction of their ſubſtance.

To which we may add the obſervation of the diligent *Bartholinus*, who affirms the like of the diaphragm alſo; namely, that it being wounded, the lungs will fall together, and the reſpiration ceaſe, which my experiments oppoſe not, provided the wound be any thing great. And indeed the diaphragm ſeems the principal inſtrument of ordinary and gentle reſpiration, although to reſtrained reſpiration (if I may ſo call it) the intercoſtal muſcles, and perhaps ſome others, may be allowed eminently to concur. But the chief of the controverſies formerly pointed at, is not yet decided; namely, what it is that conveys the air into the lungs. For when, to counterbalance all that hath been alledged, thoſe that plead for the lungs demand what it is that ſhould bring the air into the lungs, if themſelves do not attract it, their antagoniſts diſagree about the reply. For when to this queſtion ſome of the beſt modern philoſophers anſwer, that by the dilatation of the cheſt the contiguous air is thruſt away, and that preſſing upon the next air to it, and ſo onwards, the propulſion is continued till the air be driven into the lungs, and ſo dilate them; when this (I ſay) as anſwered, it is objected even by *Bartholine* himſelf, as a convincing reply, that, according to this doctrine, a man could not fetch his breath from a great veſſel full of air, with a ſlender neck; becauſe, that when his mouth covers the orifice of the neck, the dilatation of his thorax could not propel the air in the veſſel into his lungs, by reaſon of its being ſeparated by the incloſing veſſel from the ambient air; and yet, ſay they, experience witneſſeth, that out of ſuch a veſſel a man may ſuck air. But of this difficulty our engine furniſheth us with an eaſy ſolution, ſince many of the former experiments have manifeſted, that in the caſe propoſed, there needs not be made any (though it is true that in ordinary reſpiration there is wont to be made ſome) propulſion of the air by the ſwelling thorax or abdomen into the lungs; ſince upon the bare dilatation of the thorax, the ſpring of that internal air, or halituous ſubſtance that is wont to poſſeſs as much of the cavity of the cheſt as the lungs fill not up, being much weakened, the external and contiguous air muſt neceſſarily preſs in at the open wind-pipe into the lungs, as finding there leſs reſiſtance than any where elſe about it.

AND hence (by the way) we may derive a new aſſiſtance to judge of that famous controverſy diſputed among Naturaliſts and Phyſicians, ever ſince *Galen*'s time, ſome maintaining that the cheſt, with the contained lungs, may be reſembled to a pair of bellows, which comes therefore to be filled becauſe it was dilated; and others pleading to have the compariſon made to a bladder, which is therefore dilated becauſe it is filled. For as to the thorax, it ſeems evident from what hath been lately ſaid, that it, like a pair of bellows, happens to be partly filled with air, but becauſe it was dilated: but as for the lungs themſelves, who want fibres to diſtend them, they may fitly enough be compared to a bladder; ſince they are dilated by being filled, namely, by that air which ruſheth into them upon the dilatation of the cheſt, in whoſe increaſed cavity it finds (as we freſhly noted) leſs reſiſtance to its ſpring than elſewhere. And this brings into my mind that ſtrange obſervation of *Nicolaus Fontanus*, a phyſician at *Amſterdam*, who teſtifieth, that in a boy of the ſame town, four years old, there was found, inſtead of lungs, a certain membranous bladder; which being filled with wind, and furniſhed with little veins, had its origination from the wind-pipe itſelf: which being ſuppoſed true, how well it will agree with moſt of the opinions touching reſpiration, I leave to be conſidered.

AND thus may the grand objection of *Bartholine*, and others, be anſwered: but I I leave to Anatomiſts to conſider what is to be ſaid to ſome obſervations that ſeem to contradict thoſe anatomical experiments already mentioned: ſuch was particularly that which I remember I have read in *Sennertus* (from the obſervation of his father-in-law *Schato*) of a melancholy ſtudent, who having ſtabbed himſelf, and pierced the diaphragm in the thinner or tendonous part (called by many the nervous circle) lived ſeven months after he had wounded himſelf, though after his death (preceded by violent vomitings) the wound (perchance dilated by thoſe ſtrainings) appeared ſo great, that the whole ſtomach was found to have got in by it into the left ſide of the thorax. And ſuch alſo was the accident that happened to a nobleman, whom I remember I have ſeen, and who is yet alive, in whoſe cheſt there hath, for theſe many years, remained a hole ſo great, that the motion of his heart may be perceived by it. Theſe (I ſay) and ſome other obſervations, I ſhall now forbear to inſiſt on, becauſe I hold it not unfit, before we come to conſider the uſe of reſpiration, that we acquaint your Lordſhip with an ingenious conjecture, that was made at the cauſe of the haſty death of the animals our engine killed; namely, that it was not the want of air that deſtoyed them, but the preſſure of the innate air in the cavity of the cheſt: as if the ſpring of the air being no longer counterbalanced by the ambient air, was thereby become ſo ſtrong, that it kept the thorax forcibly diſtended, and hindered its wonted contraction; and ſo compreſſed the lungs and their veſſels, as to obſtruct the circulation of the hlood. And this conjecture, as it is ſpecious enough, ſo I might have admitted it for true; but that I conſidered, that (not to mention that one, eſpecially of the animals killed in our engine, ſeemed manifeſtly for a pretty while, and not long before he died, to move his thorax, as if he exerciſed reſpiration) the diligent *Wallæus* relates, that he divers times obſerved, in the diſſection of live bodies, that the membrane that inveſts the lungs had pores in it as big as the larger ſort of peas; which agreeth with the obſervations of chirurgeons and phyſicians, *viz.* that matter collected in the thorax hath penetrated into the lungs, and been diſcharged by coughing. And I remember too, that moſt of the animals we killed in our engine were birds, of whoſe lungs *Harvey* ſomewhere informs us, that he obſerved them very manifeſtly to open at their extremities into the abdomen; and by ſuch perforations we may well ſuppoſe the paſſage free betwixt the external air, and that in the abdomen: but this conjecture may be farther conſidered. Beſides, to ſhow that the animals that died in our glaſſes, need not be ſuppoſed to have been killed by the want of air, we foreſee another argument, that we muſt deal ſo ingenuouſly with your Lordſhip, as not to conceal You very well know, that beſides the generality of the ſchools, there are many new philoſophers, who, though they diſſent from the old Peripatetics in other things, do, as they, deny the poſſibility of a vacuum; and hold, that thoſe ſpaces which are devoid of air, and other groſſer bodies, are all of them exactly repleniſhed with a certain ethereal matter, ſo thin and ſubtile, that it can freely permeate the pores of the compactedſt and cloſeſt bodies, and even of glaſs itſelf. Now ſome of thoſe Naturaliſts that are of this perſuaſion may object, that the animals that died in our receiver, did ſo, not ſo much for lack of air, as by reaſon that the air that was pumped out was neceſſarily ſucceeded by an etherial ſubſtance; which conſiſting of parts vehemently agitated, and ſo very ſmall, as without reſiſtance to paſs in and out through the very pores of glaſs; it may well be ſuppoſed, that a conſiderable quantity of this reſtleſs and ſubtile matter meeting together in the receiver, with the exceſſive heat of it, may be quickly able to deſtroy a little animal, or at leaſt make the air too intemperately hot to be fit for reſpiration.

BUT though this be a difficulty not ſo eaſily to be reſolved without the aſſiſtance of our engine, yet I ſuppoſe we have already anſwered the objection of our 38th and 39th experiments; which, though we made partly for other purpoſes, yet we premiſed them only to clear up the difficulty propoſed.

ANOTHER ſuſpicion we ſhould have entertained concerning the death of our animals; namely, that upon the ſudden removal of the wonted preſſure of the ambient air, the warm blood of thoſe animals was brought to an efferveſcence or ebullition; or at leaſt ſo vehemently expanded, as to diſturb the circulation of the blood, and to diſorder the whole œconomy of the body: this, I ſay, I ſhould have had ſome ſuſpicion of, but that animals of a hot conſtitution are not the ſole ones that cannot in our exhauſted engine exerciſe the function of life. But I muſt not now dwell upon matters of this nature, becauſe I think it high time to proceed to the conſideration of the principal ſubject of our engine, namely, the uſe of reſpiration; or rather, the uſe of the air in reſpiration. For whereas of the divers uſes of it mentioned by Anatomiſts, the moſt, ſuch as the production and modulation of the voice by the eliſion of the air, the larynx, *&c.* the expulſion of excrements by coughing, the conveying in of odours by inſpiration, and ſome others, rather convenient for the well being of an animal, than abſolutely neceſſary to his life; whereas (I ſay) the other uſes are ſuch as we have ſaid, the great *Hippocrates* himſelf gives this notable teſtimony to the uſe of the air, as to animals endowed with lungs: *Mortalibus* (ſaith he) *hic* (ſpiritus) *tum vitæ, tum morborum ægrotis cauſa eſt. Tantaque corporibus omnibus ſpiritûs ineſt neceſſitas, ut ſiquidem aliis omnibus & cibis & potionibus, quis abſtineat, duos tamen aut tres, vel plures dies poſſit vitam ducere: at ſi quis ſpiritus in corpus vias intercipiat, vel exiguâ diei parte, homini pereundum ſit; adeo neceſſarius eſt uſus ſpiritûs in corpore. Ad hæc quoque, quum omnibus aliis actionibus homines quieſcant, quod mutationibus innumeris vita ſit expoſita, ab hâc tamen ſolâ actione unquam deſiſtant animantia, quin aut ſpiritum adducant, aut reddant.*

BUT touching the account upon which the inſpiration and exſpiration of air (both which are comprehended in ἀναϖνοὴ, reſpiration) is ſo neceſſary to life, both Naturaliſts and Phyſicians do ſo diſagree, that it will be very difficult either to reconcile their opinions, or determine their controverſies.

FOR firſt, many there are, who think the chief, if not ſole uſe of reſpiration, to be the cooling and tempering of that heat in the heart and blood, which otherwiſe would be immoderate; and this opinion not only ſeems to be moſt received amongſt ſcholaſtic writers, but divers of the new philoſophers, Carteſians and others, admitted with ſome variation; teaching, that the air is neceſſary, by its coldneſs, to condenſe the blood that paſſeth out of the right ventricle of the heart into the lungs, that thereby it may contain ſuch a conſiſtence as is requiſite to make it fit fewel for the vital fire or flame, in the left ventricle of the heart. And this opinion ſeems favoured by this, that fiſhes, and other cold creatures, whoſe hearts have but one cavity, are alſo unprovided of lungs, and by ſome other conſiderations. But though it need not be denied, that the inſpired air may ſometimes be of uſe by refrigerating the heart, yet (againſt the opinion that makes this refrigeration the moſt genuine and conſtant uſe of the air) it may be objected, that divers cold creatures (ſome of which, as particularly frogs, live in the water) have yet need of reſpiration; which ſeems not likely to be needed for refrigeration by them that are deſtitute of any ſenſible heat, and beſides, live in the cold water: that even decrepid old men, whoſe natural heat is made very languid, and almoſt extinguiſhed by reaſon of age, have yet a neceſſity of frequent reſpiration: that a temperate air is fitteſt for the generality of breathing creatures; and as an air too hot, ſo alſo an air too cold may be inconvenient for them (eſpecially if they be troubled with an immoderate degree of the ſame quality which is predominant in the air:) that in ſome diſeaſes the natural heat is ſo weakened, that in caſe the uſe of reſpiration were too cool, it would be more hurtful than beneficial to breathe; and the ſuſpending of the reſpiration may ſupply the place of thoſe very hot medicines that are wont to be employed in ſuch diſtempers: that nature might much better have given the heart but a moderate heat, than ſuch an exceſſive one, as needs to be perpetually cooled, to keep it from growing deſtructive; which the gentle, and not the burning heat of an animal's heart, ſeems not intenſe enough ſo indiſpenſably to require. Theſe, and other objections might be oppoſed, and preſſed againſt the recited opinion; but we ſhall not inſiſt on them, but only add to them, that it appears not by our foregoing experiments (I mean the 38th and 39th) that in our exhauſted receiver, where yet animals die ſo ſuddenly for want of reſpiration, the ambient body is ſenſibly hotter than the common air.

OTHER learned men there are, who will have the very ſubſtance of the air to get in by the veſſels of the lungs, to the left ventricle of the heart, not only to temper its heat, but to provide for the generation of ſpirits. And theſe alledge for themſelves the authority of the ancients, among whom *Hippocrates* ſeems manifeſtly to favour their opinion; and both *Ariſtotle* and *Galen* do ſometimes (for methinks they ſpeak doubtfully enough) appear inclinable to it. But for aught ever I could ſee in diſſections, it is very difficult to make out, how the air is conveyed into the left ventricle of the heart, eſpecially the ſyſtole and diaſtole of the heart and lungs being very far from being ſynchronical: beſides, that the ſpirits ſeeming to be but the moſt ſubtile and unctuous particles of the blood, appear to be of a very differing nature from that of the lean and incombuſtible corpuſcles of air. Other objections againſt this opinion have been propoſed, and preſſed by that excellent Anatomiſt, and my induſtrious friend Dr. *Highmore*, to whom I ſhall therefore refer you.

ANOTHER opinion there is touching reſpiration, which makes the genuine uſe of it to be ventilation not of the heart, but of the blood, in its paſſage through the lungs; in which paſſage it is diſburthened of thoſe excrementitious ſteams proceeding, for the moſt part, from the ſuperfluous ſeroſities of the blood (we may add) and of the chyle too, which (by thoſe new conduits of late very happily detected by the famous *Pecquet*) hath been newly mixed with it in the heart. And this opinion is that of the induſtrious *Mœbius*, and is ſaid to have been that of that excellent philoſopher *Gaſſendus*; and hath been, in part, an opinion almoſt vulgar. But this hypotheſis may be explicated two ways: for firſt, the neceſſity of the air in reſpiration may be ſuppoſed to proceed from hence; that as a flame cannot long burn in a narrow and cloſe place, becauſe the fuliginous ſteams it inceſſantly throws out, cannot be long received into the ambient body; which, after a while, growing too full of them to admit any more, ſtifles the flame: ſo that the vital fire in the heart requires an ambient body of a yielding nature, to receive into it the ſuperfluous ſeroſities, and other recrements of the blood, whoſe ſeaſonable expulſion is requiſite to depurate the maſs of blood and make it fit, both to circulate and to maintain the vital heat reſiding in the heart. The other way of explicating the above-mentioned hypotheſis is, by ſuppoſing, that the air doth not only, as a receptacle, admit into its pores the excrementitious vapours of the blood, when they are expelled through the wind-pipe, but doth alſo convey them out of the lungs, in regard that the inſpired air reaching to all the ends of the *aſpera arteria*, doth there aſſociate itſelf with the exhalations of the circulating blood, and when it is exploded, carries them away with itſelf: as we ſee that winds ſpeedily dry up the ſurfaces of wet bodies, not to ſay any thing of what we formerly obſerved touching our liquor, whoſe fumes were ſtrangely elevated upon the ingreſs of the air.

Now, of theſe two ways of explicating the uſe of reſpiration, our engine affords us this objection againſt the firſt; that upon the exſuction of the air, the animal dies a great deal ſooner than if it were left in the veſſel; though by that exſuction, the ambient ſpace is left much more free to receive the ſteams that are either breathed out of the lungs of the animal, or diſcharged by inſenſible tranſpiration through the pores of his ſkin.

BUT if the hypotheſis propoſed be taken in the other ſenſe, it ſeems congruous enough to that grand obſervation, which partly the phænomena of our engine, and partly the relations of travellers, have ſuggeſted to us; namely, that there is a certain conſiſtence of air requiſite to reſpiration: ſo that if it be too thick, and already over-charged with vapours, it will be unfit to unite with, and carry off thoſe of the blood, as water will diſſolve and aſſociate to itſelf but a certain proportion of ſaline corpuſcles; and if it be too thin or rarefied, the number or ſize of the aërial particles is too ſmall to be able to carry off the halituous excrements of the blood, in ſuch plenty as is requiſite.

Now that air too much thickened (and as it were clogged) with ſteams, is unfit for reſpiration, may appear by what is wont to happen in the lead mines of *Devonſhire* and for aught I know, in thoſe too of other countries, though I have ſeen mines where no ſuch thing was complained of) for I have been informed by more than one credible perſon (and particularly by an ingenious man that hath often, for curioſity, digged in thoſe mines and been employed about them) that there often riſeth damps (as retaining the German word by which we call them) which doth ſo thicken the air, that, unleſs the workmen ſpeedily make ſigns to them that are above, they would (which alſo ſometimes happens) be preſently ſtifled for want of breath: and though their companions do make haſte to draw them up, yet frequently, by that time they come to the free air, they are, as it were, in a ſwoon, and are a good while before they come to themſelves again. And that this ſwooning ſeems not to proceed from any arſenical or poiſonous exhalation contained in the damp, as from its over-much condenſing the air, ſeems probable from hence; that the ſame damps oftentimes leiſurely extinguiſh the flames of their candles or lamps; and from hence alſo that it appears (by many relations of authentical authors) that in thoſe cellars where great ſtore of new wine is ſet to work, men have been ſuffocated by the too great plenty of the ſteams exhaling from the muſt, and too much thickning the air: as may be gathered from the cuſtom that is now uſed in ſome hot countries, where thoſe that have occaſion to go into ſuch cellars, carry with them a quantity of well-kindled coals, which they hold near their faces; whereby it comes to paſs, that the fire diſcuſſing the fumes, and rarefying the air, reduceth the ambient body to a conſiſtence fit for reſpiration.

We will add (by way of confirmation) the following experiment: in ſuch a ſmall receiver, as thoſe wherein we killed divers birds, we carefully cloſed up one, who, though for a quarter of an hour he ſeemed not much prejudiced by the cloſeneſs of his priſon, afterwards began firſt to pant very vehemently, and keep his bill very open, and then to appear very ſick; and laſt of all, after ſome long and violent ſtrainings, to caſt up ſome little matter out of his ſtomach; which he did ſeveral times, till growing ſo ſick that he ſtaggered and gaſped, as being juſt ready to die. We perceived, that within about three quarters of an hour from the time that he was put in, he had ſo thickened and tainted the air with the ſteams of his body, that it was become altogether unfit for the uſe of reſpiration: which he will not much wonder at, who hath taken notice in *Sanctorius* his *Statica Medicina*, how much that part of our aliments which goeth off by inſenſible tranſpiration, exceeds in weight all the viſible and groſſer excrements both ſolid and liquid.

That (on the other ſide) an air too much dilated is not ſerviceable for the ends of reſpiration, the haſty death of the animal we killed in our exhauſted receiver ſeems ſufficiently to manifeſt. And it may not irrationally be doubted, whether or no, if a man were raiſed to the very top of the atmoſphere, he would be able to live many minutes, and would not quickly die for want of ſuch air as we are wont to breathe here below. And that this conjecture may not appear extravagant, I ſhall, on this occaſion, ſubjoin a memorable relation that I have met with in the learned *Joſephus Acoſta*, who tells us, that when he himſelf paſſed the high mountains of *Peru* (which they call *Pariacaca*) to which, he ſays, that the *Alps* themſelves ſeemed to them but as ordinary houſes in regard of high towers, he and his companions were ſurprized with ſuch extreme pangs of ſtraining and vomiting (not without caſting up blood too) and with ſo violent a diſtemper, that he concludes he ſhould undoubtedly have died, but that this laſted not above three or four hours, before they came into a more convenient and natural temperature of air: to which our learned author adds an inference, which being the principal thing I deſigned in mentioning the narrative, I ſhall ſet down in his own words: *I therefore* (ſays he) *perſuade myſelf, that the element of the air is there ſo ſubtile and delicate, as it is not proportionable with the breathing of man, which requires a more groſs and temperate air; and I believe it is the cauſe that doth ſo much alter the ſtomach, and trouble all the diſpoſition.* Thus far our author, whoſe words I mention, that we may gueſs, by what happens ſomewhat near the confines of the atmoſphere (though probably far from the ſurface of it) what would happen beyond the atmoſphere. That, which ſome of thoſe that treat of the height of mountains, relate out of *Ariſtotle*, namely, that thoſe that aſcend to the top of the mountain *Olympus*, could not keep themſelves alive, without carrying with them wet ſpunges, by whoſe aſſiſtance they could reſpire in that air, otherwiſe too thin for reſpiration (that relation, I ſay, concerning this mountain) would much confirm what hath been newly recited out of *Acoſta*, if we had ſufficient reaſon to believe it. But I confeſs I am very diffident of the truth of it; partly, becauſe when I paſſed the *Alps*, I took notice of no notable change betwixt the conſiſtence of the air at the top and the bottom of the mountain; partly, becauſe in a punctual relation made by an Engliſh gentleman, of his aſcenſion to the top of the pike of *Tenariff* (which is by great odds higher than *Olympus*) I find no mention of any ſuch difficulty of breathing; and partly alſo, becauſe the ſame author tells us out of *Ariſtotle*, that upon the top of *Olympus* there is no motion of the air, inſomuch that letters traced upon the duſt, have been, after many years, found legible and not diſcompoſed; whereas that inquiſitive *Buſbequius* (who was ambaſſador from the German to the Turkiſh emperor) in one of his eloquent Epiſtles, tells us, upon his own knowledge, that Olympus *may be ſeen from* Conſtantinople, *blanched with perpetual ſnow*; which ſeems to argue, that the top of that, as well as of divers other tall hills, is not above that region of the air wherein meteors are formed. Though otherwiſe, in that memorable narrative which *David Frælichius* made of his aſcent to the top of the prodigiouſly high Hungarian mountain *Carpathus*, he tells us, *that when having paſſed through very thick clouds, he came to the very top of the hill, he found the air ſo calm and ſubtile, that not a hair of his head moved*; *whereas, in the lower ſtages of the mountain, he felt a vehement wind*. But this might well be caſual, as was his, having a clear air where he was, though there were clouds, not only beneath him, but above him.

Epiſt. 3.

Frælichius apud Varen. Geogra. Gener. lib. 1. *cap.* 19.

But, though what hath been hitherto diſcourſed, incline us to look upon the ventilation and depuration of the blood, as one of the principal and conſtant uſes of reſpiration; yet methinks it may be ſuſpected that the air doth ſomething more than barely help to carry off what is thrown out of the blood, in its paſſage through the lungs, from the right ventricle of the heart to the left. For we ſee, in phlegmatic conſtitutions and diſeaſes, that the blood will circulate tolerably well, notwithſtanding its being exceſſively ſerous: and in aſthmatical perſons, we often ſee, that though the lungs be very much ſtuffed with tough phlegm, yet the patient may live ſome months, if not ſome years. So that it ſeems ſcarce probable, that either the want of throwing out the ſuperfluous ſerum of the blood for a few moments, or the detaining it, during ſo ſhort a while, in the lungs, ſhould be able to kill a perfectly ſound and lively animal: I ſay, for a few moments, becauſe, that having divers times tried the experiment of killing birds in a ſmall receiver, we commonly found, that within half a minute of an hour, or thereabout, the bird would be ſurpriſed by mortal convulſions, and within about a minute more would be ſtark dead, beyond the recovery of the air, though never ſo haſtily let in. Which ſort of experiments ſeem ſo ſtrange, that we were obliged to make it ſeveral times, which gained it the advantage of having perſons of differing qualities, profeſſions and ſexes (as not only ladies and lords, but doctors and mathematicians) to witneſs it. And to ſatiſfy your Lordſhip, that it was not the narrowneſs of the veſſel, but the ſudden exſuction of the air that diſpatched theſe creatures ſo ſoon; we will add, that we once incloſed one of theſe birds in one of theſe ſmall receivers, where, for a while, he was ſo little ſenſible of his impriſonment, that he eat very cheerfully certain ſeeds that were conveyed in with him, and not only lived ten minutes, but had probably lived much longer, had not a great perſon, that was ſpectator of ſome of theſe experiments, reſcued him from the proſecution of the trial. Another bird being within about half a minute caſt into violent convulſions, and reduced into a ſprawling condition, upon the exſuction of the air, by the pity of ſome fair ladies, related to your Lordſhip, who made me haſtily let in ſome air at the ſtop-cock, the gaſping animal was preſently recovered, and in a condition to enjoy the benefit of the ladies compaſſion. And another time alſo, being reſolved not to be interrupted in our experiment, we did at night ſhut up a bird in one of our ſmall receivers, and obſerved that for a good while he ſo little felt the alteration of the air, that he fell aſleep with his head under his wing; and though he afterwards awaked ſick, yet he continued upon his legs between forty minutes and three quarters of an hour: after which, ſeeming ready to expire, we took him out, and ſoon found him able to make uſe of the liberty we gave him for a compenſation of his ſufferings.

IF to the foregoing inſtances of the ſudden deſtruction of animals, by the removal of the ambient air, we ſhould now annex ſome, that we think fitter to reſerve till anon; perhaps your Lordſhip would ſuſpect, with me, that there is ſome uſe of the air which we do not yet ſo well underſtand, that makes it ſo continually needful to the life of the animals. *Paracelſus*, indeed, tells us, *that as the ſtomach concocts meat, and makes part of it uſeful to the body, rejecting the other part; ſo the lungs conſume part of the air, and proſcribe the reſt.* So that, according to our Hermetic philoſopher (as his followers would have him ſtyled) it ſeems we may ſuppoſe, that there is in the air a little vital quinteſſence (if I may ſo call it) which ſerves to the refreſhment and reſtauration of our vital ſpirits, for which uſe the groſſer and incomparably greater part of the air being unſerviceable, it need not ſeem ſtrange, that an animal ſtands in need of almoſt inceſſantly drawing in freſh air. But though this opinion is not (as ſome of the ſame author) abſurd, yet beſides that it ſhould not be barely aſſerted, but explicated and proved; and beſides that ſome objections may be framed againſt it, out of what hath been already argued againſt the tranſmutation of air into vital ſpirits: beſides theſe things, it ſeems not probable, that the bare want of the generation of the wonted quantity of vital ſpirits, for leſs than one minute, ſhould, within that time, be able to kill a lively animal, without the help of any external violence at all.

BUT yet, on occaſion of this opinion of *Paracelſus*, perhaps it will not be impertinent if, before I proceed, I acquaint your Lordſhip with a conceit of that deſervedly famous Mechanician and Chymiſt, *Cornelius Drebell*, who, among other ſtrange things that he performed, is affirmed, by more than a few credible perſons, to have contrived, for the late learned King *James*, a veſſel to go under water; of which, trial was made in the *Thames*, with admired ſucceſs, the veſſel carrying twelve rowers, beſides paſſengers; one of which is yet alive, and related it to an excellent Mathematician that informed me of it. Now that for which I mention this ſtory is, that having had the curioſity and opportunity to make particular enquiries among the relations of *Drebell*, and eſpecially of an ingenious Phyſician that married his daughter, concerning the grounds upon which he conceived it feaſible to make men unaccuſtomed to continue ſo long under water without ſuffocation, or (as the lately mentioned perſon that went in the veſſel affirms) without inconvenience; I was anſwered, that *Drebell* conceived, that it is not the whole body of the air, but a certain quinteſſence (as Chymiſts ſpeak) or ſpirituous part of it, that makes it fit for reſpiration; which being ſpent, the remaining groſſer body, or carcaſe, if I may ſo call it, of the air, is unable to cheriſh the vital flame reſiding in the heart: ſo that, for aught I could gather, beſides the mechanical contrivance of his veſſel, he had a chymical liquor, which he accounted the chief ſecret of his ſubmarine navigation. For when, from time to time, he perceived that the finer and purer part of the air was conſumed, or overclogged by the reſpiration and ſteams of thoſe that went in his ſhip, he would, by unſtopping a veſſel full of this liquor, ſpeedily reſtore to the troubled air ſuch a proportion of vital parts, as would make it again, for a good while, fit for reſpiration,

ration, whether by diſſipating, or precipitating the groſſer exhalations, or by ſome other intelligible way, I muſt not now ſtay to examine; contenting myſelf to add, that having had the opportunity to do ſome ſervice to thoſe of his relations that were moſt intimate with him, and having made it my buſineſs to learn what this ſtrange liquor might be, they conſtantly affirmed that *Drebell* would never diſcloſe the liquor unto any, nor ſo much as tell the matter whereof he had made it, to above one perſon, who himſelf aſſured me what it was.

THIS account of *Drebell*'s performance I mention, not that I any farther aſſent to opinion than I have already intimated, but becauſe the man and the invention being extraordinary, I ſuppoſe your Lordſhip will not be diſpleaſed to know the utmoſt I could learn about it; eſpecially not having found it mentioned by any writer. Wherefore I have been ſometimes inclined to favourable thoughts of their opinion, who would have the air neceſſary to ventilate and cheriſh the vital flame, which they do ſuppoſe to be continually burning in the heart. For we ſee, that in our engine the flame of a lamp will laſt almoſt as little after the exſuction of the air, as the life of an animal: nay, I remember that though I deviſed a more promiſing way to make a fire laſt in our exhauſted receiver, yet it would not ſucceed. We took a hard body made in the form of a clove, but twice as long, and proportionably thick; this body being made of ſuch a compoſition, that if it be kindled at the upper end, it will moſt certainly burn away to the very bottom, much better than a match; we conveyed it divers times, kindled at the upper end, into one of our ſmall receivers, but ſtill found, that though preſently upon the exſuction of the air, it would leave ſmoking, and ſeem quite gone out, and again begin to ſmoke as ſoon as the air was let in upon it; yet if the air were kept out but four or five minutes, the fire would be totally and irrevocably extinguiſhed. To which we will add, that though we conveyed into a great receiver a ſmall lamp with rectified ſpirit of wine, that being ſo pure as not to ſmut the cotton wick, or ſo much as a piece of white paper held over it; yet we could not by divers trials make the flame laſt a couple of minutes, after the air was begun to be drawn out. But though our engine thus ſhews us a new kind of reſemblance betwixt fire and life, yet the opinion we have laſt mentioned is not free from difficulties. For, though in the hearts of many animals, blood be a warm liquor, and in ſome even a hot one; yet it is not eaſy to conceive either how the air (in ſubſtance) can get thither, or how, in caſe it could, it were able to increaſe the heat. Since, however, the air may increaſe the heat of a coal by blowing off the aſhes, and making the active corpuſcles pierce farther into the kindled body, and ſhatter it the more; yet we ſee hot liquors have their heat allayed, and not augmented, by having air blown on them. And whereas ſome eminent Naturaliſts think it not inconvenient to make the heat reſiding in the heart to be a true flame, provided they add, that it is ſuch a temperate and almoſt inſenſible fire, as the flame of ſpirit of wine, which will long burn upon fine white linen or paper without conſuming either; give me leave to wiſh that they had been more curious to make differing trials with that liquor. For (as we obſerve in another treatiſe) the reaſon why a linen cloth, dipped in common ſpirit of wine, is not burnt by the flame of it, is, becauſe the phlegm of the liquor defends the cloth: and the flame of the ſpirit of wine is ſo far from being too weak to burn a piece of paper, or of linen, that I have uſed it in lamps to diſtil liquors out of tall cucurbites, and found that the ſpirit burned away indeed much faſter than ſallad oil, but gave at leaſt as great a heat: nay, I have, for curioſity ſake, melted crude gold, and that readily enough, with the bare flame of pure ſpirit of wine.

But not to preſs this any farther, we will, on this occaſion, venture to ſubjoin an odd obſervation, which may perhaps invite to a farther inquiry into the opinion we have for diſcourſe ſake oppoſed. Our Engliſh *Democritus*, Dr. *Harvey*, propoſeth this difficult and noble problem to Anatomiſts, *Why a fœtus, even out of the womb, if involved in the ſecundines, may live a good while without reſpiration; but in caſe after having once began to breathe, its reſpiration be ſtopped, it will preſently die?* We are far from pretending to ſolve ſo hard a problem, but this we tried in relation to it: we took a bitch that was ſaid to be almoſt ready to whelp, and having cauſed her to be hanged, we preſently opened her abdomen, and found four puppies in her womb; one of theſe we took out, and having freed him from the teguments that involved him, and from the liquor he ſwam in, we obſerved that he quickly opened his mouth very wide, moved his tongue, and exerciſed reſpiration. Then we opened both his abdomen and his cheſt, and cut aſunder the diaphragm, notwithſtanding which, he ſeemed often to endeavour reſpiring, and moved in a notable manner, both the intercoſtal muſcles, part of the diaphragm, the mouth and the tongue. But that which we mention this puppy for, was this, that being deſirous to try whether the other young ones, that had not yet breathed at all, would long ſurvive this or no; we took them alſo out of the womb, and having opened them, found none of them ſo much alive, as to have any perceptible motion in his heart; whereas the heart of that puppy which had once enjoyed the benefit of reſpiration continued beating ſo long, that we ourſelves obſerved the auricle to beat, after five or ſix hours; and a ſervant that ſtaid up and watched it after we were gone to bed, affirmed, that he ſaw the pulſation continue about two hours longer. I ſhall leave it to others to make reflexions upon this obſervation, compared with Dr. *Harvey*'s problem.

It is much doubted, whether fiſhes breathe under water, and we ſhall not take upon us, as yet, to determine the queſtion either way, becauſe we have not yet been able to procure little fiſhes alive to make experiments upon. That ſuch as are not ſetaceous (for ſuch manifeſtly breathe) have not reſpiration, properly ſo called, ſuch as is exerciſed by four-footed beaſts, and birds, may be argued from their having no cavity in their hearts, and from their want of lungs, whence they are obſerved to be mute; unleſs we ſay, what is not altogether abſurd, that their gills ſeem ſomewhat analogous (as to their uſe) to lungs. But that, on the other ſide, air is neceſſary to the lives even of fiſhes, and that therefore it is probable they have ſome obſcure kind of reſpiration, ſeems manifeſt by two or three obſervations and experiments, mentioned by divers authors, who tell us, *that fiſhes ſoon die in ponds and glaſſes quite filled with water; if the one be ſo frozen over, and the other ſo cloſely ſtopped, that the fiſhes cannot enjoy the benefit of the air*; if we allow them to be true. But becauſe theſe relations are not wont to be delivered by writers upon their own knowledge; as I ſhall not reject them, ſo I dare not build upon them, till I have opportunity to examine them by experience. In the mean time, we will add, that our engine hath taught us two things that may illuſtrate the matter in hand: the one, that there is wont to lurk in water many little parcels of interſperſed air, whereof it ſeems not impoſſible that fiſhes may make ſome uſe, either by ſeparating it, when they ſtrain the matter thorow their gills, or by ſome other way; the other, what may be collected from the following experiment.

We took a large eel (being able to procure no other fiſh alive) and removing it out of the veſſel of water, wherein it was brought us, into our great receiver, we cauſed the air to be pumped out; and obſerved, that the eel, after ſome motion to and fro in the glaſs, ſeemed ſomewhat diſcompoſed; and that when we had proſecuted the exſuction of the air ſomewhat obſtinately, ſhe turned up her belly, as dying fiſhes are wont to do, and from thence forward lay altogether moveleſs, juſt as if ſhe were ſtark dead; and though I did not think her ſo, yet the continuing in that poſture, even after the cover of the receiver was taken off (whereby the air was let in) I ſhould have been of the opinion of the by-ſtanders, if the diffidence I am wont to exerciſe in trying experiments (eſpecially ſuch as are not uſual) had not invited me to take the fiſh out of the receiver; upon which ſhe ſhewed herſelf, by her vivid motions, as much alive as before.

BUT that is moſt ſtrange which we obſerved of a great, grey houſe-ſnail (as they call it) which being cloſed up in one of our ſmall receivers, did not only not fall down from the ſide of the glaſs, upon the drawing out of the air; (for that may be aſcribed to the tenacity of the liquor wherewith ſnails uſe to ſtick themſelves, even to the ſmootheſt bodies:) but was not ſo much as deprived of progreſſive motion by the receſs of the air; though, except this ſnail, we never put any living creature into our receiver, whom it did not kill, or at leaſt reduce to ſeem ready to die. But as we ſhall not here examine what intereſt the glutinous, and uneaſily diſſipable nature of the juices of ſnails may have on this event; ſo whether this eſcape of our eel be aſcribed to the particular and vivacious nature of this ſort of fiſhes, or to this, that the air is not indeed neceſſary to the life of fiſhes; or finally to this, that though theſe animals need ſome air, yet they need ſo little, that that which could not be drawn out of the receiver might (at leaſt for a while) ſuffice them, we will not now determine.

NOR are we at leiſure to examine that paradox of *Hippocrates*, which ſome learned phyſicians have of late revived, namely, that the fœtus reſpires in the womb. For, on the one ſide, it ſeems very difficult to conceive, how air ſhould traverſe the body of the mother, and the teguments of the child: and ſince nature hath, in new-born babes, contrived peculiar and temporary veſſels, that the blood may circulate thorough other paſſages, than it is wont to do in the ſame individuals when they come to have the free uſe of their lungs; it ſeems unlikely that infants in the womb do properly reſpire. But then ſince our experiments have manifeſted, that almoſt all kinds of liquors do, as well as water, abound with interſperſed corpuſcles of air, it ſeems not altogether abſurd to ſay, that when the fœtus is grown big, he may (eſpecially the upper part of the involving amnios being deſtitute of liquor, and filled only with an halituous ſubſtance) exerciſe ſome obſcure reſpiration; eſpecially, ſince it is not (as many wiſe men think it) a fable, that children have been heard to cry in the mother's womb; for though it happens exceeding rarely, yet ſometimes it hath been obſerved. And I know a young lady, whoſe friends, when ſhe was ſome years ſince with child, complained to me, that ſhe was ſeveral times much frighted with the cries of her infant, which, till I diſabuſed her, ſhe and her friends looked upon as portentous. And ſuch obſervations are the more credible, becauſe not only houſewives, but more judicious perſons, mention it as no very unfrequent thing, to hear the chick pip and cry in the egg, before the ſhell be broken. But this I mention but as a probable, not a cogent argument, till I can diſcover whether an eliſion of an halituous ſubſtance, though no true air, may not at the top of the larynx produce a ſound; ſince I find that the blade of a knife, held in ſeveral poſtures in the ſtream of vapours (or rarefied water) that iſſues out of an æolipile, will afford various and very audible ſounds.

I HAD thoughts of conveying into our receiver young ones, ripped out of the womb of their dams, with their involving coats entire, but could not procure them. And I have alſo had thoughts of trying whether it be not practicable to make a receiver, though not of glaſs, yet with little glaſs windows, ſo placed, that one may freely look into it, capacious enough to hold a man, who may obſerve ſeveral things, both touching reſpiration, and divers other matters; and who, in caſe of fainting, may, by giving a ſign of his weakneſs, be immediately relieved, by having air let in upon him. And it ſeems not impoſſible, but that by accuſtomance, ſome men may bring themſelves to ſupport the want of air a pretty while, ſince we ſee that divers will live, ſo much longer than other men, under water; that thoſe that dive for pearls in the *Weſt Indies* are ſaid to be able to ſtay a whole hour under water: and *Cardan* tells us of one *Colanus* a diver in *Sicily*, who was able to continue (if *Cardan* neither miſtake nor impoſe upon us) three or four times as long. Not to mind your Lordſhip, that you have yourſelf oftentimes ſeen in *England* a corpulent man, who is wont to deſcend to the bottom of the *Thames*, and bring out of deep holes, at the bottom of the banks, large fiſhes alive in his hands. And *Acoſta* tells us, he ſaw in *Peru* the like manner of fiſhing, but more difficult, practiſed by the *Indians*.

I MADE mention of ſome men, and of accuſtomance; becauſe there are but very few, who, though they uſe themſelves to it by degrees, are fit to ſupport, for many minutes, the want of air. Inſomuch that an ingenious man of my acquaintance, who is very famous for the uſeful ſkill of drawing goods, and even ordnance, out of ſunk ſhips, being aſked by me how long he was able to continue at the depth of 50 or 60 foot under water, without the uſe of reſpiration, confeſſed to me, that he cannot continue above two minutes of an hour, without reſorting to the air, which he carries down with him in a certain engine (whereof I can ſhow your Lordſhip a deſcription). Another thing I alſo learned of him by enquiry, that was not deſpicable: for aſking him, whether he found any uſe of chawing little ſponges, dipt in oil, in his mouth, when he was perfectly under water, and at a diſtance from his engine; he told me, that by the help of theſe ſponges he could much longer ſupport the want of his wonted reſpiration, than he was able to do without them. The true cauſe of which would perhaps, if diſcovered, teach us ſome thing pertinent to the problem touching the reſpiration of fiſhes.

BUT the neceſſity of air to the moſt part of animals unaccuſtomed to the want of it, may beſt be judged of, by the following experiments, which we tried in our engine, to diſcover, whether inſects themſelves have not, either reſpiration, or ſome other uſe of the air equivalent thereunto.

WE took then an humble bee, one of thoſe common flies that are called fleſh-flies, and one of thoſe hairy worms that reſemble caterpillars, and are wont to be called palmer worms: theſe three we conveyed into one of our ſmall receivers, and obſerved, to the great wonder of the beholders, that not only the bee, and the fly fell down, and lay with their bellies upwards, but the worm itſelf ſeemed to be ſuddenly ſtruck dead; all of them being reduced to lie without motion, or any other diſcernable ſign of life, within ſomewhat leſs (if we miſtake not) than one minute of an hour; and this, notwithſtanding the ſmallneſs of the animals in proportion to the capacity of the veſſels. Which circumſtance we the rather mention, becauſe we found that the veſſel was not free from leaks. And to ſatisfy the ſpectators, that it was the abſence of the air that cauſed this great and ſudden change; we had no ſooner re-admitted the air at the ſtop-cock, than all the three inſects began to ſhew ſigns of life, and by little and little to recover. But when we had again drawn out the air, their motions preſently ceaſed, and they fell down ſeemingly dead as before, continuing moveleſs, as long as, by continuing to pump, the veſſel was kept exhauſted. This invited us thankfully to reflect upon the wiſe goodneſs of the creator, who, by giving the air a ſpring, hath made it ſo very difficult, as men find it, to exclude a thing ſo neceſſary to animals:

and it gave us alſo occaſion to ſuſpect, that if inſects have no lungs, nor any part analogous thereunto, the ambient air affects them, and relieves them at the pores of their ſkin; it not being irrational to extend to theſe creatures that of *Hippocrates*, who ſaith, that a living body is throughout perſpirable; or, to uſe his expreſſion, εἰσπνοῦν δὲ ἐκπνοῦν, diſpoſed to admit and part with what is ſpirituous. Which may be ſomewhat illuſtrated by what we have elſewhere noted, that the moiſter parts of the air readily inſinuate themſelves into, and recede from the pores of the beards of wild oats, and thoſe of divers other wild plants; which almoſt continually wreath and unwreath themſelves according to, even, the light variations of the temperature of the ambient air.

This circumſtance of our experiment we particularly took notice of, that when at any time, upon the ingreſs of the air, the bee began to recover, the firſt ſign of life ſhe gave, was a vehement panting, which appeared near the tail; which we therefore mention, becauſe we have obſerved the like in bees drowned in water, when they firſt come to be revived by a convenient heat: as if the air were in the one caſe as proper to ſet the ſpirits and alimental juice moving, as heat is in the other; and this may, perchance, deſerve a farther conſideration.

We may add, that we ſcarce ever ſaw any thing that ſeemed ſo much as this experiment to manifeſt, that even living creatures (man always excepted) are a kind of curious engines, framed and contrived by nature (or rather the author of it) much more ſkilfully than our groſs tools and imperfect wits can reach to. For in our preſent inſtance we ſee animals, vivid and perfectly ſound, deprived immediately of motion, and any diſcernable ſigns of life, and reduced to a condition that differs ftom death, but in that it is not abſolutely irrecoverable. This (I ſay) we ſee performed without any, ſo much as the leaſt external violence offered to the engine; unleſs it be ſuch as is offered to a wind-mill, when the wind ceaſing to blow on the ſails, all the ſeveral parts remain moveleſs and uſeleſs, till a new breath put them into motion again.

And this was farther very notable in this experiment; that whereas it is known that bees and flies will not only walk, but fly for a great while, after their heads are off; and ſometimes one half of the body will, for divers hours, walk up and down, when it is ſevered from the other: yet, upon the exſuction of the air, not only the progreſſive motion of the whole body, but the very motions of the limbs do forthwith ceaſe; as if the preſence of the air were more neceſſary to theſe animals, than the preſence of their own heads.

But, it ſeems, that in theſe inſects, that fluid body (whether it be a juice or flame) wherein life chiefly reſides, is nothing near ſo eaſy diſſipable as in perfect animals. For whereas we have above recited, that the birds we conveyed into our ſmall receiver were within two minutes brought to be paſt recovery, we were unable (though by tiring him that pumped) to kill our inſects by the exſuction of the air: for though, as long as the pump was kept moving, they continued immovable, yet, when he deſiſted from pumping, the air that preſſed in at the unperceived leaks did, though ſlowly, reſtore them to the free exerciſe of the functions of life.

But, my Lord, I grow troubleſome, and therefore ſhall paſs on to other experiments: yet without deſpairing of your pardon for having entertained you ſo long about the uſe of reſpiration, becauſe it is a ſubject of that difficulty to be explained, and yet of that importance to human life, that I ſhall not regret the trouble my experiments have coſt me, if they be found in any degree ſerviceable to the purpoſes to which they were deſigned. And though I deſpair not but that hereafter our engine may furniſh us with divers phænomena uſeful to illuſtrate the doctrine of reſpiration;

yet having not, as yet, had the opportunity to make the other trials, of various kinds, that I judge requiſite for my information, I muſt confeſs to your Lordſhip, that in what I have hitherto ſaid, I pretend not ſo much to eſtabliſh or overthrow this or that hypotheſis, as to lay together divers of the particulars that occurred to me, in order to a future enquiry, I ſay, divers of the particulars, becauſe I could add many others, but that I want time, and fear that I ſhould need your Lordſhip's pardon for having been ſo prolix in writing; and that of Phyſicians (which perhaps I ſhall more eaſily obtain) for having invaded anatomy, a diſcipline which they challenge to themſelves, and indeed have been the almoſt ſole improvers of. Without denying then, that the inſpired and exſpired air may be ſometimes very uſeful, by condenſing and cooling the blood that paſſeth through the lungs; I hold that the depuration of the blood in that paſſage, is not only one of the ordinary, but one of the principal uſes of reſpiration. But I am apt alſo to ſuſpect, that the air doth ſomething elſe in reſpiration, which hath not yet been ſufficiently explained; and therefore, till I have examined the matter more deliberately, I ſhall not ſcruple to anſwer the queſtions that may be aſked me, touching the genuine uſe of reſpiration, in the excellent words employed by the acute St. *Auſtin*, to one who aſked him hard queſtions: *Mallem quidem* (ſays he) *eorum quæ à me quæſiviſti, habere ſcientiam quam ignorantiam: ſed quia id nondum potui, magis eligo cautam ignorantiam confiteri, quam falſam ſcientiam profiteri.*

EXPERIMENT XLII.

HAVING (partly upon the conſideration of ſome of the foregoing experiments, and partly upon grounds not now to be inſiſted on) entertained a ſuſpicion, that the action of corroſive liquors in the diſſolving of bodies may be conſiderably varied by the gravitation or preſſure of the incumbent air, and the removal of it; I thought fit to examine my conjecture by the following experiment:

I took whole pieces of red coral, and caſt them into as much ſpirit of vinegar, as ſufficed to ſwim above an inch over them: theſe ſubſtances I made choice of, that the ebullition upon the ſolution might not be too great, and that the operation might laſt the longer.

Having then put about half a ſcore ſprigs of coral, together with the menſtruum, into a ſomewhat long-necked phial, whereof they ſeemed ſcarce to fill a third part, we conveyed that phial into one of our ſmall pneumatical glaſſes, containing by gueſs about a quart of water; and having faſtened on the cover, after the accuſtomed manner, we ſuffered the liquor to remain unmoved a while, to obſerve whether the menſtruum would work upon the coral otherwiſe than before. But finding there did only ariſe, as formerly, a pretty number of ſmall bubbles, that made there no ſenſible froth upon the ſurface of the diſtilled vinegar, there were made two or three exſuctions of the air; upon which, there emerged from the coral ſuch a multitude of bubbles, as made the whole body of the menſtruum appear white; and ſoon after a froth, as big as all the reſt of the liquor, was ſeen to ſwim upon it; and the menſtruum plainly appeared to boil in the glaſs, like a ſeething-pot. And though, if we deſiſted but one minute from pumping, the decrement of the froth and ebullition, upon the getting in of a little air, at ſome leak or other, ſeemed to argue, that the removal of the preſſure of the external air was the cauſe, or, at leaſt, the occaſion of this effervеſcence; yet to evince this the more clearly, we turned the key, and let in the external air at the ſtop-cock: immediately upon whoſe entrance, the froth vaniſhed, and ſo many of the bubbles within the body of the liquor diſappeared, that it loſt its whiteneſs, and grew tranſparent again; the menſtruum alſo working as languidly upon the coral, as it did before they were put into the receiver. But when we had again drawn out the air, firſt the whiteneſs re-appeared, then the ebullition was renewed, which (the pumping being a while longer, and nimbly purſued) grew ſo great, that for three or four times one after another, whenever the air was let out of the receiver into the emptied cylinder, the frothy liquor overflowed the glaſs, and ran down by the ſides of it; and yet, upon the re-admitting of the excluded air, the boiling liquor grew immediately as calm and as tranſparent as at firſt: as if indeed the operation of it, upon the coral, had been facilitated by the exſuction of the incumbent air, which on its receſs left it more eaſy for the more active parts of the liquor to ſhew themſelves ſuch, than it was whilſt the wonted preſſure of the air continued unremoved. It may indeed be ſuſpected, that thoſe vaſt and numerous bubbles proceeded not from the action of the menſtruum upon the coral, but from the ſudden emerſion of thoſe many little parcels of air that (as we formerly obſerved) are wont to be diſperſed in liquors, without excluding ſpirit of vinegar: but having had this ſuſpicion before we tried the experiment, we conveyed our diſtilled vinegar alone into the receiver, and kept it a while there, to free it from its bubbles, (which were but very ſmall) before ever we put the coral into it. It may be ſuſpected likewiſe, that the agitation of the liquor, neceſſarily following upon the ſhaking of the glaſs, by pumping, might occaſion the recited ebullition; but upon trial made, there appeared not any notable change in the liquor, or its operation, though the containing veſſel were ſhaken, provided no air were ſucked out of it. The former experiment was another time tried in another ſmall receiver, with coral groſly powdered, and the ſucceſs was very much alike, ſcarce differing in any thing, but that the coral being reduced to ſmaller parts, upon the ebullition of the liquor, ſo many little lumps of coral would be carried and buoyed up by the emerging bubbles, as ſometimes to darken the phial, though the ſame coralline corpuſcles would be let fall again upon the letting in of the air.

SOMETHING alſo we tried in our great receiver, concerning the ſolution of metals in aqua fortis, and other corroſive liquors; but partly the ſtink, and partly ſome accidents, kept us from obſerving any thing peculiar and remarkable about thoſe ſolutions.

ONE thing we muſt not omit, that when the ſpirit of vinegar was boiling upon the coral, we took off the cover of the receiver, and took out the phial, but could not find, that, notwithſtanding ſo very late an ebullition, the liquor had any heat great enough to be at all ſenſible to our hands.

EXPERIMENT XLIII.

WE will now ſubjoin an experiment, which, if the former did not leſſen, the wonder of it would probably appear very ſtrange to your Lordſhip, as it did to the firſt ſpectators of it.

THE experiment was this: we cauſed water to be boiled a pretty while, that by the heat it might be freed from the latitant air, ſo often already taken notice of in common water. Then almoſt filling with it a glaſs phial, capable of containing near four ounces of that liquor; we conveyed it, whilſt the water was yet hot, into one of our ſmall receivers (big enough to hold about a pound of water) and having luted on the cover, we cauſed the air to be drawn out. Upon the two firſt exſuctions, there ſcarce appeared any change in the liquor, nor was there any notable alteration made by the third; but at the fourth, and afterwards, the water appeared to boil in the phial, as if it had ſtood over a very quick fire; for the bubbles were much greater than are uſually found upon the ebullition of very much more water than was contained in our phial. And this effervеſcence was ſo great in the upper part of the water, that the liquor boiling over the top of the neck, a pretty deal of it ran down into the receiver, and ſometimes continued (though more languidly) boiling there. Proſecuting this experiment, we obſerved, that ſometimes, after the firſt ebullition, we were reduced to make divers exſuctions of the air, before the liquor would be brought to boil again. But at other times, as often as the key was turned to let the air paſs from the receiver into the pump, the effervеſcence would begin afreſh, though the pump were plied for a pretty while together; which ſeemed to argue, that the boiling of the water proceeded from hence, that upon the withdrawing the preſſure of the incumbent air, either the fiery corpuſcles, or rather the vapours agitated by the heat in the water (which laſt, what we have formerly noted touching the rarefied water of an æolipile, manifeſt to be capable of an elaſtical power) were permitted to expand themſelves mightily in the evacuated receiver; and did, in their tumultuous dilatation, lift up (as the air is wont to do) the uppermoſt part of the water, and turning it into bubbles, made the water appear boiling. This conjecture was farther confirmed by theſe additional circumſtances: firſt, the effervеſcence was confined to the upper part of the water, the lower remaining quiet, unleſs the liquor were but ſhallow. Next, although ſometimes (as is already noted) the ebullition began again, after it had ceaſed a pretty while, which ſeemed to infer, that ſome concurrent cauſe (whatever that were) did a little modify the operation of heat; yet, when the water in the phial could by no pumping be brought to boil any more, the ſelf-ſame water, being in the very ſame phial warmed again and reconveyed into the pneumatical glaſs, was quickly brought to boil afreſh, and that vehemently and long enough; not to mention, that a new parcel, taken out of the ſame parcel of the boiled water, with the former, and put in cold, could by no pumping be brought to the leaſt ſhew of effervеſcence. Beſides, having tried the experiment in hot ſallad-oil, being a much more tenacious liquor, and requiring a ſtronger heat to make it boil, it could not be brought to an effervеſcence in our receiver; whereas the chymical oil of turpentine, being thinner and more volatile, was preſently made to boil up, till it reached four or five times the former height in the phial, in whoſe bottom it lay, and continued boiling till it was almoſt reduced to be but lukewarm. Wine alſo being a more thin and ſpirituous liquor than water, being conveyed in hot, inſtead of the oil, did, as I remember, at the very firſt exſuction begin to boil ſo vehemently, that, in a ſhort time that the pump was kept moving, four parts of five, by our gueſs, boiled over out of the phial, though it had a pretty long neck. On which occaſion we will add, that even the water itſelf, near one half, would ſometimes boil over into the receiver before it became lukewarm. And it was alſo remarkable, that once, when the air had been drawn out, the liquor did, upon a ſingle exſuction, boil ſo long with prodigiouſly vaſt bubbles, that the effervеſcence laſted almoſt as long as was requiſite for the rehearſing of a *pater noſter*. Now the experiment having been tried more than once, and found to ſucceed as to the main, ſeems much to countenance the conjecture we made at the beginning of this letter, where we told your Lordſhip, that perhaps the preſſure of the air might have an intereſt in more phænomena than men have hitherto thought. For as we had not then made this experiment, ſo now we have made it, it ſeems to teach, that the air, by its ſtronger or weaker preſſure, may very much modify (as the ſchoolmen ſpeak) divers of the operations of that vehement and tumultuous agitation of the ſmall parts of bodies, wherein the nature of heat ſeems chiefly, if not ſolely, to conſiſt. Inſomuch that if a heated body were conveyed above the atmoſphere, it is probable that the heat may have a differing operation, as to the power of diſſipating the parts of it, from what it hath here below.

To conclude; this experiment might have been farther proſecuted, but our want of leiſure makes us content ourſelves to add at preſent, that perhaps it would not be loſt labour if this were tried, not only with other liquors, but with variety of heated, and eſpecially ſoft or melted bodies: but in ſuch caſes the receiver ought to be ſhaped, as is moſt proper to preſerve the cement wherewith the cover muſt be faſtened on, from being melted by the heat of the included matter; the inconvenience to be hereby avoided, having befallen us in the uſe of a receiver too ſhallow, though otherwiſe capacious enough.

The CONCLUSION.

BEING come thus far, my dear Lord, not without thoughts of proceeding farther; the unwelcome importunity of my occaſions becomes ſo prevalent, that it quite hinders, for the preſent, my deſigned progreſs; and reduceth me, not only to reſerve for another opportunity that kind of experiments, which, at ſome diſtance, from the beginning of this letter, I called (as your Lordſhip may remember) experiments of the ſecond ſort; but to leave uneſſayed ſome of the firſt ſort, which I might try in the engine, as it now is, were it not that my avocations are grown ſo urgent, for my remove from the place where the engine was ſet up, that I am put to write your Lordſhip this excuſe: weary, and in an inn which I take in my way to my deareſt brother *Corke*; who being at length arrived in *England*, after I have for divers years been deprived of his company, and wiſhed for it as long; whatever my other occaſions may be, my firſt buſineſs muſt be to wait on him and your excellent mother; in whoſe grateful company I may hope to forget a while thoſe public calamities that diſtreſs this too unhappy nation; ſince that is endeared to me, both by their perſonal merit; by the near relation which nature gives me to him, affinity to her, and friendſhip to both; and alſo by their many favours, eſpecially that of my owing them my *Lord of Dungarvan.* But I ſuffer myſelf to be tranſported too far with theſe delightful thoughts; to return therefore to our engine. Though I find this letter is beyond my expectation ſwelled, not only into a book, but almoſt into a volume; yet the experiments already mentioned in it, are ſo far from compriſing all thoſe that may be tried by the help of our engine, that I have not yet been able to try all thoſe, which, preſently occurring to my thoughts, upon my firſt ſeeing the working of it, I cauſed to be ſet down in a catalogue within leſs than half an hour. But I doubt I have but too much cauſe to apprehend that the affairs and other things I complain of, have made it needful for me to apologize, as well for the things I have ſet down, as for thoſe I am neceſſitated to omit. For as partial as men uſe to be to the children of their own brains, as well as to thoſe of their loins, I muſt not deny that the foregoing trials are not altogether free from ſuch unaccurateneſſes, nor the recital of them from ſuch imperfections, as I myſelf can now diſcern, and could, perhaps, partly mend, if I had the leiſure to repeat the experiments, with the circumſtances that have ſince offered themſelves to my thoughts, as things that might have been worth obſervation or inquiry. But the truth is, that I was reduced to make theſe experiments, when my thoughts had things that more concerned me to employ them, and the ſame avocations made me ſet them down, for the moſt part, as ſoon as I had made them, and in the ſame order, and that ſo faſt, that I had not over frequently the opportunity to mind any more than the bare truth of what I ſet down; without allowing it any of thoſe advantages that method, ſtyle, and decent embelliſhments, are wont to confer on the compoſures they are employed to adorn.

BUT, my Lord, though to invite and encourage you and your learned friends at *Paris*, to make a farther uſe of this engine, than I have yet been able to do, I am thus free to acknowledge the imperfections of the foregoing letter; yet, if ſome intelligent perſons miſtake not, by what hath been done, ſuch as it is, there is a way opened, whereby ſagacious wits will be aſſiſted to make ſuch farther diſcoveries in ſome points of natural philoſophy, as are yet ſcarce dreamed of. And I am the more deſirous to engage you to that employment, becauſe I am apt to think, that if the making and writing of ſuch experiments ſhall coſt you as much trouble as they have me, you will be inclined to excuſe me; and if the diſcoveries give you as much pleaſure as they gave me, you will (perhaps) be invited to thank me. However, I think (my Lord) I may juſtly pretend, that the things I have ſet down have been faithfully recorded, though not elaborately written: and, I ſuppoſe, my former papers may have long ſince ſatisfied you, that though many deviſe experiments better than your ſervant, none perhaps hath related them more carefully and more truly: and particularly of theſe, ſometimes one, ſometimes another, hath been performed in the preſence of perſons, divers of them eminent for their writings, and all for their learning. Wherefore, having in the foregoing narratives made it my buſineſs to ennoble them with the chief requiſites of hiſtorical compoſures, candour and truth, I cannot deſpair that you will either excuſe their imperfections, or, at leaſt, forgive them: eſpecially, conſidering that this unpoliſhed letter is as well a production of your Lordſhip's commands and my obedience, as a teſtimony of my deſire to make others beholden to my Lord of *Dungarvan*, by the ſame way which I endeavour to expreſs myſelf,

Beconsfield, this 20th
of *December*, 1659.

His Lordſhip's

Moſt obedient Servant, and

Moſt affectionate Uncle,

ROBERT BOYLE.

A
DEFENCE
OF THE
DOCTRINE
TOUCHING THE
SPRING and WEIGHT of the AIR,

Propoſed by Mr. *R. BOYLE,* in his NEW PHYSICO-MECHANICAL EXPERIMENTS;

AGAINST THE OBJECTIONS OF

FRANCISCUS LINUS.

Wherewith the OBJECTOR'S FUNICULAR HYPOTHESIS is alſo examined.

The PUBLISHER *to the* READER.

FRIENDLY READER,

YOU may poſſibly in this volume have expected the Appendix which the author heretofore promiſed, and has intended ſhall contain ſome additional experiments to thoſe which were formerly publiſhed, and are here now reprinted in this ſecond edition. Theſe following anſwers to *Franciſcus Linus* and Mr. *Hobbs* are preſented in compenſation of the delay, and for your forbearance of that Appendix, which ere long you may expect in kind. For the author having hinted the promiſe, ſeems thereby to acknowledge the debt, and to be content to continue the obligation to ſee it performed. And theſe ought the rather to be his excuſe, becauſe the writing theſe anſwers, and publiſhing the *Sceptical Chymiſt*, and ſome other diſcourſes, have been the principal hindrances to that piece; which is really ſo near a readineſs, that part of it has lain at the preſs theſe ſix months: but yet it being not all perfected, the Stationer was loth to delay any longer the publication of theſe, for which he has been ſo frequently called upon. And they (though a Latin edition is intended) appear now the rather in Engliſh, that they may accompany the ſecond edition of the original experiments, which were printed firſt in that language in octavo;

and that, inſtead of the promiſed Appendix, they may complete the bulk of the quarto volume.

As for that part of this piece that concerns Mr. *Hobbs*, it might have been larger: but the informarion that the author had, that the learned Dr. *Wallis* was writing againſt ſome paſſages in Mr. *Hobbs* his Dialogues (as well that concerning the air, as the reſt) was the occaſion why his H. would make no animadverſion on ſome paſſages therein, and thought it not fit to enlarge upon others. And for the Errata of the preſs, I hope they will not be many: however, the author as to theſe is to be excuſed, who never (in regard to his eyes and impediments on other occaſions) gives himſelf the trouble of corrections and reviſes; neither could the publiſher much attend the preſs, it being printed in a diſtant place from his uſual abode. If, as I wiſh, you ſhall find this jealouſy of mine to have been cauſeleſs, you will have reaſon to give the piece that is ſo kindly offered, and leads you ſuch rare and untrodden paths in the beſt way of Natural Philoſophy, the fairer entertainment and acceptance. Farewel.

RO. SH.

The AUTHOR'S PREFACE *and* DECLARATION.

THEY that know how indiſpoſed I naturally am to contentiouſneſs, will, I preſume, wonder to ſee me publickly engaged in two controverſies at once. But that I am ſtill as averſe as ever from entering into diſputes that may handſomely be declined, the way wherein I have managed the following controverſies will, I hope, evince. And the inducements I now have to appear in public are ſuch, that it would be hard for me to reſiſt the being prevailed on by them.

For, in the firſt place, I was (by name, as it were) challenged by a perſon, who undertook to diſprove not one or two of my conjectures, but as much of the whole body of my treatiſe as concerned the ſpring of the air, which moſt of my explications ſuppoſe. And this being done by a learned man, who writes very confidently of the goodneſs of his hypotheſis and arguments, and his book being ſoon after followed by another written by Mr. *Hobbs*, a man of name in the world; there ſeemed to be ſome danger that ſo early an oppoſition might oppreſs the doctrine I had propoſed, before it was well underſtood and duly pondered. Wherefore I feared I might be wanting to the truth and myſelf, if I ſhould at ſuch a time be altogether ſilent; eſpecially ſince I might probably divert many who would otherwiſe be forward to appear againſt us, by letting them ſee how defenſible our doctrine is, even againſt ſuch adverſaries as thoſe I have replied to. And this courſe I the rather choſe, that in caſe I ſhould henceforward comply with thoſe who would have me forbear to write any further of theſe controverſies, it might not be preſently inferred from my ſilence, that a good cauſe cannot enable a pen no better than mine to defend it.

But I ſcarce doubt but that intelligent readers, eſpecially thoſe that are imbued with the principles of the Corpuſcularian philoſophy, will be much more apt to think that I had reaſon to write the following diſcourſes, than to think that I had any to make them ſo prolix: and eſpecially ingenious men, that are accuſtomed to admit nothing that either is not intelligible, or is precarious, will think divers of the objections I reply to, have needed no anſwers, or at leaſt no ſolemn ones. But to theſe I have four things to repreſent,

And firſt, that which not a little ſwells the bulk of the following treatiſes, is the inſerting thoſe paſſages of my adverſaries that I examine in their own words: which being a practice that I expect from any that ſhall think fit to animadvert upon any opinion or argument of mine; I thought it but equitable to do what I deſired to have done to me, though oftentimes I could not do it in a little room.

Next, I was the more willing to proſecute ſome of *Franciſcus Linus* his objections, becauſe the fear of being reduced to grant a vacuum has ſo prevailed with many eminent perſons bred up in the received philoſophy of the ſchools, that though they diſagree both with him and among themſelves about the particular manner of ſolving the phænomena of the Torricellian experiment; yet they agree in aſcribing them to ſome extremely rarefied ſubſtance, that fills up the ſpace deſerted by the quickſilver. So that this opinion, as to the main, being approved of by many eminent ſcholars, eſpecially of that moſt learned order of the Jeſuits (to whom perhaps its congruity to ſome articles of their religion chiefly recommends it) I was willing to pay them that reſpect, as not to diſſent from perſons, divers of whom for their eminence in mathematics and other learning I much eſteem, without ſhewing that I do it not but upon conſiderations that I think weighty.

Thirdly, though the examiner's hypotheſis have but few, and not very conſiderable arguments, to countenance it; yet his objections againſt our doctrine (the reply to which takes up the firſt part of the following treatiſe) are ſuch, as though they may be ſolidly anſwered by any that thoroughly underſtands our hypotheſis, yet they may chance to puzzle ſuch readers as do not, and theſe poſſibly will prove more than a few.

And laſtly, becauſe that ſometimes when the argument objected did not perhaps deſerve to be much inſiſted on, the argument treated of deſerved to be conſidered; I thought it not amiſs to make uſe, now and then, of ſome ſuch opportunities to illuſtrate the matter itſelf under conſideration: which I the rather did, for theſe two reaſons:

First, becauſe I find that, except by ſome able mathematicians and very few other contemplative men, the doctrine of the ſpring of the air, at leaſt as I have propoſed it, is not yet ſufficiently apprehended (and therefore needs to be inculcated). Inſomuch that through a great part of ſome late diſcourſes of men otherwiſe eminently learned (written againſt other Elateriſts, not me) there ſeems to run ſo great and clear a miſtake, perhaps for want of ſkill in the Hydroſtaticks, that I can ſcarce impute it to any thing but to their not thoroughly underſtanding the hypotheſis they would confute.

And next, becauſe I was willing to lay down in my anſwer to the objections I examined, the grounds of anſwering ſuch other arguments as may be built upon the ſame or the like principles. And perhaps I may truly enough ſay, that in the following treatiſe I have already, in effect, anſwered ſeveral diſcourſes, written ſome before and ſome ſince mine, by learned men, about the Torricellian and other new experiments relating to a vacuum, though I forebore to mention the names or words of the authors, becauſe I found not that my writings or experiments were as yet known to them. To theſe things I may add, that I thought the diſcourſes of *Linus* the fitter to be inſiſted on, becauſe he ſeems to have more diligently than ſome others (who yet venture to diſpute againſt it) inquired into our doctrine. And I ſhall not ſcruple to ſay thus much of an adverſary (and one to whom I gave no provocation to be ſo) that though I dare not ſpeak in general of thoſe that have written, either about the weight of the air, or elſe for or againſt a vacuum, becauſe (as I acknowledge in the firſt chapter following) I cannot yet procure the books of divers learned men, eſpecially of thoſe great perſonages, *Robervall*, *Balianus* and *Caſatus*; yet among the writers I have hitherto met with, who have recourſe to the Ariſtotelian rarefaction and condenſation in the controverſies under debate, ſcarce any ſeems to have contrived his hypotheſis better than our *Linus*. Not that I think his principle is either true or (at leaſt to ſuch as I) intelligible; but that the Funiculus he aſſumes being allowed him, he may, for a reaſon to be touched a little below, make out, though not all the phænomena of my experiments, yet many more of them than moſt other Pleniſts, that deny the ſpring of the air, can deduce from their hypotheſis if granted. And in regard that, whereas we aſcribe to the air a motion of reſtitution outwards, he attributes to it the like motion inwards, it cannot but happen that, though the principles cannot both be true, yet many of the phænomena may be explicable by which of them ſoever is granted: becauſe of this, I ſay, it is not ſo eaſy as many ingenious readers may be apt to think, to draw pertinent objections from experience againſt the adverſary I have to deal with. Which I repreſent, leſt, as ſome may think, I have employed more arguments than I needed, ſo others ſhould think I have omitted many, as indeed I have omitted ſome, that I might pertinently have employed.

But there is another ſort of perſons beſides thoſe I mentioned at the beginning of this preface, to whom I muſt addreſs the remaining part of it; namely, to thoſe who ſeem troubled, that I ſuffer myſelf to be diverted either by *Linus* or Mr. *Hobbs* from perfecting thoſe experimental treatiſes that are lying by me, almoſt promiſed by the learned publiſher of the Latin edition of my eſſays; and from proſecuting thoſe ways of inquiry into the nature of things, wherein they are pleaſed to think I may be more ſerviceable to real learning and the lovers of it. And I confeſs that theſe men's reaſons and perſuaſions have ſo far prevailed with me, that after what I have done in the two following treatiſes, to vindicate my writings from the objections made againſt them by two learned men of very differing hypotheſes, and thereby to ſhew in ſome meaſure, that I am not altogether unacquainted with the way of defending oppoſed truths, I have laid aſide the thoughts of writing any more diſtinct or entire polemical treatiſes about the ſubjects already diſputed of. And to this I am invited by ſeveral other reaſons (beſides what I have newly intimated).

About the hiſtory of flame, of heat, of colours, of the origin of qualities and forms, &c.

For firſt, as I elſewhere declare, it was not my chief deſign to eſtabliſh theories and principles, but to deviſe experiments, and to enrich the hiſtory of nature with obſervations faithfully made and delivered; that by theſe and the like contributions made by others, men may in time be furniſhed with a ſufficient ſtock of experiments, to ground hypotheſes and theories on. And though, in my *Phyſico-Mechanical Epiſtle* and my *Specimens* I have ventured ſome conjectures alſo as the cauſes of the phænomena I relate, leſt the diſcourſe ſhould appear to inquiſitive readers too jejune; yet (as I formerly ſaid) I propoſed my thoughts but as conjectures deſigned (though not only, yet chiefly) to excite the curioſity of the ingenious, and afford ſome hints and aſſiſtance to the diſquiſitions of the ſpeculative. And accordingly I have not forborn to mention divers things, which judicious readers may eaſily perceive I foreſaw, that many would think unfavourable to the opinions I inclined to. So that for me to leave experimental for controverſial ſtudies, were a courſe unſuitable to the principal ſcope of my writings.

Next, though I have adventured to improve the doctrine of the ſpring and weight of the air, by ſome ſupplements where I found it deficient, and to recommend it by ſome new illuſtrations and arguments deduced from my experiments; yet the hypotheſes themſelves (for the main) being the opinions alſo of far learneder men than I, it might be thought injurious both to them and to our common cauſe, if I ſhould needleſly go about to hinder them from the honour of vindicating the truths we agree in; eſpecially ſome of them being excellent Mathematicians, and others eminent

Naturaliſts, whoſe concern to maintain the hypotheſes againſt objections, if any ſhall ariſe, is equal to mine, and whoſe leiſure and abilities far exceed thoſe of a perſon who both is ſickly, and hath other employments enough, and who (if he were far better ſkilled in geometry than he pretends to be) hath ſuch a weakneſs in his eyes, as makes him both unwilling and unfit to engage in any ſtudy where the converſing with mathematical ſchemes is neceſſary.

THIRDLY, nor do I ſee much cauſe to doubt that the things I have delivered will, notwithſtanding my ſilence, be left undefended: the forwardneſs I have already obſerved in divers Virtuoſi to vindicate thoſe writings, which they are pleaſed to ſay have convinced them, and to ſave me the labour of penning the following treatiſes, ſcarce permitting me ſuch an apprehenſion; eſpecially ſince there are ſome things that will much facilitate their taſk, if not keep men from putting them upon it. For though Mr. *Hobbs* and *Linus* have examined my writings upon principles wherein they differ as much from each other as from me; yet neither have they ſeen cauſe to deny any thing that I deliver as experiment, nor have their objections been conſiderable, whether as to number or to weight, againſt the applications I have made of my principles to ſolve the phænomena. So that uſually, without objecting any incongruity to my particular explications, they are fain to fall upon the hypotheſes themſelves: in whoſe defence I think I may with the more reaſon expect to be ſeconded, becauſe not only I have endeavoured, as I formerly noted, to lay the grounds of anſwering ſuch objections as I foreſaw might ariſe; but I have alſo, to prevent or eaſe their labour, written the two firſt parts of my defence againſt *Linus*, without being obliged to do ſo for the vindicating of my explications, which are particularly maintained in the third part.

I KNOW not whether I may venture to add on this occaſion, that thoſe who have taken notice of the uſefulneſs of experiments to true philoſophy, and have obſerved that neverthleſs the difficulty, trouble and charge of making them is ſuch, that even in this learned age of ours, there are very few *Bacons* or *Merſennuſe's* to be met with, and thoſe who have either made themſelves, or at leaſt ſeen others make experiments, even ſuch as thoſe I have publiſhed, with the care I am wont to think myſelf obliged to employ on ſuch occaſions, will perhaps not only believe that they coſt me far more time and pains than they that have not made nor ſeen ſuch trials, are apt to imagine; but will poſſibly think it enough for a perſon, that is not by profeſſion a ſcholar, to make them carefully and ſet them down faithfully, and will allow him to let others vindicate the truths he may have the good fortune to diſcover; eſpecially, when there are ſo many fitter for it than he, who have (as well as his adverſaries) more leiſure to write diſputations than opportunity to proſecute experiments; the latter of which, to be performed as it ought to be, doth, in many caſes, beſides ſome dexterity ſcarce to be gained but by practice, require ſometimes more diligence, and oftentimes too more coſt than moſt are willing, or than many are able to beſtow upon them.

To be ſhort, though if any thing very worthy to be taken notice of by me be ſuggeſted againſt any of my chief opinions or explications, I may either take occaſion to ſay ſomewhat to it elſewhere, or at leaſt have an opportunity to conſider it in a review, wherein I may alter, mend, ſupply, vindicate or retract divers paſſages of my other writings: yet I would not have it expected that I ſhould exchange a book with every one that is at leiſure to write one againſt a vacuum, or about the air. Which declaration I make, not that I think it will, or ought to hinder any man from making uſe of his liberty to expreſs a diſſent, if he ſees cauſe; but for theſe two reaſons:

The one, that my ſilence might not injure either the truth or myſelf, by tempting men to think, that whatever I do not anſwer, I cannot; but might give unbiaſſed and judicious readers a caution to allow as little of advantage to the writings of my adverſaries upon the account of their being unanſwered by me, as if I were no longer in the world. And the other, that I may not hinder thoſe who would reply to ſuch adverſaries by leaving them an apprehenſion that either I may prevent them, or they me. To conclude, I ſee no cauſe to deſpair, that whether or no my writings be protected, the truths they hold forth will, in time, in ſpite of oppoſition, eſtabliſh themſelves in the minds of men, as the circulation of the blood, and other, formerly, much conteſted truths, have already done. My humour has naturally made me too careful not to offend thoſe I diſſent from, to make it neceſſary for any man to be my adverſary upon the account of perſonal injuries or provocations. And as for any whom either judgment or envy may invite to contend, that the things I have communicated to the world deſerved not ſo much applauſe as they have had the luck to be entertained with; that ſhall make no quarrel betwixt us: for perhaps I am myſelf as much of that mind as he; and however I ſhall not ſcruple to profeſs myſelf one of thoſe who is more deſirous to ſpend his time uſefully, than to have the glory of leaving nothing that was ever written againſt him unanſwered; and who is more ſolicitous to purſue the ways of diſcovering truth, than to have it thought that he never was ſo much ſubject to human frailties as to miſs it.

A Defence of Mr. ROBERT BOYLE's Explications of his *Phyſico-Mechanical Experiments*,

AGAINST

FRANCISCUS LINUS.

PART I.

Wherein the Adverſary's Objections againſt the Elateriſts are examined.

CHAP. I.

A NEWLY-publiſhed treatiſe, *De corporum inſeparabilitate*, being brought to my hands, I find ſeveral chapters of it employed to oppoſe the explications I ventured to give of ſome of my new experiments, touching the ſpring of the air. Wherefore, though I am very little delighted to be engaged in controverſies, and though I be not at preſent without employments enough (of a private, and of a public nature) to make it unſeaſonable for me, to be by a work of this ſort diverted from them; yet for the reaſons ſpecified in the preface, I hold it not amiſs to examine briefly what is objected againſt the thing I have delivered: and the rather, partly, becauſe the learned author, whoever he be (for it is the title-page of his book that firſt acquainted me with the name of *Franciſcus Linus)* having forborn provoking language in his objections, allows me in anſwering them to comply with my inclinations and cuſtom of exerciſing civility, even where I moſt diſſent in point of judgment. Beſides, the author himſelf has ſomewhat facilitated my reply to him, by directing me in the ninth page to ſome books and paſſages that I had not, when I publiſhed my epiſtle, either ſeen or taken notice of. As indeed there are beſides ſome of theſe ſeveral other diſcourſes that treat of the Torricellian experiments, which, though by the names of their authors I gueſs to be learnedly written, I have not to this day had opportunity to peruſe; my ſtay in the remoter part of *Ireland* (whither philoſophical books were not, in that time of public confuſion, brought) having kept me from hearing of divers of them, till they were all bought up. Which I here mention, to excuſe myſelf if I have not taken notice of ſome things or paſſages to be met with in theſe writings, which their learned authors or inquiſitive readers might juſtly perhaps expect I ſhould take ſome notice of, in caſe thoſe writings had fallen into my hands. But to digreſs no further.

It is true indeed, and it ſomewhat troubles me that it is ſo, that I can ſcarce promiſe myſelf to make my adverſary a proſelyte, ſince he without ſcruple aſſumes thoſe very things as principles, that to me ſeem almoſt as great inconviences as I would deſire to ſhew any opinion I diſlike, to be liable unto. But ſince whatever operation the following diſcourſe may have upon the perſon that occaſioned it, I hope it may bring ſome ſatisfaction to thoſe philoſophers who can as little as I underſtand the Ariſtotelian rarefaction, and who will as well as I be backward to admit what they cannot underſtand; it ſhall ſuffice me to defend the truths I have delivered, if I cannot be ſo happy as to convince my acute adverſary of them; and I ſhall not believe my labour loſt, if this diſcourſe can contribute to the eſtabliſhment of ſome notions in philoſophy that I think not inconſiderable in the minds of thoſe whoſe clear principles make me the moſt reſpect their judgments, and for whoſe ſakes I principally write.

Now though I be not in ſtrictneſs obliged to defend any more than ſuch of my own explications as the examiner has thought fit to queſtion, and thoſe particulars which I have added by way of improvement to the two hypotheſes of the ſpring and weight of the air; yet that I may the more effectually proſecute what I lately intimated I aim at in this writing, and may as well illuſtrate my doctrine as defend it, I ſhall divide the enſuing treatiſe into three parts; whereof the firſt is deſigned to anſwer my adverſary's objections againſt our principles; the ſecond ſhall examine the funicular hypotheſis he would ſubſtitute in their ſtead; and the third ſhall contain particular replies to what he alleges againſt ſome of my particular explications.

CHAP II.

ALTHOUGH our author confeſſes in his ſecond chapter, that the air has a ſpring as well as a weight, yet he reſolutely denies that ſpring to be near great enough to perform thoſe things which his adverſaries (whom for brevity ſake we will venture to call Elateriſts) aſcribe to it. And his whole fourth chapter, as the title declares, is employed to prove that the ſpring of the air is unable in a cloſe place to keep the mercury ſuſpended in the Torricellian experiment. The proof of this aſſertion he ſays is eaſy: but alleges two or three arguments for it, which I think will be more eaſily anſwered than his aſſertion evinced.

In the firſt he ſays, that thoſe experiments concerning the adheſion of one's finger, &c. which he had mentioned in the foregoing chapter, *eodem modo ſe habent in loco clauſo ac in aperto*. But the anſwering of this we ſhall ſuſpend till anon; partly, becauſe it may then be more conveniently examined; and partly, becauſe our author ſeems not to build much upon it, his chief argument being that which he propoſes in theſe words: *Cum tota vis hujus Elaterii pendeat à refutato jam aëris æquipondio cum* Page 20. *digitis 29½ argenti vivi, ita ut nec plus, nec minus faciat hoc elaterium in loco occluſo, quam fit per illud æquipondium in loco aperto; manifeſtum eſt, cum jam oſtenſum ſit fictitium planè eſſe hujuſmodi æquipondium, fictitium quoque eſſe tale elaterium.* ‘Being the whole power of the ſpring of the air depends upon the æquilibrium of its weight with twenty nine inches and a half of quickſilver, ſo that this ſpring doth neither more nor leſs in a ſhut place, than is done by that æquilibrium in an open place; it is manifeſt, ſeeing we have ſhewed the æquilibrium to be plainly fictitious and imaginary, that the ſpring aſcribed to the air is ſo likewiſe.’ Wherefore ſince all the validity of his objection againſt the ſpring of the air depends upon his former chapter, wherein he thinks he has diſproved the weight of the air; it will behove us to look back into the former chapter, and examine the four arguments which he there propoſes. But I muſt crave leave to vary from his method, and conſider the third in the firſt place, becauſe the removal of that objection will facilitate and ſhorten the anſwer to the reſt. His third argument therefore is thus ſet down: *Nam ſi tubus* Page 16. *viginti tantum digitorum (quo uſi ſumus in primo argumento) non totus impleatur argento, ut prius, ſed ſpacium aliquod inter digitum ſuperiorem & argentum relinquatur in quo ſit ſolus aër; videbimus ſubtracto inferiore digito ſuperiorem non ſolum deorſum trahi, ut prius, ſed etiam argentum jam deſcendere, idque notabiliter, quantum nimirum extendi poteſt exigua illa aëris particula à tali pondere deſcendente. Unde ſi loco illius aëris ponatur aqua, aliuſve liquor qui non tam facilè extenditur, deſcenſus nullus erit.*

Hinc, inquam, contra hanc ſententiam formatur argumentum: nam ſi externus ille aër nequeat vel hos viginti digitos argenti à lapſu ſuſtentare, uti jam vidimus, quomodo quæſo ſuſtentabit 29½? Certè hæc nullatenus reconciliari poſſunt.

‘For if a tube but twenty inches long (ſuch as we uſed in our firſt argument) be not quite filled with quickſilver, as before, but a little ſpace be left betwixt the mercury and the finger on the top of the tube, in which air only may abide; we ſhall find that the finger below being removed, the finger on the top will not only be drawn downwards, as before, but the quickſilver ſhall deſcend alſo, and that notably, *viz.* as much as ſo ſmall a parcel of air can be extended by ſuch a deſcending weight. So that if, inſtead of air, water, or any other liquor which is not ſo eaſily extended, be put in its place, there will be no deſcent at all.

‘Hence, I ſay, againſt this opinion an argument is framed: for if the external air cannot keep up thoſe twenty inches of quickſilver from deſcending, as we have proved; how ſhall it keep up twenty-nine inches and an half? Aſſuredly theſe can no way be reconciled.’

But to this argument, which he thinks ſo irreconcilable with his adverſary's hypotheſis, he has himſelf furniſhed them with an anſwer in theſe words; *Dices forte ideo argentum in hoc caſu deſcendere, quia deorſum truditur ab aëre illo ſeſe per ſuum Elaterium dilatante.* ‘You will perchance ſay, that the quickſilver therefore doth in the alleged caſe deſcend, becauſe it is thruſt down by that parcel of air which dilates itſelf by its own ſpring.’ Which anſwer I think ſufficient for the objection, notwithſtanding the two exceptions he takes at it.

For firſt, whereas he ſays, that *ſic deberet digitus potius à tubo repelli, quam eidem affigi, cum non minus ſurſum quam deorſum fiat hujuſmodi dilatatio:* ‘So ſhould the finger be rather thruſt from the top of the tube, than thereby faſtened to it; becauſe this dilatation muſt be made as well upwards as downwards:’ He conſiders not, that though the endeavour of the included air to expand itſelf be at firſt every way alike, yet the expanſion itſelf in our caſe muſt neceſſarily be made downward, and not upward; becauſe the finger that ſtops the tube being expoſed on the upper parts and the ſides to the external air, has the whole weight and preſſure of the atmoſphere upon it; and conſequently cannot be thruſt away but by a force capable to ſurmount that preſſure: whereas on the lower ſide of the included air there is the weight of the whole mercurial cylinder to aſſiſt the ſpring of the air to ſurmount the weight of the atmoſphere, that gravitates upon the reſtagnant mercury. So that the air included and endeavouring to expand itſelf, finding no aſſiſtance to expand itſelf upward, and a conſiderable one to expand itſelf downward, it is very natural that it ſhould expand itſelf that way whence it finds leſs reſiſtance. As accordingly it will happen, till the ſpring of the air be ſo far debilitated by its expanſion, that its preſſure, together with the weight of the mercury that remains ſuſpended, will but counterbalance, not overcome, the preſſure of the outward air upon the reſtagnant mercury. And this explication may be confirmed by this trial that I have purpoſely made; namely, that if inſtead of quickſilver you employ water, and leave as before in the tube an inch of air, and then inverting it, open it under water, you will perceive the included inch of air not to dilate itſelf any thing near (for I need not here define the proportion) half ſo far as it did when the tube was almoſt filled with mercury; becauſe the weight of ſo ſhort a cylinder of water does but equal that of between an inch and an inch and an half only of quickſilver; and conſequently the inward air is far leſs aſſiſted to dilate itſelf, and ſurmount the preſſure of the outward air by the cylinder of water, than by that of mercury. And as for what our author ſays, that if inſtead of air, water or ſome other liquor be left at the top of the tube, the quickſilver will not deſcend; the Elateriſts can readily ſolve that phænomenon, by ſaying that water has either no ſpring at all, or but an exceeding weak one; and ſo ſcarce preſſes but by its weight, which in ſo ſhort a cylinder is inconſiderable. Now the ſame ſolution we have given of our examiner's objection, gives us alſo an account why the finger is ſo ſtrongly faſtened to the upper part of the tube it ſtops; for the included air being ſo far dilated, that an inch, for example, left at firſt in the upper part of the tube, reaches twice or thrice as far as it did before the deſcent of the quickſilver, its ſpring muſt be proportionably weakened. And conſequently that part of the finger that is within the tube will have much leſs preſſure againſt it from the dilated air within, than the upper part of the ſame finger will have from the unrarefied air without. By which means the pulp of the finger will be thruſt in (which our author is pleaſed to call ſucked in) as we ſhall ere long have occaſion to declare in our anſwer to his ſecond argument.

And having ſaid thus much to our author's firſt exception againſt the ſolution he foreſaw we would give of his third argument; we have not much to ſay at preſent to Page 17. this ſecond. For whereas he ſays, *Concipi non poſſe quomodo aër ille ſic ſe dilatet, argentumque deorſum trudat, niſi occupando majorem locum: quod tamen hi authores quam maxime refugiunt, aſſerentes rarefactionem non aliter fieri, quam per corpuſcula aut vacuitates.* ‘It cannot be conceived how that air ſhould dilate itſelf, or thruſt down the mercury, unleſs by taking up a greater place; which thing theſe authors are much againſt, aſſerting that rarefaction can be made no other ways than by corpuſcles or vacuities.’ I wiſh he had more clearly expreſſed himſelf, ſince as his words are couched I cannot gueſs what he means, and much leſs eaſily diſcern how they make an argument againſt his adverſaries. For, ſure he thinks them not ſo abſurd, as to imagine that the air can dilate itſelf, and thruſt down the mercury, without in ſome ſenſe taking up more room than it did before: for the very word dilatation, and the effect they aſcribe to the included air, clearly imply as much; ſo that I ſee not why he ſhould ſay that they are ſo averſe from granting the air to take up more place than before, eſpecially ſince he takes notice in the former chapter, that we compare the expanſion of the air to that of compreſſed wool; and ſince he here alſo annexes, that we explicate rarefaction either by corpuſcles or vacuities. But this latter clauſe makes me ſuſpect his meaning to be, that the Elateriſts do not admit that the ſame air may adequately fill more of place at one time than at another; which I believe to be as true as that the ſelf-ſame lock of compreſſed wool has no more hairs in it, nor does adequately fill more place with them, when it is permitted to expand itſelf, than whilſt it remained compreſſed. But againſt this way of rarefaction our author here has not any objection, unleſs it be intimated in theſe words: *Concipi non poteſt:* which if it be, I ſhall need only to mind him in this place, that whereas many of the chiefeſt philoſophers, both of ancient and our own times, have profeſſed they thought not the Ariſtotelian way of rarefaction conceivable; and he acknowledges (as we ſhall ſee anon) that it is not clear; what the ableſt of his party (the modern Pleniſts) are wont to object againſt the way of rarefaction he diſlikes, is, that it is not true, not that it is not intelligible.

CHAP. III.

OUR author's ſecond objection (for ſo I reckon it) is thus propoſed by him: *Si ſumatur tubus utrinque apertus ſed longior, puta digitorum* 40. *argentoque impleatur,* Page 15. *eique digitus ſupernè applicatur ut prius, videbimus ſubtracto inferiore digito, argentum quidem deſcendere uſque ad conſuetam ſuam ſtationem; digitum autem ſuperiorem fortiter intra tubum trahi, eique firmiſſime, ut prius, adhærere. Ex quo rurſum evidenter concluditer, argentum, in ſua ſtatione conſtitutum, non ibidem ſuſtentari ab externo aëre, ſed à funiculo quodam interno ſuſpendi, cujus ſuperior extremitas, digito affixa, eum ſic intra tubum trahit, eique affigit.* ‘If you take a tube open at both ends of a good length, ſuppoſe forty inches long, and fill it with mercury, and place your finger on the top as before, taking away your lower finger, you will find the mercury to deſcend even to its wonted ſtation, and your finger on the top to be ſtrongly drawn within the tube, and to ſtick cloſe unto it. Whence again it is evidently concluded that the mercury placed in its own ſtation is not there upheld by the external air, but ſuſpended by a certain internal cord, whoſe upper end being faſtened to the finger, draws and faſtens it after this manner into the tube.’ But this argument being much of the ſame nature with that drawn from his third experiment, the anſwer made to that and to his firſt may be eaſily applied, and will be ſufficient for this alſo; eſpecially becauſe in our preſent caſe there is leſs preſſure againſt the pulp of the finger in the inſide of the tube than in the third experiment (where ſome air is included, though much expanded and weakened;) the preſſure of the atmoſphere being in the preſent caſe kept off from it by the ſubjacent mercury: whereas there is nothing of that preſſure abated againſt the other parts of the finger that kept it off from the deſerted cavity of the tube, ſave only that from the pulp that is contiguous to the tube, there may be ſomewhat of that preſſure taken off by the weight of the glaſs itſelf. But as for that part of the finger which immediately covers the hole, whether or no there be any ſpring in its own fibres, or other conſtituent ſubſtances, which finding no reſiſtance in the place deſerted by the quickſilver, may contribute to its ſwelling (for that we will not now examine) he that has duly conſidered the account already given of this intruſion of the pulp into the glaſs, will find no need of our author's internal funiculus; which to ſome ſeems more difficult to conceive, than any of the phænomena in controverſy is to be explained without it.

CHAP. IV.

Page 12.

BY what we have already ſaid againſt our examiner's third argument, we may be aſſiſted to anſwer his firſt, though he propoſe it as a very clear demonſtration; and though it be indeed the principal thing in his book. *Sumatur* (ſays he) *tubus brevior digitis* $29\frac{1}{2}$ *puta digitorum* 20. *non tamen clauſus altero extremo (ut hactenus) ſed utrinque apertus: hic tubus, immerſo ejus orificio argento reſtagnanti, ſuppoſitoque digito, ne effluat argentum tubo infundendum, impleatur argento vivo: aliuſque deinde digitus orificio quoque applicetur, illudque bene claudat. Quo facto, ſi ſubtrahatur inferior digitus, ſentietur ſuperior vehementer trahi ac ſugi intra tubum, tamque pertinaciter ei (vel argento potius, ut poſtea) adhærere, ut ipſum tubum cum toto argento incluſo facilè elevet teneatque in vaſe pendulum.*

Ex quo ſane experimento clariſſimè refellitur hæc ſententia: cum enim, juxta eam, argentum in tubo hujuſmodi 20 *tantum digitorum, ſurſum trudatur à præponderante aëre externo: nunquam profecto per eam explicabitur, quomodo digitus ille ſic trahatur deorſum, tuboque tam vehementer adhæreat; non enim à trudente ſurſum poteſt ſic deorſum trahi.*

‘ Take a tube ſhorter than twenty-nine inches and an half, for inſtance of twenty digits, not ſhut, as hitherto, at one end, but with both ends open: let this tube, its orifice being immerſed in reſtagnant mercury, and one finger being placed underneath, that the mercury to be poured in run not through, be filled with mercury; and then another finger be applied to its orifice, to cloſe it well: which being done, if you draw away your lower finger, the upper will be found to be ſtrongly drawn and ſucked into the tube, and ſo ſtiffly to adhere to it (or rather to the quickſilver, as I ſhall hereafter ſhew) that it will elevate the tube itſelf with all the quickſilver, and make it continue to hang pendulous in the veſſel.

‘ FROM which experiment this opinion is moſt clearly refuted: for ſeeing according to it the quickſilver in ſuch a tube but twenty inches long muſt be thruſt upwards by the preponderating air; it will never by it be explained how this finger is ſo drawn downwards, and made ſo ſtrongly to ſtick to the tube. For it cannot by the air thruſting upwards be thus drawn downwards.’

THUS far our author's objection, in anſwer whereunto I have divers things to repreſent, to ſhew that a good account may be given of this experiment in the hypotheſis of the Elateriſts; which is ſufficient to manifeſt how far the argument is from being ſo unanſwerable as the propoſer of it would perſuade his reader.

I DENY then that the finger is drawn downward, or made by ſuction to adhere to the tube; but I explicate that which he calls the ſuction of the finger, as I lately did in anſwer to his third argument, as we ſhall more particularly ſee anon.

HE ſays indeed, that the air which thruſt up the quickſilver cannot ſo ſtrongly draw down the finger. As if the air were not a fluid body, but a ſingle and intire pillar of ſome ſolid matter. But to ſhorten our reply to his objections, the beſt way perhaps will be briefly to explicate the phænomenon thus:

WHEN the tube is filled with quickſilver, the finger that ſtops the upper orifice is almoſt equally preſſed above and at the ſides by the contiguous air; but when the lower finger is removed, then the cylinder of mercury, which before gravitated upon the finger, comes to gravitate upon the reſtagnant mercury, and by its intervention to preſs againſt the outward air: ſo that againſt thoſe parts of the finger that are contiguous to the air there is all the wonted preſſure of the outward air; whereas againſt that pulp that is contiguous to the mercury there is not ſo much preſſure as againſt the other parts of the finger, by two thirds. I ſay by two thirds, or thereabout, becauſe the mercurial cylinder in this experiment is ſuppoſed to be twenty inches high; and if it were but a little more than thirty inches high (which is a third more) then the weight of the quickſilver would take off not two thirds only, but the whole preſſure of the outward air, from the above-mentioned pulp of the finger. For in that caſe the quickſilver would quite deſert it, and ſettle beneath it. Wherefore ſince it has appeared by our anſwer to the examiner's third argument, that the preſſure of the outward air is taken off from the body that remains in the upper part of the tube, according to the weight of the liquor ſuſpended in the tube; and ſince in our hypotheſis the preſſure of the outward air is able to keep thirty inches of quickſilver, or two or three and thirty foot of water, ſuſpended in a tube; it need be no great wonder, if a preſſure of the ambient air, equal to the weight of a cylinder of water of near twenty two foot long, ſhould be able to thruſt in the pulp of the finger at the upper orifice of the tube, and make it ſtick cloſely enough to the lip of it.

I KNOW the examiner affirms, that no thruſting or preſſure from without can ever effect ſuch an adheſion of the finger to the tube. But this ſhould be as well proved as ſaid. But firſt, though I am willing to think the examiner would not knowingly relate any thing he is not perſuaded of; yet as far as I, and another perſon very well verſed in theſe experiments, have purpoſely tried, I could not find the adheſion of the finger to the tube to be near ſo ſtrong as our author hath related. Secondly, if you carefully endeavour by preſſure and otherwiſe to thruſt the pulp of your finger into the orifice of the tube, you may through the glaſs perceive it to be manifeſtly tumid in the cavity of the pipe. And if by preſſing your finger againſt the orifice of the tube, you ſhould not make the pulp adhere quite ſo ſtrongly to the tube, nor ſwell quite ſo much within it, as may happen in ſome mercurial experiments; it is to be conſidered, that the air being a fluid as well as a heavy body, it does not (as groſſer weights would) preſs only againſt the upper part of the finger, but preſſing as much of the finger as is expoſed to it almoſt every where, and almoſt uniformly, as well as ſtrongly, it does by its lateral preſſure on every ſide thruſt in the pulp of the finger into the hole, where there is not any reſiſtance at all, or at leaſt near ſo much preſſure againſt the pulp as that of the ambient air againſt the parts of the finger contiguous to it.

BY this it may appear that we need not borrow the objection our author offers to lend us; namely, that in the experiment under conſideration the quickſilver is preſſed downward by the ſpring of ſome air lurking betwixt it and the finger. (Though I am prone to think, that unleſs the experiment be made with a great deal of care, ſuch a thing may eaſily happen, and contribute to the ſtronger adheſion of the finger to the tube.) This I ſay may appear, notwithſtanding what our author objects, that the air expanding itſelf will thruſt away the finger upwards, ſince the contrary of that pretence we have lately manifeſted in the anſwer to his third argument. And as for what he adds, to confirm his argumentation, in theſe words; *Quod vel inde* Page 14. *confirmatur, quia cum præponderans ille aër ſuccedat (uti aſſeritur) loco ſublati inferioris* *digiti, id eſt, eodem modo nunc ſuſtentet argentum quo ante ab applicato digito inferiore ſuſtentabatur; manifeſtum eſt, non debere, juxta hanc ſententiam, magis deorſum trahi digitum ſuperiorem poſt ſublatam inferiorem quam ante. Cum itaque contrarium planè doceat experientia, ſatis liquet ſententiam illam eſſe falſam:* 'Which is thence confirmed, becauſe if that preponderating air ſucceeds, as is aſſerted, in the place of the lower finger which was withdrawn, that is, if it uphold the quickſilver after the ſame manner which it was upheld by the lower finger applied under it; it is manifeſt according to this opinion, that the finger on the top ought not to be more drawn downwards after the lower finger is removed than before. Seeing then that experience teacheth the contrary, it is manifeſt that opinion muſt be falſe:' We muſt conſider that the tube being ſuppoſed perfectly full of mercury, the finger that ſtops the lower orifice, is wont to be kept ſtrongly preſſed againſt it, leſt any of that ponderous liquor ſhould get out between the tube and the finger. So that although both the lower finger do indeed keep up the mercury in the tube, and the preſſure of the outward air would do ſo too; yet there is this difference, that the preſſure of the atmoſphere depending upon its weight, cannot be intended and weakened as we pleaſe, as can that of the undermoſt finger. And therefore whereas the atmoſpherical cylinder will not keep up a cylinder of quickſilver of above thirty inches high, thoſe that make the Torricellian experiment do often, upon one occaſion or other, keep up with the finger a mercurial cylinder of perhaps forty or fifty inches, or far more: ſo that whereas in our caſe, before the removal of the undermoſt finger, the pulp of the uppermoſt muſt have about the ſame preſſure againſt it where it is contiguous to the mercury, as there is againſt the other part of the ſame finger; after the removal of the undermoſt finger, there is as much of the atmoſpherical preſſure, if I may ſo ſpeak, taken off from the newly mentioned pulp, as counter-balances a cylinder of quickſilver of twenty inches long.

CHAP. V.

Page 18. THE examiner's fourth and laſt experiment is thus propoſed: *Quarto denique* (ſays he) *impugnatur; quia ex eo ſequetur, argentum vivum per ſimilem tubum e vaſculo exugi poſſe eâdem prorſus facilitate qua ex eodem exugeretur aqua: quod tamen experientiæ repugnat, quâ docemur aquam in os ſugentis facillimè attrahi; quo tamen argentum vivum ne toto quidem adhibito conatu perduci queat, imo vix ad tubi medietatem.*

Sequelam autem ſic oſtendo: Quia cum in hac ſententia nihil aliud agendum ſit quam hoc, ut per tubum ſic aſcendat ſubjectus liquor, ſive aqua fuerit, ſive argentum, niſi ut ſugendo ſurſum trahatur aër tubo incluſus, quo ſic attracto aſcendit illico ſubjectus liquor, protruſus nimirum ab externo aëre jam præponderante (uti docet Pecquettus *in diſſertatione anatomica pag.* 63.) *manifeſtum eſt, eadem planè facilitate exugendum ſic argentum vivum qua exugitur aqua: quod quum experientiæ tam aperte repugnat, neceſſe eſt ſententiam ex qua ſequitur falſam eſſe.*

'In the fourth place, it is impugned, becauſe thence it would follow that quickſilver through a like tube might be ſucked with the ſame eaſineſs out of a veſſel that water is ſucked out of the ſame. Which notwithſtanding is contrary to experience, by which we are taught that water is eaſily drawn into the mouth of him that ſucks; whereas quickſilver cannot be drawn thither by his utmoſt endeavour, nay ſcarce unto the middle of the tube.

'The ſequel I thus manifeſt: becauſe ſeeing, according to this opinion, that the liquor underneath, whether it be water or mercury, may ſo aſcend, no more is required but that the air ſhut in the tube may be drawn upwards by ſucking; which

‘ being drawn up, the liquor underneath will immediately aſcend, being thruſt thither by the external air now preponderating (as *Pecquet* declares in his Anatomical Diſcourſe, *p.* 63.) it is manifeſt that the mercury may be ſucked out with the ſame eaſineſs that water is ſucked out with. Which being ſo evidently againſt experience, the opinion from whence it is deduced muſt needs be falſe.’

THIS experiment I remember I made ſome years ago; accordingly, it is alledged in the fourth eſſay of the treatiſe (I was then writing) to prove againſt the vulgar opinion, that liquors do not, to prevent a vacuum, ſpontaneouſly aſcend; which I preſume will be ſo far allowed of by our author, who would have liquors ſuppoſed to be raiſed by ſuction violently drawn up by the contraction of his funiculus. But to examine this experiment, as it concerns the preſent controverſy, we may recall to mind that we formerly ſhewed in the anſwer to our author's third argument, that when the mercurial cylinder that leans upon the reſtagnant mercury has at the other end of it air, kept from any intercourſe with the atmoſphere, that included air has ſo much of the preſſure of the external air taken off from it, as counterpoiſes the mercurial cylinder. And the finger that is expoſed to the whole preſſure of the ambient air in ſome of its parts, and in others but to the much fainter preſſure of the included air, endures an unuſual preſſure from the preponderating power of the atmoſphere.

WE may conſider alſo, that there is againſt the thorax, and thoſe muſcles of the abdomen that are ſubſervient to reſpiration, the preſſure of the whole ambient air. Which preſſure notwithſtanding, the muſcles deſigned for the uſe of reſpiration are able, without any conſiderable reſiſtance, to dilate the thorax at pleaſure; becauſe, as faſt as they open the cheſt, and by dilating it weaken the ſpring of that air which is then within the body, the external air by flowing in, for want of finding the uſual reſiſtance there, keeps that within the thorax in an æquilibrium of force with that without. Theſe things premiſed, it is not difficult in our hypotheſis to give an anſwer to our examiner's experiment. For we ſay when a cylinder of mercury is raiſed in the tube to any conſiderable height, the preſſure of the air in the thorax is leſſened by the whole weight of that mercurial cylinder; and conſequently the reſpiratory muſcles are thereby diſabled to dilate the cheſt as freely as they were wont, by reaſon of the prevalency of the undiminiſhed preſſure of the external air againſt the weakened preſſure of the internal. But if inſtead of mercury you ſubſtitute water, ſo ſhort a cylinder of that comparatively light liquor takes off ſo little of the preſſure of the included air, that it comes into the lungs with almoſt its uſual ſtrength, and conſequently with almoſt as much force as the outward air preſſes with againſt the thorax.

AND on this occaſion there occurs to my thoughts a noble experiment of the moſt ingenious Monſieur *Paſchal*, which clearly ſhews, that if we could free the upper part of ſuch a tube as we are now conſidering from the preſſure of all internal air, it would follow, as the examiner ſays it ſhould, that the quickſilver would by the preſſure of the outward air be impelled up into the tube as well as water, till it had attained a height great enough to make its weight not inferior, but equal to that of the atmoſphere. The experiment itſelf being ſo pertinent and conſiderable, we ſhall annex it in the ſame words wherein it is related by his countryman and acquaintance, the learned and candid *Gaſſendus: Neque hoc verò ſolum, ſed inſuper vitreo diabete clyſteréve ea qua par fuerit longitudine confecto, & poſt embolum ad orificium uſque compulſum, immiſſo ad normam in ſubjectum hydrargyrum, deprehendit, ubi embolum ſenſim deinde educitur, conſequi hydrargyrum aſcendereque ad eandem uſque duorum pedum & digitorum trium cum ſemiſſe altitudinem.* ‘ And not only this, but over and above, if a glaſs diabetes

Gaſſ. Phyſ. Sect. 1. lib. 2 p. 204. De nupero Inanis experimento.

‘ or ſyringe be made of a ſufficient length, and after that the ſucker is thruſt into the utmoſt orifice, it be placed according to uſe in the mercury underneath; he finds that as ſoon as the ſucker is drawn out, the mercury follows, and aſcends to the ſame height of two feet and three inches and an half.’ To which he immediately ſubjoins a circumſtance very conſiderable to the preſent controverſy, in the following clauſe: *Ac ubi deinceps, adhibita licet non majore vi, embolum altius educitur, conſiſtere hydrargyrum, neque amplius conſequi, ac fieri interim Inane, quod ſpatium intercipitur ab ipſo ad embolum uſque.* ‘ And when afterwards, although no greater force be added, the ſucker is drawn higher, he finds that the mercury ſtands, and follows no further; and ſo that ſpace is made empty which remains between the mercury and the ſucker.’ Thus far he. So that as to the examiner's experiment, we may well explicate it in our hypotheſis, by ſaying, that, agreeably to it, it happens, that in a more forcible reſpiration the mercurial cylinder is raiſed higher than in a more languid; becauſe, in the former caſe, the cheſt being more dilated, the included air is alſo more expanded; whereby its debilitated ſpring cannot as before enable the mercurial cylinder to counterpoiſe altogether the preſſure of the ambient air. And that the reaſon why the quickſilver is not by reſpiration raiſed as high as it is kept ſuſpended in the Torricellian experiment, is not, that the preſſure of the outward air is unable to to raiſe it ſo high, but becauſe, as we have already declared, the free dilatation of the thorax is oppoſed by the preſſure of the ambient air: which preſſure being againſt ſo great a ſuperficies, and being but imperfectly reſiſted by the debilitated preſſure of the air within the thorax, will be eaſily imagined to be very conſiderable, by him who conſiders that in our engine the preſſure of the external air againſt the ſucker of leſs than three inches diameter was, as we relate in the 33d experiment, able to thruſt up a weight of above a hundred pound. And here we may obſerve upon the by, in confirmation of our former doctrine, that when we ſtrongly ſuck up quickſilver in a glaſs tube, though the elevation of the quickſilver be, according to our author, performed likewiſe by his Funiculus contracting itſelf every way, and though there be a communication betwixt the internal ſurface of the lungs, and the cavity of the tube; yet we feel not in our lungs any endeavour of the ſhrinking Funiculus to tear off that membrane they are lined with.

And thus we have examined our author's four arguments, to prove that in the Torricellian experiment the quickſilver cannot be kept ſuſpended by the counterpoiſe of the external air: againſt which opinion he tells us indeed, that other arguments
Page 19. might be alledged, but as it is not probable that if he had thought them better than thoſe he has elected to inſiſt on, he would have omitted them, ſo it is not unlikely that anſwers might be as well found for them as for the others; eſpecially ſince that which he ſingles out for a ſpecimen is, that from his adverſaries hypotheſis it would follow, that the quickſilver would deſcend much more (I ſuppoſe it is a miſtake of the preſs, for much leſs) in cold weather than in hot, becauſe the air is then thicker and heavier, and therefore ought to impel up the quickſilver higher. For beſides that we ſhall in its due place queſtion the validity of our author's conſequence, it will be here ſufficient to reply, that the obſervation on which he grounds it does not conſtantly hold, as his objection ſuppoſes: which may appear by that part of our 18th experiment whence the matter of fact is deſumed, as we ſhall have occaſion to take further notice of when we ſhall come to the defence of that experiment. So that what has been hitherto diſcourſed on both ſides being duly conſidered, the reader is left to judge what ground the examiner had for the Ἐπιφώνημα wherewith he is pleaſed to conclude
Page 19. his third chapter; *Maneat igitur tot argumentis comprobatum, quorum quodlibet ſe ſolo ſufficit, argentum (facto experimento in loco aperto) per externi aëris gravitatem à lapſu*

minime ſuſtentari. ‘ Be it therefore confirmed by ſo many arguments, of which every ‘ one is ſufficient in itſelf, that quickſilver (the experiment being made in an open ‘ place) is not upheld from falling by the weight of the external air.’

CHAP. VI.

HIS fourth chapter, wherein the title promiſes that he will prove, *argentum in loco occluſo non ſuſtentari à lapſu per ipſum aëris elaterium*, ‘ that quickſilver in a ‘ cloſe ſpace is not upheld from falling by the elater or ſpring of the air ;’ is very ſhort, and does not require that we ſhould dwell long upon it. For the proof he brings of his aſſertion being this, *Cum tota vis hujus elaterii pendeat à refutato jam* Page 20. *aëris æquipondio cum digitis 29½ argenti vivi, ita ut nec plus nec minus faciat hoc elaterium in loco occluſo quam fit per illud æquipondium in loco aperto ; manifeſtum eſt, cum jam oſtenſum ſit fictitium planè eſſe hujuſmodi æquipondium, fictitium quoque eſſe tale elaterium :* ‘ Seeing the whole power of this ſpring depends upon the already-confuted æquilibrium ‘ of the air with 29 inches and an half of quickſilver, ſo that this ſpring does neither ‘ more nor leſs in a cloſe place than is done by that æquilibrium in an open place ; ‘ it is manifeſt, ſeeing this equilibrium is already ſhewn to be plainly fictitious and ‘ imaginary, that the ſpring of the air is ſo likewiſe.’ This being no new argument, but an inference from thoſe he had ſet down in the former chapter, by our anſwers to them it is become needleſs for us to make any diſtinct reply to this. We ſhall rather deſire the reader to take notice, that whereas our author ſays, that according to his adverſaries, *Nec plus nec minus faciat hoc elaterium in loco occluſo quam fit per illud æquipondium in loco aperto :* ‘ And that this ſpring doth neither more nor leſs in a cloſe place, than is done by that æquilibrium in an open place :’ Whatever others may have written, we for our part allow of this opinion but in ſome caſes ; for in others we have performed much more by the ſpring of the air, which we can within certain limits increaſe at pleaſure, than can be performed by the bare weight, which for aught we know remains always ſomewhat near the ſame. And of this advantage that the ſpring of the air may have in point of force above the weight of it, we have formerly given an inſtance in our 17th experiment (where, by compreſſing the air in the receiver, we impelled the mercurial cylinder higher than the ſtation at which the counterpoiſe of the air is wont to ſuſtain it) and ſhall hereafter have occaſion to give yet more conſiderable proofs. To the lately recited words our examiner ſubjoins theſe ; *Adde, cum allata jam capite præcedente experimenta de adhæſione digiti, &c. eodem* Page 21. *modo ſe habent in loco clauſo ac in aperto, neceſſarium eſſe facta ex eis argumenta contra æquipondium, eadem quoque contra elaterium vim habere.* ‘ Add, that ſeeing the experiments ‘ brought in the chapter above of the adheſion of the finger, &c. are alike in a cloſe ‘ and an open place ; it is neceſſary and certain, that the ſame arguments made ‘ againſt the æquilibrium have force againſt the ſpring of the air.’ But though he propoſe this as a new argument, yet ſince it is built but upon the adheſion of the finger (of which we have already given an account in our hypotheſis) I ſee not how it requires any new and particular anſwer. And whereas he ſays, that the experiments he had mentioned concerning the adheſion of one's finger, &c. *eodem modo ſe habent in loco clauſo ac in aperto* ; I could wiſh he had added what way he took to make the trials. For he gives no intimation that he did them any other ways than in ordinary rooms : and in ſuch there ſcarce ever wants a communication betwixt the inward and outward air, either at the chimney, or window, or door not exactly ſhut, or at ſome hole or crevice or other, by means of which the weight of the atmoſphere has its operation within the room.

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To his ſecond argument our author adds not a third, unleſs we take take that for an argument which he immediately annexes to his laſt recited words: *Et profectò* (ſays he) *ſi ſecum expenderent hi authores, quanta ſit difficultas explicandi hujuſmodi aëris elaterium, niſi idem aër ſe ſolo occupet majorem locum (ut paulo ante) credo eos ſententiam facilè mutaturos.* ‘And really if theſe authors would conſider how great a difficulty there is in explaining this ſpring of the air, unleſs the ſame air by itſelf alone may take up a greater place, I believe they would readily alter their opinion.’ But this being ſaid *gratis*, does not exact an anſwer; and he muſt make it more intelligible than any man that I know of has yet done, how the ſame air can adequately fill more ſpace at one time than at another, before he perſuade me to change my opinion about the ſpring of the air: eſpecially ſince he himſelf allows that the air has a ſpring, whereby it is able, when it has been violently compreſſed, to recover its due extenſion; the manner whereof if he will intelligibly explicate, his adverſaries will have no great difficulty to make out the ſpring of the air. But whether his hypotheſis, or ours, be the more intelligible, will be more properly conſidered in the ſecond part of our diſcourſe, to which we will therefore now proceed.

PART II.

Wherein the Adverſaries Funicular Hypotheſis *is examined.*

CHAP. I.

What is alleged to prove the Funiculus, *is conſidered; and ſome difficulties are propoſed againſt the Hypotheſis.*

THE hypotheſis that the examiner would, as a better, ſubſtitute in the place of ours, is, if I miſtake it not, briefly this; that the things we aſcribe to the weight or ſpring of the air are really performed by neither, but by a certain Funiculus, or extremely thin ſubſtance, provided in ſuch caſes by nature, *ne detur vacuum*, which being exceedingly rarefied by a forcible diſtenſion, does perpetually and ſtrongly endeavour to contract itſelf into dimenſions more agreeable to the nature of the diſtended body; and conſequently does violently attract all the bodies whereunto it is contiguous, if they be not too heavy to be removed by it.

But this hypotheſis of our author's does to me, I confeſs, appear liable to ſuch exceptions, that though I diſliked that of his adverſaries, yet I ſhould not embrace his, but rather wait till time and further ſpeculations or trials ſhould ſuggeſt ſome other theory, fitter to be acquieſced in than this; which ſeems to be partly precarious, partly unintelligible, partly inſufficient, and beſides needleſs: though it will not be ſo convenient to prove each of theſe apart, becauſe divers of my objections tend to prove the doctrine, againſt which they are alleged obnoxious to more than one of the imputed imperfections.

First, then, the arguments by which our author endeavours to evince his Funiculus, are incompetent for that end. The arguments which he propoſes in his ſixth chapter (where he undertakes to make good his aſſertion) I there find to be three.

The firſt he ſets down in theſe words; *Conſtat hoc primò ex jàm dictis capite præcedente: nequit enim argentum deſcendens ſic digitum deorſum trahere, tuboque affigere, niſi à tali funiculo ſuſpendatur, eumque ſuo pondere vehementer extendat, ut per ſe patet.* 'This appears from what has been already ſpoken in the preceding chapter: for the quickſilver deſcending, cannot ſo draw the finger downwards, and faſten it unto the tube, unleſs it be hung upon the finger by ſuch a cord, which by its weight it vehemently ſtretches, as is manifeſt by itſelf.' But to this proof anſwer has been made already in the former part of this diſcourſe: only whereas the author ſeems to refer us to the foregoing chapter, we will look back to it, and take notice of what I find there againſt the Vacuiſts. For though I neither am bound, nor intend, in this diſcourſe, to declare myſelf for or againſt a vacuum; yet, ſince I am now writing againſt the funicular hypotheſis, it will much conduce to ſhew that it is not firmly grounded, if I examine what he here alleges againſt the aſſertors of a vacuum.

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In the next place therefore, I conſider that, according to the examiner, there can be no vacuum; and that he makes to be the main reaſon why nature in the Torricellian and our experiments does act after ſo extraordinary a manner as is requiſite to the production of his Funiculus. For, in the 47th page, having, in his adverſaries name, demanded what need there is, at the deſcent of the quickſilver, that before it falls, a ſuperficies ſhould be ſeparated from it, and extended; *Reſpondeo* (ſays he) *ideo hoc fieri, ne detur vacuum; cum nihil aliud ibi adſit quod loco argenti deſcendentis poſſit ſuccedere,* 'I anſwer, that this comes to paſs, that there may be no vacuity, ſeeing there is nothing elſe there that can ſucceed into the place of the deſcending quickſilver.' To which he immediately ſubjoins (with what cogency I will not now examine) *Atque hinc planè confirmatur commune illud per tot jam elapſa ſecula uſurpatum in ſcholis axioma, viz. Naturam à vacuo abhorrere.* 'And hence is confirmed that common axiom uſed in the ſchools for ſo many ages paſt, that nature doth abhor a vacuum.' And though he ſeem to make his Funiculus the immediate cauſe of the phænomena occurring in the Torricellian and our experiments; yet that, if you purſue the inquiry a little higher, he reſolves them into nature's abhorrency of a vacuum, himſelf plainly informs us in the next page; *Nam licet* (ſays he) *immediata ratio cur aqua v. g. ex hydria hortulana ſuperne clauſa (quo exemplo utuntur) non deſcendat, non ſit metus vacui, ſed ea quam modo diximus, nempe quod non detur ſufficiens pondus ad ſolvendum illum nexum quo adhæreat aqua clauſæ hydriæ ſummitati; ad eam tamen rationem tandem neceſſario veniendum eſt.* 'For though the immediate cauſe why water (for inſtance) doth not deſcend from a gardener's watering-pot, (for that example they uſe) ſtopt on the top, is not the fear of a vacuum, but the reaſon now mentioned; namely, that there is not weight ſufficient to looſe that conjuncture by which the water doth adhere to the top of the cloſed water-pot: nevertheleſs, in the end, we muſt of neceſſity come to that cauſe.' But though, as well our author's Funiculus as the other ſcarce conceivable hypotheſes that learned men have deviſed, to account for the ſuſpenſion of the quickſilver otherwiſe than by the reſiſtance of the external air, ſeem to have been excogitated only to ſhun the neceſſity of admitting a vacuum; yet I ſee not how our examiner cogently proves, either that there can be none *in rerum naturâ*, or that *de facto* there is none produced in theſe experiments. For, in his fifth chapter (where he profeſſedly undertakes that taſk) he has but theſe two incompetent arguments. The firſt is drawn from the attraction, as he ſuppoſes, of the finger into the deſerted cavity of the tube in the Torricellian experiment;

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Page 22. riment: *Quæ quidem* (ſays he) *tam vehemens tractio & adhæſio, cum non niſi à reali aliquo corpore inter digitum & argentum conſtitutum queat provenire, manifeſtum eſt ſpatium illud vacuum non eſſe, ſed verâ aliquâ ſubſtantiâ repletum.* ‘ Which traction and adheſion, when it cannot proceed but from ſome real body placed between the finger ‘ and the mercury, it is manifeſt that that ſpace is not empty, but filled with ſome ‘ true ſubſtance.’ But to this argument having already given an anſwer, let us (without ſtaying to urge, that the Vacuiſts will perhaps object, that they ſee not a neceſſity, though they ſhould admit of traction in the caſe, that the internal ſubſtance muſt therefore perfectly repleniſh the deſerted cavity; without preſſing this, I ſay, let us) conſider his other, which he draws from the diaphaneity of the deſerted part of the tube, which ſpace, he ſays, were it empty, would appear like a little black pillar, *Eo quod nullæ ſpecies viſuales neque ab eo neque per illud poſſunt ad oculum pervenire:* ‘ Becauſe no viſual ſpecies could proceed either from it, or through it, unto ‘ the eye.’ But (not to engage ourſelves in optical ſpeculations and controverſies) if we grant him ſomewhat more than perhaps he can prove; yet as the experiment will not demonſtrate that there is nothing of body in any part of the ſpace deſerted by the mercury, ſo neither will the argument conclude (as the propoſer of it does twice in this chapter) that ſpace *verâ aliquâ ſubſtantiâ repleri,* ‘ to be filled with any true ‘ ſubſtance.’ For, according to the hypotheſis of the Epicureans and other Atomiſts, who make Light to be a corporeal effluvium from lucid bodies, and to conſiſt of atoms ſo minute as freely to get in at the narrow pores of glaſs, there will be no cauſe to deny interſperſed vacuities in the upper part of the tube. For the corpuſcles of light that permeate that ſpace may be ſo numerous, as to leave no ſenſible part of it un-inlightened; and yet may have ſo many little empty intervals betwixt them, that if all that is corporeal in the ſpace we ſpeak of were united into one lump, it would not perhaps adequately fill the one half (not to ſay the tenth, or even the hundredth part) of the whole ſpace: according to what we have noted in the 17th experiment, that a room may appear full of the ſmoke of a perfume, though if all the corpuſcles that compoſe that ſmoke were re-united, they would again make up but a ſmall paſtil. To which purpoſe I remember I have taken camphire, of which a little will fill a room with its odour, and having in well-cloſed diſtillatory glaſſes caught the fumes driven over by heat, I thereby reduced them to re-conjoin into true camphire, whoſe bulk is very inconſiderable in compariſon of the ſpace it fills as to ſenſe, when the odorous corpuſcles are ſcattered through the free air.

To which I might add, that the Torricellian experiment being made in a dark night, or in a room perfectly darkened, if it ſucceed (as there is little cauſe to expect it will not) it may well be doubted whether our author's argument will there take place. For if he endeavour to prove that the place in queſtion was full in the dark, becauſe, upon the letting in of the day, or the bringing in of a candle, the light appears within it, the Vacuiſts may reply according to their hypotheſis, that that light is a new one, flowing from the lucid body that darts its corporeal beams quite through the glaſs and ſpace we diſpute about, which, for want of ſuch corpuſcles, were not juſt before viſible.

AND ſuppoſing light not to be made by a trajection of atoms through diaphanous bodies, but a propagation of the impulſe of lucid bodies through them; yet it will not thence neceſſarily follow, that the deſerted part of the tube muſt be full: as in our 27th experiment (though many of thoſe groſs aërial particles that appeared neceſſary to convey a languid ſound were drawn out of our receiver at the firſt and ſecond exſuction: yet there remained ſo many of the like corpuſcles, that thoſe that were wanting, were not miſſed by the ſenſe, though afterwards, when a far greater number was drawn out, they were) ſo there may be matter enough remaining to tranſmit the impulſe of light; though betwixt the particles of that matter there ſhould be ſtore of vacuities intercepted. Whereas our author pretends to prove, not only that there is no coacervate vacuity in the ſpace ſo often mentioned, but abſolutely that there is none. For it is in this laſt ſenſe as well as the other, that the ſchools, and our author who defends their opinion, deny a vacuum.

BUT notwithſtanding what we have now diſcourſed, as in our 17th experiment, we declined determining whether there be a vacuum or no; ſo now, what we have ſaid to the examiner's argument, has not been to declare our whole ſenſe of the controverſy, but only to ſhew, that though his hypotheſis ſuppoſes there is no vacuum, yet his arguments do not ſufficiently prove it; which may help to ſhew his doctrine to be precarious: for otherwiſe the Carteſians, though Pleniſts, may plauſibly enough (whether truly or no, I now diſpute not) decline the neceſſity of admitting a vacuum in the deſerted ſpace of the tube, by ſuppoſing it filled with their ſecond and firſt element, whoſe particles they imagine to be mute enough freely to paſs in and out through the pores of glaſs. But then they muſt allow the preſſure of the outward air to be the cauſe of the ſuſpenſion of the quickſilver: for though the *materia cœleſtis* may readily fill the ſpaces the mercury deſerts; yet that within the tube cannot hinder ſo ponderous a liquor from ſubſiding as low as the reſtagnant mercury; ſince all the parts of the tube, as well the lowermoſt as the uppermoſt, being pervious to that ſubtile matter, it may with like facility ſucceed in whatever part of the tube ſhall be forſaken by the quickſilver.

The examiner's ſecond argument in the ſame place is, that ſince the mercurial cylinder is not ſuſtained by the outward air, it muſt neceſſarily be, that it be kept ſuſpended by his internal ſtring. But ſince, for the proof of this he is content to refer us to the third chapter, our having already examined that, allows us to proceed to his third argument, which is, that the mercurial cylinder reſting in its wonted ſtation, does not gravitate; as may appear by applying the finger to the immerſed or lower orifice of the tube. Whence he infers, that it muſt of neceſſity be ſuſpended from within the tube. And indeed if you dexterouſly apply your finger to the open end of the tube, when you have almoſt, but not quite, lifted it out of the reſtagnant mercury (which circumſtance muſt not be neglected, though our author have omitted it) that ſo you may ſhut up no more quickſilver than the mercurial cylinder is wont to conſiſt of, you will find the experiment to ſucceed well enough (which makes me ſomewhat wonder to find it affirmed, that the learned *Maignan* denies it) not but that you will feel upon your finger, a gravitation or preſſure of the glaſs tube, and the contained mercury as of one body; but that you will not feel any ſenſible preſſure of the mercury apart, as if it endeavoured to thruſt away your finger from the tube. But the reaſon of this is not hard to give in our hypotheſis; for according to that, the mercurial cylinder and the air counterpoiſing one another, the finger ſuſtains not any ſenſibly differing preſſure from the ambient air that preſſes againſt the nail and ſides of it, and from the included quickſilver that preſſes againſt the pulp. But if the mercurial cylinder ſhould exceed the uſual length, then the finger would feel ſome preſſure from that ſurpluſage of quickſilver, which the air does not aſſiſt the finger to ſuſtain. So that this pleaſant phænomenon may be as well ſolved in our hypotheſis as in the examiner's: in which, if we had time to clear an objection which we foreſee might be made, but might be anſwered too, we would demand why, when the mercury included in the tube is but of a due altitude, it ſhould run out upon the removal of the finger that ſtops it beneath, in caſe it be ſuſtained only by the internal Funiculus, and do, according to his doctrine, when the Funiculus ſuſtains it, emulate a ſolid body, if the preſſure of the external air has not (as our author teaches it not to have) any thing to do in this matter.

And if ſome inquiſitive perſon ſhall here object, that certainly the finger muſt feel much pain by being ſqueezed between two ſuch preſſures as that of a pillar of thirty inches of quickſilver on the one ſide, and an equivalent preſſure from the atmoſpherical pillar on the other, it may readily be repreſented, that in fluid bodies (ſuch as are thoſe concerned in our difficulty) a ſolid body has no ſuch ſenſe of preſſure from the ambient bodies as (unleſs experience had otherwiſe inſtructed us) we ſhould perhaps imagine. For, not to mention that having inquired of a famous diver, whether he found himſelf ſenſibly compreſſed by the water at the bottom of the ſea; he agreed with the generality of divers in the negative. I am informed that the learned *Maignan* did purpoſely try, that his hand being thruſt three or four palms deep into quickſilver, his fingers were not ſenſible, either of any weight from the incumbent, or of any preſſure from the ambient quickſilver. The reaſon of which (whether that inquiſitive man have given it or no) is not neceſſary, in our preſent controverſy, to be looked after.

Page 25. To theſe three arguments the examiner adds not a fourth, unleſs he deſigns to preſent it us in this concluding paſſage: *Huc etiam faciunt inſignes librationes, quibus argentum ſubito deſcendens agitatur: idem enim hic fit, quod in aliis pendulis & ab alto demiſſis fieri ſolet.* ‘And to this purpoſe make thoſe conſiderable vibrations, with ‘which quickſilver is ſtirred in its deſcent: for the ſame thing happens here, that ‘befalls other pendula in their fall from on high.’ But of this phænomenon alſo it is eaſy to give an account in our hypotheſis by two ſeveral ways; whereof the firſt (which is proper chiefly when the experiment is made in a cloſe place, as our receiver is) that the quickſilver by its ſudden deſcent acquires an impetus ſuper-added to the preſſure it has upon the ſcore of its wonted gravity; whereby it, for a while, falls below its ſtation, and thereby compreſſes the air, that leans upon the reſtagnant mercury: which air, by its own ſpring, again forcibly dilating itſelf to recover its former extenſion, and (as is uſual in ſprings) haſtily flying open, expands itſelf beyond it, and thereby impels up the quickſilver ſomewhat above its wonted ſtation; in its fall from whence it again acquires ſomewhat (though not ſo much as before) of impetus, or power, to force the corpuſcles of the air to a ſub-ingreſſion; and this reciprocation of preſſure betwixt the quickſilver and the outward air decreaſing by degrees, does at length wholly ceaſe, when the mercury has loſt the ſuper-added preſſure, which it acquired by its falling from parts of the tube higher than its due ſtation. But this firſt way of explicating theſe vibrations is not neceſſary in the free air: for if we conſider the ambient air only as a weight, and remember what we have newly ſaid of the impetus acquired by deſcent; this phænomenon may be eaſily enough explained, by taking notice of what happens in a balance, when one of the equiponderant ſcales chancing to be depreſſed, they do not till after many vibrations ſettle *in æquilibrio*.

And on this occaſion I ſhall add this experiment: I took a glaſs-pipe, whoſe two legs (very unequal in length) were parallel enough, and both perpendicular to that part of the pipe that connected them (ſuch a ſiphon is deſcribed in our 36th experiment, to find the proportion of the gravity of mercury and water) into this quickſilver was poured till it was ſome inches high, and equally high in both legs: then the pipe being inclined, till the moſt part of the quickſilver was fallen into one of the legs, I ſtopped the orifice of the other leg with my finger, and erecting again the pipe, though the quickſilver were forced to aſcend a little in that ſtopped leg; yet by reaſon my finger kept the air from getting away, the quickſilver was kept lower by a good deal in that ſtopped leg than in the other; but if, by ſuddenly removing my finger, I gave paſſage to the included and ſomewhat compreſſed air, the preponderant quickſilver in the other leg would, with the mercury in this unſtopped leg, make divers undulations before that liquor did in both legs come to reſt in an æquilibrium. Of which the reaſon may be eaſily deduced from what has been newly delivered; and yet in this caſe there is no pretence to be made of a Funiculus of violently diſtended air to effect the vibrations of the mercury.

CHAP. II.

Divers other difficulties are objected againſt the Funicular Hypotheſis.

THIRDLY, but though our examiner have not ſufficiently proved his hypotheſis, yet perhaps it may be in its own nature ſo like to be true, as to deſerve to be embraced as ſuch. Wherefore we will now take notice of ſome of thoſe many things, that to our apprehenſion render it very improbable.

AND firſt, whereas our author acknowledges, that quickſilver, water, wine and other liquors, will, as well one as another, deſcend in tubes exactly ſealed at the top, in caſe the cylinder of liquor exceed the weight of a mercurial cylinder of 29½ inches; and will ſubſide no longer than till it is come to equiponderate a cylinder of quickſilver of that height; whereas I ſay, the examiner is, by the ingenious Monſieur *Paſchal*'s, and other experiments, induced to admit this; it cannot but ſeem ſtrange, that, whatever the liquor be, there ſhould be juſt the ſame weight or ſtrength to extend them into a Funiculus: though water, for inſtance, and quickſilver be near fourteen times as heavy one as the other, and be otherwiſe of very diſtant natures; and though divers other liquors, as oil and water, be likewiſe of textures very differing. And this may ſomewhat the more be wondered at, becauſe our author (in his animadverſions upon our 31ſt experiment) is pleaſed to make ſo great a difference betwixt the diſpoſition of bodies of various conſiſtences, as fluid and firm, to be extenuated into a Funiculus, that he will not allow any human force to be able to produce one by the divulſion of two flat marbles, in caſe the contact of their ſurfaces were ſo exquiſite as quite to exclude all air; though in the ſame place his ratiocination plainly enough teaches (which experience however does) that adhering marbles, though with extraordinary difficulty, may be forcibly ſevered, and according to him, the ſuperficial parts may be diſtended into a Funiculus, that prevents a vacuum.

BUT now the hypotheſis of his adverſaries is not at all incumbered with this difficulty. For the weight of the outward air being that, which keeps liquors ſuſpended in tubes ſealed at the top; it matters not of what nature or texture the ſuſpended liquor is, provided its weight be the ſame with that of a mercurial cylinder equiponderant to the aërial one: as if there be a pound of lead in one ſcale, it will not deſtroy the æquilibrium, whether what be put in the other be gold, or quickſilver, or wool, or feathers, provided its weight be juſt a pound.

IN the next place we may take notice, that the account our examiners gives us of his Funiculus in the tenth chapter (where he takes upon him to explicate it) is much more ſtrange than ſatisfactory, and not made out by any ſuch parallel operations of nature, as his adverſaries will not (and may not well do it) diſpute the truth of. Whereas the weight and ſpring of the air may be inferred from ſuch unqueſtioned experiments, as are not concerned in our preſent controverſy. For the gravity of the air may be manifeſt by a pair of ſcales, and the ſpring of it diſcloſes itſelf ſo clearly in wind-guns and other inſtruments, that our adverſary (as we have already had occaſion to inculcate) does not deny it. But to conſider his explication of his Funiculus Page 38. he would have us note two things: firſt, *Argentum, dum replet totum tubum, non mere tangere ejus ſummitatem (ut primo aſpectu videtur) ſed eidem quoque firmiter adhærere. Patet hoc* (ſubjoins he) *experimento illo in primo argumento capitis tertii de tubo utrinque aperto.* ‘ Quickſilver, while it fills the whole tube, doth not only touch its top (as you ‘ would think at the firſt ſight) but doth firmly ſtick unto it alſo; as it is manifeſt ‘ from the experiment mentioned in the firſt argument of the third chapter, con‘ cerning the tube open at both ends.’ But what is to be anſwered to this proof may be eaſily gathered from what we have replied to that argument. And to what our author adds to prove, that the adheſion of the finger is to the ſubjacent mercury, Page 38. not the tube; namely, that *licet illud tubi orificium oleo, aliâve materiâ adhæſionem impediente, inungatur, non minus tamen firmiter adhærebit digitus quàm priùs:* ‘ Though ‘ that orifice of the tube be anointed with oil, or any other matter that will hinder ‘ adheſion, nevertheleſs the finger will no leſs firmly ſtick than before.’ An anſwer may be drawn from the ſame place: nor perhaps will his reaſoning much ſatisfy thoſe, who conſider, that bodies by truſion may eaſily enough be made ſtick together, as much as in our caſe the tube and finger do, notwithſtanding one of them is anointed with oil, and that this adheſion of the finger to the tube is to be met with in caſes where the ſurface of the included quickſilver is not contiguous to the finger, but many inches below. As for what he adds concerning the reaſon, why water and quickſilver aſcend by ſuction, we have already taught what is to be anſwered to it, by aſcribing that aſcenſion to the preſſure of the external air; without any need of having recourſe to a Funiculus: or imagining with him in this place, that becauſe nothing beſides the water or quickſilver can in ſuch caſes ſucceed the air (which yet Page 40. is not eaſy to be proved in reference to a thin æthereal ſubſtance) therefore, *Partes ipſius aëris* (to uſe his expreſſion) *ſic tubo incluſæ (quæ aliàs tam facile ſeparantur) nunc tam fortiter ſibi invicem agglutinentur, ut validiſſimam (uti videmus) conficiunt catenam, qua non ſolum aqua, ſed ponderoſum illud argentum ſic in altum trahatur:* ‘ That the parts ‘ of air itſelf ſo ſhut up in the tube (which otherwiſe are ſo eaſily ſevered) are now ſo ‘ firmly glued to one another, that they make (as we ſee) a ſtrong chain, by which ‘ not only water but even weighty quickſilver is drawn on high.’ Which way of wreathing a little rarefied air into ſo ſtrong a rope, how probable it is, I will for a while leave the reader to judge, and advance to our author's ſecond notandum, which he thus propoſes:

Page 40, 41. *Rarefactionem ſive extenſionem corporis ad occupandum majorem locum fieri non ſolo calore, ſed etiam diſtenſione ſeu vi divulſivâ; ſicut è contra condenſatio non ſolo frigore perficitur, ſed etiam compreſſione, uti innumera paſſim docent exempta:* ‘ That the rarefac‘ tion or extenſion of a body, ſo as to make it take up more ſpace, is not only made ‘ by heat, but by diſtenſion or a certain disjoining power; as, on the contrary, con‘ denſation is not only made by cold, but alſo by compreſſion, as infinite examples ‘ bear us witneſs.’ And it is true and obvious, that the condenſation of bodies, taking that word in a large ſenſe, may be made as well by compreſſion as cold. But I wiſh he had more clearly expreſſed what he means in this place by that rarefaction, which he ſays is to be made by diſtenſion, or a *vis divulſiva,* whereof he tells us there are innumerable inſtances. For, as far as may be gathered from the three examples he ſubjoins, it is only the air, that is capable of being ſo extended as his hypotheſis requires quickſilver and even ſtones muſt be. And I know not how it will be proved, that even air may be thus extended ſo far, as in the *Magdeburg* experiment, to fill a place more than two thouſand times as big as that it filled before. For that the ſame air in this and his two foregoing inſtances does adequately fill more ſpace at one time than another, he proves but by the ruſhing in of water into the evacuated glaſs, and filling it within a little quite full, which, he ſays, is done by the diſtended air, that contracting itſelf draws up the water with it. Which explication how much leſs likely it is, than that the water is in ſuch caſes impelled up by the preſſure of the atmoſphere, we ſhall anon (when we come to diſcuſs his way of rarefaction and condenſation) have occaſion to examine. In the mean time let us conſider with him the explication, which, after having promiſed the two above recited obſervations, he gives us of his Funiculus; Page 41. *Cum per primum notandum argentum ita adhæreat tubi vertici, & per ſecundum, rarefactio fiat per meram corporis diſtenſionem, ita rem ſe habere, ut argentum deſcendens à vertice tubi affixam ei relinquat ſuperficiem ſuam extimam ſive ſupremam, eamque eouſque ſuo pondere extendat extenuetque, donec facilius ſit aliam ſuperficiem ſimiliter relinquere quam priorem illam ulterius extendere: ſecundam igitur relinquit, eamque eodem modo deſcendendo extendit, donec facilius ſit tertiam adhuc ſeparari quam illam ſecundam extendere ulterius; & ſic deinceps, donec tandem vires amplius non habeat ſuperficies ſic ſeparandi & extendendi; nempe donec perveniat ad altitudinem digitorum duntaxat* 29½ *ubi quieſcit, ut capite primo dictum eſt.* 'Seeing by the firſt note it is manifeſt, that 'the quickſilver doth ſo ſtick to the top of the tube, and by the ſecond note the 'rarefaction is made only by the mere diſtenſion of the body; it ſo comes to paſs, 'that the deſcending quickſilver leaves its external or upper ſuperficies fixed unto the 'top of the tube, and by its weight doth ſo ſtretch and extenuate it, until it becomes 'eaſier to leave another ſuperficies in like manner, than to extend that any further. 'It leaves therefore a ſecond, and doth by its deſcent extend that a little further, 'until it becomes eaſier to ſeparate a third than to extend that any further; and ſo 'forwards, until at length it hath no power to ſeparate or extend any more ſuper'ficies, namely, until it comes unto the height of 29 inches and an half; where it 'acquieſces, as we have declared in the firſt chapter.' Thus far our examiner's explication: by which it is eaſy to diſcern, that he is fain to aſſign his Funiculus a way of being produced ſtrange and unparallelled enough. For, not to repeat our animadverſions upon the firſt of the two notandums, on which the explication is grounded, I muſt demand by what force, upon the bare ſeparation of the quickſilver and the top of the tube, the new body he mentions comes to be produced; or at leaſt how it appears, that the mercury leaves any ſuch thing as he ſpeaks of behind it. For the ſenſe perceives no ſuch matter at the top of the tube, nor is it neceſſary to explicate the phænomena as we have formerly ſeen. It may alſo be marvelled at, that the bare weight of the deſcending mercury ſhould be able to extend a ſurface into a body. And beſides, it ſeems precariouſly affirmed, that there is ſuch a ſucceſſive leaving behind of one ſurface after another as is here imagined: nor does it appear how, though ſome of the quickſilver were turned into a thin ſubtile ſubſtance, yet that ſubſtance comes to be contrived into a Funiculus of ſo ſtrange a nature, that ſcarce any weight (for aught appears by his doctrine) can be able to break it; that contrary to all other ſtrings it may be ſtretched without being made more ſlender; and that it has other very odd properties, ſome of which we ſhall anon have occaſion to mention. As for what our author ſubjoins in theſe words, Page 43, 44. *Eodem itaque ferè modo ſeparari videntur hæ ſuperficies ab argento deſcendente, & in tenuiſſimum quendam funiculum per deſcendens pondus extendi, quo per calorem in accenſa candela ſeparantur hujuſmodi ſuperficies à ſubjecta cera aut ſevo, & in ſubtiliſſimam flammam extenuantur. Ubi notatu dignum, quemadmodum flamma illa pluſquam millies ſine dubio majus ſpatium occupat, quam antea occupaverat pars illa ceræ, ex qua conficitur; ita prorſus & hic exiſtimandum, Funiculum illum pluſquam millies majus ſpatium occupare, quàm prius occupaverat illa*

argenti particula, ex qua ſit exortus: uti etiam ſine dubio contingit, quando talis particula à ſubjecto igne in vaporem convertitur. 'Theſe ſurfaces ſeem to be ſeparated from the quickſilver, and to be extended into a moſt ſlender ſtring by the weight that falls down, after the ſame manner that in a lighted candle ſurfaces of like ſort are ſeparated from the wax or tallow underneath by the heat above, and are extenuated into a moſt ſubtile flame. In which it is worth obſervation, that as that flame doth doubtleſs take up more than a thouſand times a greater ſpace than the part of the wax, of which the flame was made, took up; ſo is it here to be thought, that that ſtring doth take up a ſpace more than a thouſand times as big as that, which the ſmall particle of mercury, from whence it aroſe, did before take up. As alſo it doubtleſs happens, when ſuch a particle by a fire underneath is turned into a vapour.' Though it be the only example, whereby he endeavours to illuſtrate the generation of his Funiculus, yet (I preſume) he ſcarce expects we ſhould think it an appoſite one. For beſides that there here intervenes a conſpicuous and powerful agent, namely, an actual fire to ſever and agitate the parts of the candle; and beſides that there is a manifeſt waſting of the wax or tallow turned into flame; beſides theſe things, I ſay, we muſt not admit, that the fuel, when turned into a flame, does really fill (I ſay, not, with our author) more than a thouſand times, but ſo much as twice more of genuine ſpace than the wax it was made of. For it may be ſaid, that the flame is little or nothing elſe than an aggregate of thoſe corpuſcles, which before lay upon the upper ſuperficies of the candle, and by the violent heat were divided into minuter particles, vehemently agitated and brought from lying as it were upon a flat to beat off one another, and make up about the wick ſuch a figure, as is uſual in the flame of candles burning in the free air. Nor will it neceſſarily follow, that the ſpace, which the flame ſeems to take up, ſhould contain neither air nor æther, nor any thing elſe, ſave the parts of that flame, becauſe the eye cannot diſcern any other body there: for even the ſmoke aſcending from the ſnuff of a newly-extinguiſhed candle appears a dark pillar, which to the eye at ſome diſtance ſeems to conſiſt of ſmoke; when as yet there are ſo many aërial and other inviſible corpuſcles mingled with it, as if all thoſe parts of ſmoke, that make a great ſhow in the air, were collected and contiguous, they would not perhaps amount to the bigneſs of a pin's head, as may appear by the great quantity of ſtreams, that in chymical veſſels are wont to go to the making up of one drop of ſpirit. And therefore it does not ill fall out for our turn, that the examiner, to inforce his former example, alleges the turning of a particle of quickſilver into vapour, by putting fire under it: for if ſuch be the rarefaction of mercury, it is not at all like to make ſuch a Funiculus as he talks of, ſince thoſe mercurial fumes appear by divers experiments to be mercury divided and thrown abroad into minute parts, whereby though the body obtain more of ſurface than it had before, yet it really fills no more of true and genuine ſpace; ſince if all the particular little ſpaces filled by theſe ſcattered corpuſcles were reduced into one (as the corpuſcles themſelves often are in chymical operations) they would amount but to one total ſpace, equal to that of the whole mercury before rarefaction. But theſe objections againſt this explication are not all that I have to ſay againſt our adverſary's Funiculus itſelf.

For I farther demand, how the Funiculus comes by ſuch hooks or grapple-irons, or parts of the like ſhape, to take faſt hold of all contiguous bodies, and even the ſmootheſt, ſuch as glaſs, and the calm ſurface of quickſilver, water, oil, and other fluids: and how theſe ſlender and inviſible hooks cannot only in the terſeſt bodies find an innumerable company of ears or loops to take hold on, but hold ſo ſtrongly, that they are able not alone to lift up a tall cylinder of that very ponderous metal of quickſilver, but to draw inwards the ſides of ſtrong glaſſes ſo forcibly, as to break them all to pieces. And it is alſo ſomewhat ſtrange, that water and other fluid bodies (whoſe parts are wont to be ſo eaſily ſeparable) ſhould, when the Funiculus once lays hold on the ſuperficial corpuſcles, preſently emulate the nature of conſiſtent bodies, and be drawn up like maſſes each of them of an intire piece; though even in the exhauſted receiver they appear by their undulation (when they are ſtirred by bubbles that paſs freely through them) and many other ſigns to continue fluid bodies.

IT ſeems alſo very difficult to conceive, how this extenuated ſubſtance ſhould acquire ſo ſtrong a ſpring inward, as the examiner all along his books aſcribes to it. Nor will it ſerve his turn to require of us in exchange an explication of the air's ſpring outward, ſince he acknowledges, as well as we, that it has ſuch a ſpring. I know, that by calling this extenuated ſubſtance a Funiculus, he ſeems plainly to intimate, that it has its ſpring inward, upon the ſame account that lute-ſtrings and ropes forcibly ſtretched have theirs. But there is no ſmall diſparity betwixt them: for whereas in ſtrings there is required either wreathing, or ſome peculiar and artificial texture of the component parts; and a rarefaction of air (were it granted) does not include or infer any ſuch contrivance of parts as is requiſite to make bodies elaſtical. And if the Carteſian notion of the cauſe of ſpringineſs be admitted, then our extenuated ſubſtance having no pores to be pervaded by the *materia ſubtilis* (to which beſides our author alſo makes glaſs impervious) will be deſtitute of ſpringineſs. And however, ſince lute-ſtrings, ropes, *&c.* muſt, when they ſhrink inwards, either fill up or leſſen their pores, and increaſe in thickneſs as they diminiſh in length; our examiner's Funiculus muſt differ very much from them, ſince it has no pores to receive the ſhrinking parts, and contracts itſelf as to length, without increaſing its thickneſs. Nor can it well be pretended, that this ſelf-contraction is done *ob fugam vacui*, ſince though it ſhould not be made, a vacuum would not enſue. And if it be ſaid, that it is made, that the præternaturally ſtretched body might reſtore itſelf to its natural dimenſions; I anſwer, that I am not very forward to allow acting for ends to bodies inanimate, and conſequently devoid of knowledge; and therefore ſhould gladly ſee ſome unqueſtionable examples produced of operations of that nature. And however to me, who in phyſical inquiries of this nature look for efficient rather than final cauſes, it is not eaſy to conceive, how air, by being expanded (in which caſe its force, like that of other rarefied bodies, ſeems principally to tend outwards, as we ſee in fired gunpowder, in æolipiles, in warmed weather-glaſſes, *&c.*) ſhould acquire ſo prodigious a force of moving contiguous bodies inwards. Nor does it to me ſeem very probable, that, when for inſtance part of a poliſhed marble is extended into a Funiculus, that Funiculus does ſo ſtrongly aſpire to turn into marble again. I might likewiſe wiſh our author had more clearly explicated, how it comes to paſs (which he all along takes for granted) that the acceſs of the outward air does ſo much and ſo ſuddenly relax the tenſion of his Funiculus; ſince that being (according to him) a real and poreleſs body, it is not ſo obvious, how the preſence of another can ſo eaſily and to ſo ſtrange a degree make it ſhrink. But I will rather obſerve, that it is very unlikely, that the ſpace, which our adverſary would have repleniſhed with his funicular ſubſtance, ſhould be full of little highly ſtretched ſtrings, that lay faſt hold of the ſurfaces of all contiguous bodies, and always violently endeavour to pull them inwards. For we have related in our 26th experiment, that a pendulum being ſet a moving in our exhauſted receiver, did ſwing to and fro as freely, and with the ſtring ſtretched as ſtreight, as, for aught we could perceive, it would have done in the common air. Nay, the balance of a watch did there move freely and nimbly to and fro; which it is hard to conceive thoſe bodies could do, if they were to break through a medium conſiſting of innumerable exceedingly ſtretched ſtrings. On which occaſion we might add, that it is ſomewhat ſtrange, that theſe ſtrings, thus cut or broken by the paſſage of theſe bodies through them, could ſo readily have their parts re-united, and without any more ado be made intire again. And we might alſo take notice of this as another ſtrange peculiarity in our author's Funiculus, that in this caſe the two divided parts of each ſmall ſtring, that is broken, do not, like thoſe of other broken ſtrings, ſhrink and fly back from one another; but (as we juſt now ſaid) immediately redintegrate themſelves: whereas, when in the Torricellian experiment the tube and contained mercury is ſuddenly lifted up out of the reſtagnant quickſilver into the air, the Funiculus does ſo ſtrangely contract itſelf, that it quite vaniſhes: inſomuch that the aſcending mercury may riſe to the very top of the tube. Theſe, I ſay, and divers other difficulties might on this occaſion be inſiſted on; but that, ſuppoſing ourſelves to have mentioned enough of them for once, we think it now more ſeaſonable to proceed to the remaining part of our diſcourſe.

CHAP. III.

The Ariſtotelian Rarefaction (propoſed by the adverſary) examined.

BUT this is not all, that renders the examiner's hypotheſis improbable: for, beſides thoſe already mentioned particulars, upon whoſe ſcore it is very difficult to be underſtood; it neceſſarily ſuppoſes ſuch a rarefaction and condenſation, as is, I confeſs, to me, as well as to many other conſidering perſons, unintelligible.

FOR the better diſcernment of the force of this objection, we muſt briefly premiſe, that a body is commonly ſaid to be rarefied or dilated, (for I take the word in a larger ſenſe than I know, many others do, for a reaſon that will quickly appear) when it acquires greater dimenſions than the ſame body had before; and to be condenſed, when it is reduced into leſs dimenſions, that is, into a leſſer ſpace than it contained before; as when a dry ſpunge being firſt dipped in water ſwells to a far greater bulk, and then being ſtrongly ſqueezed and held compreſſed, is not only reduced into leſs room than it had before it was ſqueezed, but into leſs than it had even before it was wetted. And I muſt further premiſe, that rarefaction (as alſo condenſation) being amongſt the moſt obvious phænomena of nature, there are three (and for aught we know but three) ways of explicating it: for, either we muſt ſay with the Atomiſts and Vacuiſts, that the corpuſcles, whereof the rarefied body conſiſts, do ſo depart from each other, that no other ſubſtance comes in between them to fill up the deſerted ſpaces that come to be left betwixt the incontiguous corpuſcles; or elſe we ſay with divers of the ancient philoſophers, and many of the moderns, eſpecially the Carteſians, that theſe new intervals produced betwixt the particles of the rarefied body are but dilated pores, repleniſhed, in like manner as thoſe of the tumid ſpunge are, by the imbibed water, by ſome ſubtile æthereal ſubſtance, that inſinuates itſelf betwixt the disjoined particles: or, laſtly, we muſt imagine, with *Ariſtotle* and moſt of his followers, that the ſelf-ſame body does not only obtain a greater ſpace in rarefaction, and a leſſer in condenſation, but adequately and exactly filled it, and ſo when rarefied acquires larger dimenſions without either leaving any vacuities betwixt its component corpuſcles, or admitting between them any new or extraneous ſubſtance whatſoever.

Now it is to this laſt (and, as ſome call it, rigorous) way of rarefaction, that our adverſary has recourſe in his hypotheſis; though this, I confeſs, appears to me ſo difficult to be conceived, that I make a doubt whether any phænomenon can be explained by it; ſince to explain a thing is to deduce it from ſomething or other in nature more known than itſelf.

He that would meet with full diſcuſſions of this Ariſtotelian rarefaction, may reſort to the learned writings of *Gaſſendus*, *Carteſius* and *Maignan*, who have accuſed it of divers great abſurdities: but for my part, I ſhall at preſent content myſelf to make uſe, to my purpoſe, of two or three paſſages, that I meet with (though not together) in our author himſelf.

Let us then ſuppoſe, that in the *Magdeburgh* experiment he ſo often (though I think cauſleſsly enough) urges to prove his hypotheſis; let us (I ſay) for eaſier conſideration's ſake ſuppoſe, that the undilated air, which (as he tells us) poſſeſſed about half an inch of ſpace, conſiſted of an hundred corpuſcles, or (if that be in this caſe diſliked) a hundred parts; (for it matters not what number we pitch upon) and it will not be denied, but as the whole parcel of air, or the aggregate of this hundred corpuſcles, is adequate to the whole ſpace it fills, ſo each of the hundred parts, that make it up, is likewiſe adequately commenſurate to its peculiar ſpace, which we here ſuppoſe to be a hundred part of the whole ſpace. This premiſed, our author having elſewhere this paſſage, *Corpore occupante locum verbi gratia duplo majorem, neceſſe eſt, ut quælibet ejus pars locum quoque duplo majorem occupet*; 'A body 'taking up a place, for inſtance, twice as big as itſelf; it is of neceſſity that every 'part of it muſt likewiſe take up a place twice as big as itſelf:' prompts us to ſubjoin, that in the whole capacity of the globe (which according to him was two thouſand times as great as the room poſſeſſed by the unexpanded air) there muſt likewiſe be two hundred thouſand parts of ſpace commenſurate each of them to one of the forementioned hundredth parts of air; and conſequently, when he affirms, that that half-inch of air poſſeſſed the whole cavity of the globe, if we will not admit (as he does not) either vacuities, or ſome intervening ſubtile ſubſtance in the interval of the aërial parts, he muſt give us leave to conclude, that each part of air does adequately fill two thouſand parts of ſpace. Now that this ſhould be reſolutely taught to be not only naturally poſſible (for we diſpute not here of what the divine Omnipotence can do) but to be really and regularly done in this *Magdeburgh* experiment, will queſtionleſs appear very abſurd to the Carteſians and thoſe other philoſophers, who take extenſion to be but notionally different from body, and conſequently impoſſible to be acquired or loſt without the addition or detraction of matter; and will, I doubt not, appear ſtrange to thoſe other readers, who conſider how generally naturaliſts have looked upon extenſion as inſeparable, and as immediately flowing from matter; and upon bodies, as having neceſſary relation to a commenſurate ſpace. Nor do I ſee, if one portion of air may ſo eaſily be brought exactly to fill up a ſpace two thouſand times as big as that, which it did but fill before without the addition of any new ſubſtance; I ſee not (I ſay) why the matter contained in every of theſe two thouſand parts of ſpace may not be further brought to fill two thouſand more, and ſo onwards, ſince each of theſe newly-repleniſhed ſpaces is preſumed to be exactly filled with body, and no ſpace, nor conſequently that which the unrarefied air repleniſhed, can be more than adequately full. And ſince, according to our adverſary, not only fluid bodies, as air and quickſilver, but even ſolid and hard ones, as marble, are capable of ſuch a diſtenſion as we ſpeak of, why may not the world be made I know not how many thouſand times bigger than it is, without either admitting any thing of vacuity betwixt its parts, or being increaſed with the addition of one atom of new matter? Which to me is ſo difficult to conceive, that I have ſometimes doubted, whether, in caſe it could be proved, that in the exhauſted globe

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we ſpeak of, there were no vacuities within, nor any ſubtile matter permitted to enter from without, it were not more intelligible to ſuppoſe, that God had created a new matter to join with the air in filling up the cavity, than that the ſelf-ſame air ſhould adequately fill two thouſand ſpaces, whereof one was exactly commenſurate to it, even when it was uncompreſſed. For divers eminent Naturaliſts, both ancient and modern, believing, upon a phyſical account, the ſouls of men to be created and infuſed, will admit it as intelligible, that God does frequently create ſubſtances on certain emergent occaſions. But I know that many of them will not likewiſe think it conceivable, that without his immediate interpoſition an acceſſion of new, real dimenſions ſhould be had without either vacuities or acceſſion of matter.

And indeed when I conſidered theſe difficulties and others, that attend the rarefaction our examiner throughout his whole book ſuppoſes, and when I found that ever and anon he remits us to what he teaches concerning rarefaction; I could not but with ſome greedineſs reſort to the chapters he addreſſed me to. But when I had peruſed them, I found the difficulties remained ſuch ſtill, and that it was very hard even for a witty man to make more of a ſubject than the nature of it does bear. Which I ſay, that by profeſſing myſelf unſatisfied with what he writes, I may not be thought to find fault with a man for not doing what perhaps is not to be done, and for not making ſuch abſtruſe notions plain, as are ſcarcely (if at all) ſo much as intelligible. And indeed as he has handled this ſubject modeſtly enough, ſo in ſome places his expreſſions are to me ſomewhat dark; which I mention, not to impute it as a crime in him, that he wrote in a diffident and doubtful ſtrain of ſo difficult a matter; but to excuſe myſelf, if I have not always gueſſed aright at his meaning.

The things he alleges, in favour of the rarefaction he would perſuade, are two: the one, that the phænomena of rarefaction cannot be explicated either by vacuities or the ſubingreſſion of an æthereal ſubſtance; and the other, that there are two ways of explicating the rigorous rarefaction he contends for.

His objections againſt the Epicurean and Carteſian ways of making out rarefaction are ſome of them more plauſible than moſt of thoſe, that are wont to be urged againſt them; yet not ſuch as are not capable enough of anſwers. But whilſt ſome of the paſſages appeared eaſy to be replied to by the favourers of the hypotheſis they oppoſe, before I had fully examined the reſt, chancing to mention theſe chapters to an ingenious man, hereafter to be further mentioned in this treatiſe, he told me he had ſo far conſidered them more than the reſt of the book, that he had thought upon ſome hypotheſes, whereby the phænomena of rarefaction might be made out either according to the Vacuiſts, or according to the Carteſians, adding, that he had alſo examined the inſtance our adverſary pretends to be afforded him of his rarefaction by what happens in the *Rota Ariſtotelica*. Wherefore being ſufficiently diſtreſſed by avocations of ſeveral ſorts, and being willing to reſerve the declaration of my own thoughts concerning the manner of rarefaction and condenſation for another treatiſe, I ſhall refer the reader to the ingenious conjectures about this ſubject, which the writer of them intends to annex to the preſent diſcourſe; and only add in general, that whereas the examiner's argument on this occaſion is, that his way of rarefaction muſt be admitted, becauſe neither of the other two can be well made out; his adverſaries may with the ſame reaſon argue, that one of theirs is to be allowed, ſince his is incumbered with ſuch manifeſt difficulties. And they may enforce what they ſay by repreſenting, that the inconveniences, that attend his hypotheſis about rarefaction, are inſuperable, ariſing from the unintelligible nature of the thing itſelf; whereas thoſe, to which the other ways are obnoxious, may ſeem to ſpring but from men's not having yet diſcovered, what kind of figures and motions of the ſmall particles may beſt qualify them to make the body, that conſiſts of them, capable of a competent expanſion.

AFTER our author's objections againſt the two ways of rarefaction propoſed, the one by the Vacuiſts, and the other by the Carteſians and others, that admit the ſolideſt bodies, and even glaſs itſelf, to be pervious to an æthereal or ſubtile matter; he attempts to explicate the manner, by which that rigorous rarefaction he teaches is performed: and having premiſed, that the explication of the way, how each part of the rarefied body becomes extended, depends upon the quality of the parts, into which the body is ultimately reſolved; and having truly obſerved, that they muſt neceſſarily be either really indiviſible, or ſtill endleſsly diviſible; he endeavours to explicate the Ariſtotelian rarefacton according to thoſe two hypotheſes. But, though he thus propoſe two ways of making out his rarefaction; yet beſides that they are irreconcilable, he ſpeaks of them ſo darkly and doubtfully, that it ſeems leſs eaſy to diſcern, which of the two he would be content to ſtick to, than that he himſelf ſcarce acquieſces in either of them.

AND, firſt, having told us, how rarefaction may be explained, in caſe we admit bodies to be diviſible *in infinitum*, he does himſelf make ſuch an objection againſt the infinity of parts in a *continuum*, as he is fain to give ſo obſcure an anſwer to, that I confeſs I do not underſtand it; and preſume, that not only the moſt part of unprejudiced readers will as little acquieſce in the anſwer as I do; but even the author himſelf will not marvel at my confeſſion, ſince in the ſame place he acknowledges the anſwer to be ſomewhat obſcure, and endeavours to excuſe its being ſo, becauſe in that hypotheſis it can ſcarce be otherwiſe.

WHEREFORE I ſhall only add on this occaſion, that it is not clear to me, that even ſuch a diviſibility of a *continuum* as is here ſuppoſed would make out the rarefaction he contends for. For, let the integrant parts of a *continuum* be more or leſs finite or infinite in number, yet ſtill each part, being a corporeal ſubſtance, muſt have ſome particle of ſpace commenſurate to it; and if the whole body be rarefied, for inſtance, to twice its former bigneſs, then will each part be likewiſe extended to double its former dimenſions, and fill both the place it took up before, and another equal to it, and ſo two places.

THE ſecond argument alleged to recommend the hitherto-mentioned way of explicating rarefaction is, that many learned men, amongſt whom he names two, *Aquinas* and *Suarez*, have taught, that the ſame corporeal thing may naturally be, and *de facto* often is, in the ſouls of brutes really indiviſible and virtually extended. But, though I pay thoſe two authors a juſt reſpect for their great ſkill in ſcholaſtical and metaphyſical learning; yet the examiner cannot ignore, that I could make a long catalogue of writers, both ancient and modern, at leaſt as well verſed in natural philoſophy as Saint *Thomas* and *Suarez*, who have ſome of them in expreſs words denied this to be naturally poſſible; and others have declared themſelves of the ſame judgment by eſtabliſhing principles, with which this conceit of the virtual extenſion of the indiviſible corpuſcles is abſolutely inconſiſtent. And though no author had hitherto oppoſed it, yet I, that diſpute not what this or that man thought, but what it is rational to think, ſhould neverthelеſs not ſcruple to reject it now; and ſhould not doubt to find ſtore of the beſt Naturaliſts of the ſame opinion with me, and perhaps among them the examiner himſelf, who (however this acknowledgment may agree with the three following chapters of his book) tells us (pag. 160.) that *juxta probabiliorum ſententiam hujuſmodi virtualis extenſio rei corporeæ concedenda non eſt, utpote ſoli rei ſpirituali propria:* 'According to the more probable opinion, ſuch a virtual extenſion

‘ tenſion of a corporeal being is not to be granted, as being only proper to ſuch as ‘ are ſpiritual.’

BUT to conclude at length this tedious inquiry into the Ariſtotelian way of rarefaction (which is of ſo obſcure a nature, that it can ſcarce be either propoſed or examined in few words) I will not take upon me reſolutely to affirm, which of the two ways of explicating it (by atoms or by parts infinitely diviſible) our author declares himſelf for. But which of them ſoever it be, I think I have ſhown, that he has not intelligibly made it out: and I make the leſs ſcruple to do ſo, becauſe he himſelf is ſo ingenuous as (at the cloſe of his diſcourſe of the two ways) to ſpeak Page 169. thus of the opinion he prefers; *Præſtat communi & receptæ hactenus in ſcholis ſententiæ inſiſtere, quæ licet difficultates quidem non clarè ſolvat, iis tamen apertè non ſuccumbit:* ‘ It is better to continue in the common opinion, which hath been hitherto received ‘ in the ſchools; which although it doth not clearly reſolve all difficulties, yet it doth ‘ not openly lie under them.’ So that in this diſcourſe of rarefaction, to which our author has ſo often in the foregoing part of the book referred us, as that which ſhould make good what there ſeemed the moſt improbable; he has but, inſtead of a probable hypotheſis needleſsly rejected, ſubſtituted a doctrine, which himſelf dares not pretend capable of being well freed from the difficulties, with which it may be charged; though I doubt not but other readers, eſpecially Naturaliſts, will think he has been very civil to this obſcure doctrine, in ſaying that *difficultatibus non apertè ſuccumbit.*

As for the other way of explicating rarefaction, namely, by ſuppoſing that a body is made up of parts indiviſible; he will not, I preſume, deny, but that the objections we formerly made againſt it are weighty. For according to this hypotheſis, (which one would think he prefers, ſince he makes uſe of it in the three or four laſt Page 163. chapters of his book) *Neceſſariò fatendum eſt* (ſays he) *unam eandemque partem poni in duplici loco adæquate: cum enim indiviſibilis ſit, locumque occupet majorem quam prius, neceſſe eſt, ut tota ſit in quolibet puncto totius loci, ſive ut per totum illud ſpatium virtualiter extendatur:* ‘ We muſt needs confeſs, that one and the ſame part muſt be in two ‘ places adequately. For ſeeing it is indiviſible, and takes up a greater place than ‘ before, it muſt of neceſſity be all in every point of that place, or that be virtually ‘ extended through all that ſpace.’ So that when he in the very next page affirms, that by this virtual extenſion of the parts, the difficulties, that have for ſo many ages troubled philoſophers, may be eaſily ſolved, he muſt give me leave (who love to ſpeak intelligibly, and not to admit what I cannot underſtand) to deſire he would explain to me what this *extenſio virtualis* is, and how it will remove the difficulties that I formerly charged upon the Ariſtotelian rarefaction. For the eaſier conſideration of this matter, let us reſume what we lately ſuppoſed; namely, that in the Magdeburgic experiment the half-inch of undilated air conſiſted of a hundred corpuſcles: I demand how the indiviſibility of theſe corpuſcles will qualify them to make out ſuch a rarefaction as the author imagines. For what does their being indiviſible do in this caſe, but make it the leſs intelligible, how they can fill above a hundred parts of ſpace? It is eaſy to foreſee he will anſwer, that they are virtually extended. But not here to queſtion, how their indiviſibility makes them capable of being ſo; I demand, whether by an atom's being virtually extended, its corporeal ſubſtance do really (I mean adequately) fill more ſpace than it did before, or whether it do not: (for one of the two is neceſſary.) If it do, then it is a true and real, and not barely a virtual extenſion. And that ſuch an extenſion will not ſerve the turn, what we have formerly argued againſt the Peripatetic rarefaction will evince; and our adverſary ſeems to confeſs as much, by deviſing this virtual extenſion to avoid the inconveniences, to which he ſaw his doctrine of rarefaction would otherwiſe plainly appear expoſed. But if it be ſaid, that when an atom is virtually extended, its corporeal ſubſtance fills no more ſpace than before; this is but a verbal ſhift, that may perhaps amuſe an unwary reader, but it will ſcarce ſatisfy a conſidering one. For I demand how that which is not a ſubſtance can fill place; and how this improper and but metaphorical extenſion will ſolve the phænomena of rarefaction: as how the half-inch of air at the top of the forementioned globe ſhall without a corporeal extenſion fill the whole globe of two thouſand times its bigneſs, when the water is ſucked out of it, and act at the lower part of the globe. Which laſt clauſe I therefore add, becauſe not only our author teaches (pag. 91, and 92.) that the whole globe was filled with a certain thin ſubſtance, which by its contraction violently ſnatched up the water into which the neck of the glaſs was immerſed; but in a parallel caſe he makes it his grand argument to prove, that there is no vacuum in the deſerted part of the tube in the Torricellian experiment, that the attraction of the finger cannot be performed but by ſome real body. *Chap. 5.* Wherefore till the examiner does intelligibly explain, how a virtual extenſion, as it is oppoſed to a corporeal, can make an atom fill twice, nay two thouſand times more ſpace than it did before; I ſuppoſe this device of virtual extenſion will appear to unbiaſſed Naturaliſts but a very unſatisfactory evaſion.

Two arguments indeed there are, which our adverſary offers as proofs of what he teaches. The firſt is, that they commonly teach in the ſchools, that at leaſt *divinitus* (as he ſpeaks) ſuch a thing as is pleaded for may be done, and that conſequently it is not repugnant to the nature of a body. But, though they that either know me, or have read what I have written about matters theological, will, I hope, readily believe, that none is more willing to acknowledge and venerate Divine Omnipotence; yet in ſome famous ſchools they teach, that it is contrary to the nature of the thing. And that men, who think ſo, and conſequently look not upon it as an object of Divine Omnipotence, may (whatever he here ſay) without impiety be of a differing mind from him about the poſſibility of ſuch a rarefaction as he would here have, our author may, perchance, think fit to grant, if he remembers that he himſelf ſays a few pages after, *Cum tempus ſit Ens eſſentialiter ſucceſſivum, ita ut ne divinitus quidem poſſint duæ ejus partes ſimul exiſtere, &c.* *Page 175.* 'Seeing time is a being eſſentially ſucceſſive, ſo that neither by divine power can two of its parts exiſt together.' But, not now to diſpute of a power, that I am more willing to adore than queſtion, I ſay, that our controverſy is not what God can do, but about what can be done by natural agents, not elevated above the ſphere of nature. For though God can both create and annihilate, yet nature can do neither: and in the judgment of true philoſophers, I ſuppoſe our hypotheſis would need no other advantage, to make it be preferred before our adverſaries, than that in ours things are explicated by the ordinary courſe of nature, whereas in the other recourſe muſt be had to miracles.

BUT, though our author's way of explicating rarefaction be thus improbable, yet I muſt not here omit to take notice, that his Funiculus ſuppoſes a condenſation, that to me appears incumbered with no leſs manifeſt difficulties. For, ſince he teaches, that a body may be condenſed without either having any vacuities for the compreſt parts to retire into, or having pores filled with any ſubtile and yielding matter, that may be ſqueezed out of them; it will follow, that the parts of the body to be condenſed do immediately touch each other: which ſuppoſed, I demand how bodies, that are already contiguous, can be brought to farther approximations without penetrating each other, at leaſt in ſome of their parts. So that I ſee not how the examiner's condenſation can be performed without penetration of dimenſions; a thing that philoſophers of all ages have looked upon as by no means to be admitted in nature. And our author himſelf ſpeaks ſomewhere at the ſame rate, where to the queſtion, Why the walls that incloſe fired gun-powder muſt be blown aſunder? *Reſpondeo* (ſays he) *hæc omnia inde accidere, quod pulvis ille ſic accenſus & in flammam converſus, longe majus ſpatium nunc occupet quàm prius. Unde fit, ut cum totum cubiculum antea fuerit pleniſſimum, diſrumpantur ſic parietes, ne detur corporum penetratio.* 'I 'anſwer, that all theſe things happen becauſe the gun-powder ſo kindled and turned 'into flame takes up a much greater ſpace than before. Whence it comes to paſs, 'that ſeeing the chamber was before quite full, by this means the walls are broken, 'that there may be no penetration of bodies.' In the Magdeburgic experiment he tells us (as we have heard already) that the whole capacity of the globe is filled with an extremely thin body. But not now to examine how properly he calls that a rare body, which according to him intercepts neither pores nor any heterogeneous ſubſtance, the greater or leſſer abſence of which makes men call a body more or leſs denſe; not to inſiſt on this, I ſay, let us conſider, that before the admiſſion of water into the exhauſted globe, there was, according to him, two thouſand half-inches of a ſubſtance, which, however it was produced or got thither, was a true and real body; and that after the admiſſion of the water, there remained in the ſame globe, beſides the water that came in, no more than one half inch of body. Since then our author does not pretend (which if he did, might be eaſily diſproved) that one thouſand nine hundred ninety-nine half-inches of matter, that now appear no more, traverſed the body of water; ſince he will not allow, that it gets away through the pores of the glaſs, I demand, what becomes of ſo great a quantity of matter? For that it is annihilated, I ſuppoſe he is too rational a man to pretend (nor, if he ſhould, would it be at all believed); and to ſay, that a thouſand and ſo many hundred parts of matter ſhould be retired into that one part of ſpace, that contains the one half inch of air, is little leſs incredible: for that ſpace was ſuppoſed perfectly full of body before, and how a thing can be more than perfectly full, who can conceive? To diſpatch: according to our author's way of condenſation, two, or perhaps two thouſand, bodies may be crouded into a ſpace, that is adequately filled by one of them apart. And if this be not penetration of dimenſions, I deſire to be informed what is ſo; and till then I ſhall leave it to any unprepoſſeſſed Naturaliſt to judge, whether an hypotheſis that needs ſuppoſe a thing ſo generally concluded to be impoſſible to nature, be probable or not; and whether to tell us, that the very ſame parcel of air, that is now without violence contained in half an inch of ſpace, ſhall by and by fill two thouſand times as much room, and preſently after ſhrink again into the two thouſandth part of the ſpace it newly poſſeſſed, be not to turn a body into a ſpirit, and, confounding their notions, attribute to the former the diſcriminating and leaſt eaſily conceivable properties of the latter. And this argument is, I confeſs, with me of that weight, that this alone would keep me from admitting the examiner's hypotheſis: yet if any happier contemplator ſhall prove ſo ſharp-ſighted, as to deviſe and clearly propoſe a way of making the rarefaction and condenſation, hitherto argued againſt, intelligible to me, he is not like to find me obſtinate. Nor indeed is there ſufficient cauſe, why his ſucceeding in that attempt ſhould make our adverſaries hypotheſis preferable to ours, ſince that would not prove it either neceſſary, or ſo much as ſufficient, but only anſwer ſome of the arguments, that tend to prove it is not intelligible. And that we have other arguments on our ſide than thoſe that relate to rarefaction and condenſation, may appear partly by what has been diſcourſed already, and partly by what we have now to ſubjoin.

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CHAP.

CHAP IV.

A conſideration (pertinent to the preſent controverſy) of what happens in trying the Torricellian and other experiments, at the tops and feet of hills.

THERE remain then yet a couple of conſiderations to be oppoſed againſt the examiner's hypotheſis, which, though the paſt diſcourſe may make them be looked upon as needleſs, we muſt not pretermit; becauſe they contain ſuch arguments, as may not only be employed againſt our adverſaries doctrine, but will very much tend to the confirmation of ours.

I CONSIDER then further, that the hypotheſis I am oppoſing, being but a kind of inverſion of ours, and ſuppoſing the ſpring or motion of reſtitution in the air to tend inwards, as according to us it tends outwards; it cannot be, that if the ſuppoſition itſelf were (what I think I have proved it is not) true, many of the phænomena would be plauſibly enough explicable by it; the ſame motions in an intermediate body being in many caſes producible alike, whether we ſuppoſe it to be thruſt or drawn, provided both the endeavours tend the ſame way. But then we may be ſatisfied, whether the effect be to be aſcribed to pulſion or traction (as they commonly ſpeak, though indeed the latter ſeems reducible to the former) if we can find out an experiment, wherein there is reaſon ſuch an effect ſhould follow, in caſe pulſion be the cauſe inquired after, and not in caſe it be traction. And ſuch an *experimentum crucis* (to ſpeak with our illuſtrious *Verulam*) is afforded us by that noble obſervation of Monſieur *Paſchal*, mentioned by the famous *Pecquet*, and out of him by our author: namely, that the Torricellian experiment being made at the foot and in divers places of a very high mountain (of the altitude of five hundred fathom or three thouſand foot) he found, that after he had aſcended a hundred and fifty fathom, the quickſilver was fallen two inches and a quarter below its ſtation at the mountain's foot; and that at the very top of the hill it had deſcended above three inches below the ſame wonted ſtation. Whence it appears, that the quickſilver being carried up towards the top of the atmoſphere, falls down the lower, the higher the place is wherein the obſervation is made: of which the reaſon is plain in our hypotheſis, namely, that the nearer we come to the top of the atmoſphere, the ſhorter and lighter is the cylinder of air incumbent upon the reſtagnant mercury; and conſequently the leſs weight of cylindrical mercury will that air be able to counterpoiſe and keep ſuſpended. And ſince this notable phænomenon does thus clearly follow upon ours, and not upon our adverſaries hypotheſis; this experiment ſeems to determine the controverſy betwixt them: becauſe in this caſe the examiner cannot pretend, as he does in the ſeventeenth and divers other of our experiments, that the deſcent of the quickſilver in the tube is cauſed, not by the diminution of the external air's preſſure, but from the preternatural rareſaction or diſtenſion of that external air (in the receiver) when by ſeeking to reſtore itſelf, it endeavours to draw up the reſtagnant mercury: for in our preſent caſe there appears no ſuch forcible dilatation of that air, as in many of the phænomena of our engine he is pleaſed to imagine.

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IT need therefore be no great wonder, if his adverſaries do, as he obſerves, make a great account of this experiment, to prove, that the mercury is kept up in the tube by the reſiſtance of the external air: nor do I think his anſwers to the argument drawn from hence will keep them from thinking it cogent. For to an objection, upon which he takes notice that they lay ſo much ſtreſs, he replies but two things; which neither ſingly nor together will near amount to a ſatisfactory anſwer.

And, firſt, he queſtions the truth of the obſervation itſelf; becauſe having made trial in a low hill, the event did no ways anſwer his expectation. But though, inſtead of diſapproving, I am willing to commend his curioſity, to make the experiment himſelf, and eſpecially ſince it was both new and important; and though alſo I like his modeſty, in rather ſuſpecting ſome miſtake in the manner of the obſervation, than that the experimenters did not ſincerely deliver it: yet, ſince there muſt be an error ſomewhere, I muſt rather charge it upon the examiner's obſervation (I ſay, his obſervation, not his want of ſincerity) than upon Monſieur *Paſchal*'s. For beſides the commendations, that the learned *Gaſſendus*, who relates the experiment, gives to that ingenious gentleman (Monſieur *Paſchal)* by whoſe direction he ſuppoſes it to have been tried; the ſame *Gaſſendus* relates, that the like obſervation was five times repeated, *partim intra ſacellum, partim aëre libero, & nunc quidem flante, nunc ſilente vento:* 'Sometimes within the chapel, ſometimes in the open air; the wind ſometimes blowing, and ſometimes being ſtill.' Which circumſtances ſufficiently argue the diligence wherewith the experiment was tried in *Auvergne*. Eſpecially ſince I can confirm theſe obſervations by two more made on diſtant hills in *England:* the one of which I procured from that known Virtuoſo Mr. *J. Ball*, whom I deſired to make the experiment at a mountain in *Devonſhire*, on the ſide whereof he dwelt; and the other made in *Lancaſhire* by that ingenious gentleman Mr. *Rich. Townley*. Both which obſervations, ſince I have mentioned them at large in the Appendix to the Phyſico-Mechanical treatiſe, I ſhall not now repeat; contenting myſelf to obſerve to our preſent purpoſe, that however the proportion of the deſcent of the quickſilver may vary, according to the differing conſiſtence and other accidents of the neighbouring air, in the particular places and times of the experiments being made, yet all obſervations agree in this, that nearer the top of the atmoſphere the quickſilver falls lower than it does further from it. To all this I ſhall add two things, that will very much confirm our hypotheſis. The one is, that the freſhly-named Mr. *Townley*, and divers ingenious perſons that aſſiſted at the trial, bethought themſelves of ſo making the Torricellian experiment at the top of the hill, as to leave a determinate quantity of air in the tube, before the mouth of it was opened under the veſſelled mercury; and taking notice how low ſuch a quantity of that air depreſſed the mercurial cylinder, they likewiſe obſerved, that at the mountain's foot the included air was not able to depreſs the quickſilver ſo much. Whence we infer, that the cylinder of air at the top of the hill being ſhorter and lighter, did not ſo ſtrongly preſs againſt the included air, as did the ambient air at the bottom of the hill, where the aerial cylinder was longer and heavier.

Gaſſendus, *Tom.* 1. *p.* 211.

But becauſe that though experiments made in very elevated places are noble ones, and of great importance in the controverſies about the air, yet there are but very few of thoſe, that are qualified to make experiments of that nature, who have the opportunity of making them upon high mountains; we did, with the aſſiſtance of an ingenious man, attempt a trial, wherein we hoped to find a ſenſibly differing weight of the atmoſphere, in a far leſs height than that of an ordinary hill. But inſtead of a common tube, we made uſe of a kind of weather-glaſs, that the included air might help to make the event notable, for a reaſon to be mentioned ere long; and inſtead of quickſilver we employed common water in the pipe belonging to the weather-glaſs, that ſmall changes in the weight or reſiſtance of the atmoſphere in oppoſition of the included air might be the more diſcernable. The inſtrument we made uſe of conſiſted only of a glaſs with a broad foot and a narrow neck A B, and a ſlender glaſs pipe C D, open at both ends: which pipe was ſo placed, that the bottom of it did almoſt, but not quite, reach to the bottom of the bigger glaſs A B, within whoſe neck A, it was faſtened with a cloſe cement, that both kept the pipe in its place, and hindered all communication betwixt the inward I I and outward K K air, ſave by the cavity of the pipe C D. Now we choſe this glaſs A B more than ordinary capacious, that the effect of the dilatation of the included air I I might be the more conſpicuous. Then conveying a convenient quantity of water H H into this glaſs, we carried it to the leads of the lofty abby-church at *Weſtminſter*, and there blew in a little air to raiſe the water to the upper part of the pipe, that being above the veſſel A B, we might more preciſely mark the ſeveral ſtations of the water than otherwiſe we could. Afterward having ſuffered the glaſs to reſt a pretty while upon the lead, that the air I I within might be reduced to the ſame ſtate, both as to coldneſs and as to preſſure, with K K that without, having marked the ſtation of the water F, we gently let down the veſſel by a long ſtring to the foot of the wall, where one attended to receive it; who having ſuffered it to reſt upon the ground, cried to us, that it was ſubſided about an inch below the mark F we had put. Whereupon having ordered him to put a mark at his ſecond ſtation of it E, we drew up the veſſel again; and ſuffering it to reſt a while, we obſerved the water to be re-aſcended to or near the firſt mark F, which was indeed about an inch above E, the other. And this we did that evening a ſecond time with almoſt a like ſucceſs: though two or three days after, the wind blowing ſtrongly upon the leads, we found not the experiment to ſucceed quite ſo regularly as before; yet the water always manifeſtly fell lower at the foot of the wall than it was at the top: which I ſee no cauſe to aſcribe barely to the differing temperature of the air above and below, as to heat and cold, ſince according to the general eſtimate, the more elevated region of the air is, *cæteris paribus*, colder than that below; which would rather check the greater expanſion of the included air at the top of the leads, than to promote it. But the better to avoid miſtakes and prevent objections, we thought fit to try the experiment within the church, and got into a gallery of the ſame height with the leads: but the upper part of the pipe being caſually broken off, we thought fit to order the matter ſo, that the ſurface G of the remaining water in the pipe, ſhould be about an inch higher than the ſurface of the water in the veſſel. And then my above-mentioned correſpondent letting down the glaſs, almoſt as ſoon as it was ſettled upon the pavement, kneeling down to ſee how far it was ſubſided, I found that not only it was fallen as low as the other water, but that the outward air depreſſed it ſo far, as whilſt I was looking on, to break in beneath the bottom of the pipe, and aſcend through the water in bubbles; after which, the glaſs being drawn up again, my correſpondent affirmed, that the water was very manifeſtly reaſcended. But becauſe by the unlucky breaking of a glaſs, we were hindered to obſerve, as we deſigned, what would happen as well in a weather-glaſs, ſo contrived, that the weight or preſſure of the atmoſphere ſhould make no change in it, as in another whoſe included air was at the top (whereas, in that we employed, the included air was in the lower part) and becauſe there happened in our trials a circumſtance or two that ſeemed not ſo devoid of difficulties, but that we think it may require further examination, we deſign to ſet down a more particular account of this experiment (as how it ſucceeds with quickſilver inſtead of water, together with the capacity of the veſſel A B and the bore of the pipe C D, with ſome other variety of circumſtances) together with the event of the curioſity we had (which ſeemed very ſucceſsful) to try the Torricellian experiment upon the above-mentioned leads; and then let down the tube, together with the reſtagnant mercury, to the ground, to obſerve the increaſing altitude of the quickſilver, in the formerly-mentioned appendix to the epiſtle we have been defending. And it ſhall ſuffice us, in the mean time, that the trials already mentioned ſeem to make it evident enough, that the atmoſphere gravitates more, *cæteris paribus*, near the ſurface of the earth, than in the more elevated parts of the air. For the leads, on which we made our trials, were found by meaſure to be in perpendicular height but threeſcore and fifteen foot from the ground. To which we ſhall only add this at preſent, that once being deſirous to obſerve what we could touching the proportions of the ſubſidence of the water to the height of its ſeveral ſtations from the ground, purpoſely carrying down the veſſel ſo as not conſiderably to heat it, from the leads down the ſtairs to a little window that we gueſs to be almoſt half way to the bottom, we there perceived the water to have already ſubſided about a barley-corn's length; notwithſtanding that probably, in ſpite of our care, the veſſel were a little warmed by the heat of his body that carried it, ſince by that time we were come to the foot of the wall, the water ſtood almoſt at the higheſt mark; but after the veſſel was ſuffered to reſt a while, it relapſed by degrees to the loweſt. And thus much for the firſt of the things I had to repreſent in favour of our doctrine.

THE other particular I ſhall mention for confirmation of our hypotheſis, is that experiment (which, though it be needleſs, ſeems yet more cogent and proper to prevent evaſions) made by the ſame Monſieur *Paſchal*, of carrying a weakly-blown foot-ball from the bottom to the top of an high mountain. For that foot-ball ſwelled more and more, the higher it was carried, ſo that it appeared as if it were full blown at the top of the mountain, and gradually growing lank again, as it was carried downwards; ſo that at the foot of the hill it was flaccid as before. This, I ſay, having thus happened, we have here an experiment to prove our hypotheſis, wherein recourſe cannot be had to any forcibly and preternaturally diſtended body, ſuch as that is pretended to be, which remains in the deſerted ſpace of the tube in the Torricellian experiment.

THE other thing, which the examiner alleges againſt our argument from Monſieur *Paſchal's* trials, is, that ſuppoſing it to be true, yet it cannot thence be inferred, that the ſubſidence of the mercury at the top of the hill proceeded from the atmoſpherical cylinder's being there lighter and leſs able to ſuſtain the quickſilver. *Sed dici poteſt* (ſays he) *ideo ſic in vertice montis magis deſcendiſſe, quod ibidem eſſet aura frigidior, aut ex alio temperamento hujuſmodi deſcenſum cauſante.* Page 63. 'But it may be ſaid, that on the 'top of the mountain it therefore deſcended after that manner, becauſe the air was 'more cold there, or of ſome other temperature, ſuch as might cauſe this deſcent.' But this ſolution will not ſerve the turn: for the coldneſs of the ambient air (which yet the experiments take not notice of) would rather contract the rarefied ſubſtance within the tube, and ſo draw up the mercury higher; as our author himſelf teaches us, that it is from the ſhrinking of the Funiculus, occaſioned by the cold, that the water in thermometers aſcends in cold weather. Page 50. And whereas the only proof he adds of ſo improbable an explication is taken from our eighteenth experiment, wherein we relate, that ſometimes the quickſilver did ſenſibly fall lower in colder than in far leſs cold weather; I anſwer, that this eighteenth experiment will ſcarce make more for him than againſt him: for, as I there take notice, that the quickſilver deſcended in cold weather, ſo it ſometimes deſcended likewiſe in hot weather, and roſe in cold. And it is very ſtrange, that in all the obſervations made in differing countries and at different times, it ſhould ſtill ſo happen, that the mercurial cylinder ſhould be ſhorter near the top of the atmoſphere than further from it; if the reſiſtance of the outward air have nothing to do with the keeping it ſuſpended. And it is yet more ſtrange, that the foot-ball ſhould in like manner grow turgid and flaccid, according as it is carried into places where it has a ſhorter or longer pillar of air incumbent on it.

I WAS going to proceed to what remains of this ſecond part of our treatiſe, but that, ſince I begun this chapter, caſually meeting with an experiment lately ſent in a letter to a very ingenious * acquaintance of his and mine, by a very induſtrious † phyſician (who is ſaid to have had the curioſity to try over again many of the experiments of our engine) and finding it very proper to confirm our newly related experiment made at *Weſtminſter*, and to be of ſuch a nature as we have not in this part of *England* the opportunity to try the like, for want of hills high enough; I ſhall (according to the permiſſion given me) inſert it in this place. And the rather, that as the mountains have, by the trials made on them of the Torricellian experiment, afforded us a noble proof of the weight of the air; ſo they may afford us one of its ſpring: wherein I hope the phænomenon of the water's deſcent will not be aſcribed to any attraction made of the water by the violently-diſtended outward air. And becauſe the experiment was not made by us, but by another, we will ſet it down in his words, which are theſe: *This fifteenth of* October 1661, *we took a weather-glaſs* A B, *of about two foot in length, and carrying it to the bottom of* Hallifax *hill, the water ſtood in the ſhank at thirteen inches above the water in the veſſel: thence carrying it thus filled, with the whole frame, immediately to the top of the ſaid hill, the water fell down to the point* D, viz. *an inch and a quarter lower than it was at the bottom of the ſaid hill; which* (as he rightly infers) *proves the elaſticity of the air. For the internal air* A C, *which was of the ſame power and extenſion with the external at the bottom of the hill, did manifeſt a greater elaſticity than the mountain-air there*, and ſo extended itſelf further by* C D.

* *Mr.* Croon, one *of the learned profeſſors of* Greſham *college.*

† *Dr.* Hen. Power.

See the ſecond figure.

THE like experiment, I hear, the ſame ingenious doctor has very lately repeated, and found the deſcent of the water to be greater than before. And though ſome Virtuoſi have thought it ſtrange, that in an hill far inferior to the *Alps* and *Apennines*, ſo ſhort a cylinder of ſo light a liquor as water ſhould fall ſo much; yet I ſee not any reaſon to diſtruſt upon this ground either his experiment or ours (lately mentioned to have been made at *Weſtminſter*;) but rather to wonder the water fell no more (if the hill be conſiderably high.) For their ſuſpicion ſeems grounded upon a miſtake, as if becauſe the quickſilver in the Torricellian experiment, made without purpoſely leaving any air in the tube, would not, at the top of the mentioned hill, have ſubſided above an inch, if ſo much; the water, that is near fourteen times lighter, ſhould not fall above a fourteenth part of that ſpace: whereas in the Torricellian experiment, the upper and deſerted ſpace of the tube has little or no air left in it, but the correſpondent part of the water-glaſs was furniſhed with air, whoſe preſſure was little leſs than that of the atmoſphere at the bottom of the hill; and conſequently muſt be much greater than the preſſure of the atmoſphere at the top of the hill, where the atmoſpherical cylinder's gravity (upon whoſe account it preſſes) muſt be much diminiſhed by its being made much ſhorter, and by its conſiſting of an air leſs compreſt. And thus much for the firſt of the two conſiderations wherewith I promiſed to conclude this ſecond part of the preſent tract. Only before I proceed, I muſt in a word deſire the reader to take notice, that though I have here ſingled out but one of the nine experiments which the examiner in the 11th and 12th chapters reckons up as urged by his adverſaries; yet do not thereby declare my acquieſcing in his explications of thoſe phænomena, but only leave both them and ſome other things he delivers about ſiphons and the *Magdeburg* experiments, to be diſcourſed by thoſe that are more concerned to examine them, contenting myſelf to have ſufficiently diſproved the Funiculus which his expoſitions ſuppoſe, and cleared the grounds of explicating ſuch experiments aright.

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* Probably theſe or the like words, *did manifeſt preſſure*, are here omitted, for the mountain air there ſeems to have acted rather by its weight than elaſticity.

CHAP. V.

Two new Experiments touching the meaſure of the force of the ſpring of air compreſſed and dilated.

Page 11. THE other thing, that I would have conſidered touching our adverſary's hypotheſis is, that it is needleſs. For whereas he denies not, that the air has ſome weight and ſpring, but affirms, that it is very inſufficient to perform ſuch great matters as the counterpoiſing of a mercurial cylinder of 29 inches, as we teach that it may; we ſhall now endeavour to manifeſt by experiments purpoſely made, that the ſpring of the air is capable of doing far more than it is neceſſary for us to aſcribe to it, to ſolve the phænomena of the Torricellian experiment.

We took then a long glaſs-tube, which, by a dexterous hand and the help of a lamp, was in ſuch a manner crooked at the bottom, that the part turned up was almoſt parallel to the reſt of the tube, and the orifice of this ſhorter leg of the ſiphon (if I may ſo call the whole inſtrument) being hermetically ſealed, the length of it was divided into inches (each of which was ſubdivided into eight parts) by a ſtreight liſt of paper, which containing thoſe diviſions, was carefully paſted all along it. Then putting in as much quickſilver as ſerved to fill the arch or bended part of the ſiphon, that the mercury ſtanding in a level might reach in the one leg to the bottom of the divided paper, and juſt to the ſame height or horizontal line in the other; we took care, by frequently inclining the tube, ſo that the air might freely paſs from one leg into the other by the ſides of the mercury (we took, I ſay, care) that the air at laſt included in the ſhorter cylinder ſhould be of the ſame laxity with the reſt of the air about it. This done, we began to pour quickſilver into the longer leg of the ſiphon, which by its weight preſſing up that in the ſhorter leg, did by degrees ſtreighten the included air: and continuing this pouring in of quickſilver till the air in the ſhorter leg was by condenſation reduced to take up but half the ſpace it poſſeſſed (I ſay, poſſeſſed, not filled) before; we caſt our eyes upon the longer leg of the glaſs, on which was likewiſe paſted a liſt of paper carefully divided into inches and parts, and we obſerved, not without delight and ſatisfaction, that the quickſilver in that longer part of the tube was 29 inches higher than the other. Now that this obſervation does both very well agree with and confirm our hypotheſis, will be eaſily diſcerned by him, that takes notice what we teach; and Monſieur *Paſchal* and our Engliſh friend's experiments prove, that the greater the weight is that leans upon the air, the more forcible is its endeavour of dilatation, and conſequently its power of reſiſtance (as other ſprings are ſtronger when bent by greater weights). For this being conſidered, it will appear to agree rarely-well with the hypotheſis, that as according to it the air in that degree of denſity and correſpondent meaſure of reſiſtance, to which the weight of the incumbent atmoſphere had brought it, was able to counterbalance and reſiſt the preſſure of a mercurial cylinder of about 29 inches, as we are taught by the Torricellian experiment; ſo here the ſame air being brought to a degree of denſity about twice as great as that it had before, obtains a ſpring twice as ſtrong as formerly. As may appear by its being able to ſuſtain or reſiſt a cylinder of 29 inches in the longer tube, together with the weight of the atmoſpherical cylinder, that leaned upon thoſe 29 inches of mercury; and, as we juſt now inferred from the Torricellian experiment, was equivalent to them.

WE were hindered from proſecuting the trial at that time by the caſual breaking of the tube. But becauſe an accurate experiment of this nature would be of great importance to the doctrine of the ſpring of the air, and has not yet been made (that I know) by any man; and becauſe alſo it is more uneaſy to be made than one would think, in regard of the difficulty as well of procuring crooked tubes fit for the purpoſe, as of making a juſt eſtimate of the true place of the protuberant mercury's ſurface; I ſuppoſe it will not be unwelcome to the reader, to be informed, that after ſome other trials, one of which we made in a tube whoſe longer leg was perpendicular, and the other, that contained the air, parallel to the horizon, we at laſt procured a tube of the figure expreſt in the ſcheme; which tube, though of a pretty bigneſs, was ſo long, that the cylinder, whereof the ſhorter leg of it conſiſted, admitted a liſt of paper, which had before been divided into 12 inches and their quarters, and the longer leg admitted another liſt of paper of divers feet in length, and divided after the ſame manner. Then quickſilver being poured in to fill up the bended part of the glaſs, that the ſurface of it in either leg might reſt in the ſame horizontal line, as we lately taught, there was more and more quickſilver poured into the longer tube; and notice being watchfully taken how far the mercury was riſen in that longer tube, when it appeared to have aſcended to any of the diviſions in the ſhorter tube, the ſeveral obſervations, that were thus ſucceſſively made, and as they were made ſet down, afforded us the enſuing table: *See Fig. 5.*

A table of the condenſation of the air.

A	*A*	*B*	*C*	*D*	*E*
48	12	00	Added to $22\frac{1}{8}$ makes	$29\frac{2}{16}$	$29\frac{2}{16}$
46	$11\frac{1}{2}$	$01\frac{7}{16}$		$30\frac{9}{16}$	$33\frac{6}{16}$
44	11	$02\frac{13}{16}$		$31\frac{15}{16}$	$31\frac{12}{16}$
42	$10\frac{1}{2}$	$04\frac{6}{16}$		$33\frac{8}{16}$	$33\frac{1}{7}$
40	10	$06\frac{3}{16}$		$35\frac{5}{16}$	35- -
38	$9\frac{1}{2}$	$07\frac{14}{16}$		37	$36\frac{15}{19}$
36	9	$10\frac{2}{16}$		$39\frac{5}{16}$	$38\frac{7}{8}$
34	$8\frac{1}{2}$	$12\frac{8}{16}$		$41\frac{10}{16}$	$41\frac{2}{17}$
32	8	$15\frac{1}{16}$		$44\frac{3}{16}$	$43\frac{11}{16}$
30	$7\frac{1}{2}$	$17\frac{15}{16}$		$47\frac{1}{16}$	$46\frac{3}{5}$
28	7	$21\frac{3}{16}$		$50\frac{5}{16}$	50- -
26	$6\frac{1}{2}$	$25\frac{3}{16}$		$54\frac{5}{16}$	$53\frac{10}{13}$
24	6	$29\frac{11}{16}$		$58\frac{13}{16}$	$58\frac{2}{8}$
23	$5\frac{3}{4}$	$32\frac{3}{16}$		$61\frac{5}{16}$	$60\frac{18}{23}$
22	$5\frac{1}{2}$	$34\frac{15}{16}$		$64\frac{1}{16}$	$63\frac{6}{11}$
21	$5\frac{1}{4}$	$37\frac{15}{16}$		$67\frac{1}{16}$	$66\frac{4}{7}$
20	5	$41\frac{9}{16}$		$70\frac{11}{16}$	70- -
19	$4\frac{3}{4}$	45- -		$74\frac{2}{16}$	$73\frac{11}{19}$
18	$4\frac{1}{2}$	$48\frac{12}{16}$		$77\frac{14}{16}$	$77\frac{2}{3}$
17	$4\frac{1}{4}$	$53\frac{11}{16}$		$82\frac{12}{16}$	$82\frac{4}{17}$
16	4	$58\frac{2}{16}$		$87\frac{14}{16}$	$87\frac{3}{8}$
15	$3\frac{3}{4}$	$63\frac{15}{16}$		$93\frac{1}{16}$	$93\frac{1}{5}$
14	$3\frac{1}{2}$	$71\frac{5}{16}$		$100\frac{7}{16}$	$99\frac{6}{7}$
13	$3\frac{1}{4}$	$78\frac{11}{16}$		$107\frac{13}{16}$	$107\frac{7}{13}$
12	3	$88\frac{7}{16}$		$117\frac{9}{16}$	$116\frac{4}{8}$

AA. The number of equal ſpaces in the ſhorter leg, that contained the ſame parcel of air diverſly extended.

B. The height of the mercurial cylinder in the longer leg, that compreſſed the air into thoſe dimenſions.

C. The height of the mercurial cylinder, that counterbalanced the preſſure of the atmoſphere.

D. The aggregate of the two laſt columns *B* and *C*, exhibiting the preſſure ſuſtained by the included air.

E. What that preſſure ſhould be according to the hypotheſis, that ſuppoſes the preſſures and expanſions to be in reciprocal proportion.

For the better underſtanding of this experiment, it may not be amiſs to take notice of the following particulars:

1. That the tube being ſo tall, that we could not conveniently make uſe of it in a chamber, we were fain to uſe it on a pair of ſtairs, which yet were very lightſome, the tube being for preſervation's ſake by ſtrings ſo ſuſpended, that it did ſcarce touch the box preſently to be mentioned.

2. The lower and crooked part of the pipe was placed in a ſquare wooden box, of a good largeneſs and depth, to prevent the loſs of the quickſilver, that might fall aſide in the transfuſion from the veſſel into the pipe, and to receive the whole quickſilver in caſe the tube ſhould break.

3. That we were two to make the obſervation together, the one to take notice at the bottom, how the quickſilver roſe in the ſhorter cylinder, and the other to pour in at the top of the longer; it being very hard and troubleſome for one man alone to do both accurately.

4. That the quickſilver was poured in but by little and little, according to the direction of him that obſerved below; it being far eaſier to pour in more, than to take out any, in caſe too much at once had been poured in.

5. THAT at the beginning of the operation, that we might the more truly diſcern where the quickſilver reſted from time to time, we made uſe of a ſmall looking-glaſs, held in a convenient poſture to reflect to the eye what we deſired to diſcern.

6. THAT when the air was ſo compreſſed, as to be crouded into leſs than a quarter of the ſpace it poſſeſſed before, we tried whether the cold of a linen cloth dipped in water would then condenſe it. And it ſometimes ſeemed a little to ſhrink, but not ſo manifeſtly as that we dare build any thing upon it. We then tried likewiſe, whether heat would, notwithſtanding ſo forcible a compreſſure, dilate it; and approaching the flame of a candle to that part where the air was pent up, the heat had a more ſenſible operation than the cold had before; ſo that we ſcarce doubted, but that the expanſion of the air would, notwithſtanding the weight that oppreſt it, have been made conſpicuous, if the fear of unſeaſonably breaking the glaſs had not kept us from increaſing the heat.

Now although we deny not, but that in our table ſome particulars do not ſo exactly anſwer to what our formerly mentioned hypotheſis might perchance invite the reader to expect; yet the variations are not ſo conſiderable, but that they may probably enough be aſcribed to ſome ſuch want of exactneſs as in ſuch nice experiments is ſcarce avoidable. But for all that, till further trial hath more clearly informed me, I ſhall not venture to determine, whether or no the intimated theory will hold univerſally and preciſely, either in condenſation of air, or rarefaction: all that I ſhall now urge being, that however the trial already made ſufficiently proves the main thing, for which I here allege it; ſince by it, it is evident, that as common air, when reduced to half its wonted extent, obtained near about twice as forcible a ſpring as it had before; ſo this thus compreſt air being further thruſt into half this narrow room, obtained thereby a ſpring about as ſtrong again as that it laſt had, and conſequently four times as ſtrong as that of the common air. And there is no cauſe to doubt, that if we had been here furniſhed with a greater quantity of quickſilver and a very ſtrong tube, we might, by a further compreſſion of the included air, have made it counterbalance the preſſure of a far taller and heavier cylinder of mercury. For no man perhaps yet knows, how near to an infinite compreſſion the air may be capable of, if the compreſſing force be competently increaſed. So that here our adverſary may plainly ſee, that the ſpring of the air, which he makes ſo light of, may not only be able to reſiſt the weight of 29 inches, but in ſome caſes of above a hundred inches of quickſilver, and that without the aſſiſtance of his Funiculus, which in our preſent caſe has nothing to do. And to let you ſee, that we did not (as a little above) inconſiderately mention the weight of the incumbent atmoſpherical cylinder as a part of the weight reſiſted by the impriſoned air, we will here annex, that we took care, when the mercurial cylinder in the longer leg of the pipe was about an hundred inches high, to cauſe one to ſuck at the open orifice; whereupon (as we expected) the mercury in the tube did notably aſcend. Which conſiderable phænomenon cannot be aſcribed to our examiner's Funiculus, ſince by his own confeſſion that cannot pull up the mercury, if the mercurial cylinder be above 29 or 30 inches of mercury. And therefore we ſhall render this reaſon of it, that the preſſure of the incumbent air being in part taken off by its expanding itſelf into the ſucker's dilated cheſt; the impriſoned air was thereby enabled to dilate itſelf manifeſtly, and repel the mercury, that compreſt it, till there was an equality of force betwixt the ſtrong ſpring of that compreſt air on the one part, and the tall mercurial cylinder, together with the contiguous dilated air, on the other part.

Now, if to what we have thus delivered concerning the compreſſion of the air, we add ſome obſervations concerning its ſpontaneous expanſion, it will the better appear, how much the phænomena of theſe mercurial experiments depend upon the differing meaſures of ſtrength to be met with in the air's ſpring, according to its various degrees of compreſſion and laxity. But, before I enter upon this ſubject, I ſhall readily acknowledge, that I had not reduced the trials I had made about meaſuring the expanſion of the air to any certain hypotheſis, when that ingenious gentleman Mr. *Richard Townley* was pleaſed to inform me, that having by the peruſal of my phyſico-mechanical experiments been ſatisfied that the ſpring of the air was the cauſe of it, he endeavoured (and I wiſh in ſuch attempts other ingenious men would follow his example) to ſupply what I had omitted concerning the reducing to a preciſe eſtimate, how much air dilated of itſelf loſes of its elaſtical force, according to the meaſures of its dilatation. He added, that he had begun to ſet down what occurred to him to this purpoſe in a ſhort diſcourſe, whereof he afterwards did me the favour to ſhew me the beginning, which gives me a juſt curioſity to ſee it perfected. But, becauſe I neither know, nor (by reaſon of the great diſtance betwixt our places of reſidence) have at preſent the opportunity to inquire, whether he will think fit to annex his diſcourſe to our appendix, or to publiſh it by itſelf, or at all; and becauſe he hath not yet, for aught I know, met with fit glaſſes to make an any-thing-accurate table of the decrement of the force of the dilated air; our preſent deſign invites us to preſent the reader with that which follows, wherein I had the aſſiſtance of the ſame perſon, that I took notice of in the former chapter, as having written ſomething about rarefaction: whom I the rather make mention of on this occaſion, becauſe when he firſt heard me ſpeak of Mr. *Townley*'s ſuppoſitions about the proportion, wherein air loſes of its ſpring by dilatation, he told me he had the year before (and not long after the publication of my pneumatical treatiſe) made obſervations to the ſame purpoſe, which he acknowledged to agree well enough with Mr. *Townley*'s theory: and ſo did (as their author was pleaſed to tell me) ſome trials made about the ſame time by that noble virtuoſo and eminent mathematician the Lord *Brouncker*, from whoſe further enquiries into this mattter, if his occaſions will allow him to make them, the curious may well hope for ſomething very accurate.

A table of the rarefaction of the air.

A. The number of equal ſpaces at the top of the tube, that contained the ſame parcel of air.
B. The height of the mercurial cylinder, that together with the ſpring of the included, air counterbalanced the preſſure of the atmoſphere.
C. The preſſure of the atmoſphere.
D. The complement of *B* to *C*, exhibiting the preſſure ſuſtained by the included air.
E. What that preſſure ſhould be, according to the hypotheſis.

A	*B*	*C*	*D*	*E*
1	$00\frac{0}{0}$	Subſtracted from $29\frac{3}{4}$ leaves	$29\frac{3}{4}$	$29\frac{3}{4}$
$1\frac{1}{2}$	$10\frac{5}{8}$		$19\frac{1}{8}$	$19\frac{5}{6}$
2	$15\frac{3}{8}$		$14\frac{3}{8}$	$14\frac{7}{8}$
3	$20\frac{2}{8}$		$9\frac{4}{8}$	$9\frac{15}{12}$
4	$22\frac{5}{8}$		$7\frac{1}{8}$	$7\frac{7}{16}$
5	$24\frac{1}{8}$		$5\frac{5}{8}$	$5\frac{19}{26}$
6	$24\frac{7}{8}$		$4\frac{7}{8}$	$4\frac{27}{26}$
7	$25\frac{4}{8}$		$4\frac{2}{8}$	$4\frac{1}{4}$
8	$26\frac{0}{0}$		$3\frac{6}{8}$	$3\frac{23}{32}$
9	$26\frac{3}{8}$		$3\frac{3}{8}$	$3\frac{11}{86}$
10	$26\frac{6}{8}$		$3\frac{0}{0}$	$2\frac{39}{40}$
12	$27\frac{1}{8}$		$2\frac{5}{8}$	$2\frac{23}{48}$
14	$27\frac{4}{8}$		$2\frac{2}{8}$	$2\frac{1}{8}$
16	$27\frac{6}{8}$		$2\frac{0}{0}$	$1\frac{55}{64}$
18	$27\frac{7}{8}$		$1\frac{7}{8}$	$1\frac{47}{72}$
20	$28\frac{0}{0}$		$1\frac{6}{8}$	$1\frac{9}{80}$
24	$28\frac{2}{8}$		$1\frac{4}{8}$	$1\frac{23}{96}$
28	$28\frac{3}{8}$		$1\frac{3}{8}$	$1\frac{1}{16}$
32	$28\frac{4}{8}$		$1\frac{2}{8}$	$0\frac{119}{128}$

To make the experiment of the debilitated force of expanded air the plainer, it will not be amiſs to note ſome particulars, eſpecially touching the manner of making the trial; which (for the reaſons lately mentioned) we made on a lightſome pair of ſtairs, and with a box alſo lined with paper to receive the mercury that might be ſpilt. And in regard it would require a vaſt, and in few places procurable quantity of quickſilver, to imploy veſſels of ſuch kind as are ordinary in the Torricellian experiment, we made uſe of a glaſs-tube of about ſix feet long; for that being hermetically ſealed at one end, ſerved our turn as well as if we could have made the experiment in a tub or pond of ſeventy inches deep.

SECONDLY, We alſo provided a ſlender glaſs-pipe of about the bigneſs of a ſwan's quill, and open at both ends; all along which was paſted a narrow liſt of paper, divided into inches and half quarters.

THIRDLY, This ſlender pipe being thruſt down into the greater tube almoſt filled with quickſilver, the glaſs helped to make it ſwell to the top of the tube; and the quickſilver getting in at the lower orifice of the pipe, filled it up till the mercury included in that was near about a level with the ſurface of the ſurrounding mercury in the tube.

FOURTHLY, There being, as near as we could gueſs, little more than an inch of the ſlender pipe left above the ſurface of the reſtagnant mercury, and conſequently unfilled therewith, the prominent orifice was carefully cloſed with ſealing-wax melted; after which the pipe was let alone for a while, that the air dilated a little by the heat of the wax, might, upon refrigeration, be reduced to its wonted denſity. And then we obſerved by the help of the above-mentioned liſt of paper, whether we had not included ſomewhat more or ſomewhat leſs than an inch of air; and in either caſe we were fain to rectify the error by a ſmall hole made (with a heated pin) in the wax, and afterwards cloſed up again.

FIFTHLY, Having thus included a juſt inch of air, we lifted up the ſlender pipe by degrees, till the air was dilated to an inch, an inch and an half, two inches, *&c.* and obſerved in inches and eighths the length of the mercurial cylinder, which at each degree of the air's expanſion was impelled above the ſurface of the reſtagnant mercury in the tube.

SIXTHLY, The obſervations being ended, we preſently made the Torricellian experiment with the above-mentioned great tube of ſix feet long, that we might know the height of the mercurial cylinder, for that particular day and hour; which height we found to be 29¾ inches.

SEVENTHLY, Our obſervations made after this manner furniſhed us with the preceding table, in which there would not probably have been found the difference here ſet down betwixt the force of the air, when expanded to double its former dimenſions, and what that force ſhould have been preciſely according to the theory, but that the included inch of air received ſome little acceſſion during the trial; which this newly mentioned difference making us ſuſpect, we found by replunging the pipe into the quickſilver, that the included air had gained about half an eighth, which we gueſſed to have come from ſome little aërial bubbles in the quickſilver, contained in the pipe (ſo eaſy is it in ſuch nice experiments to miſs of exactneſs). We tried alſo with 12 inches of air ſhut up to be dilated; but being then hindered by ſome unwelcome avocations to proſecute thoſe experiments, we ſhall elſewhere, out of other notes and trials (God permitting) ſet down ſome other accurate tables concerning this matter. By which poſſibly we may be aſſiſted to reſolve, whether the atmoſphere ſhould be looked upon (as it uſually is) as a limited and bounded portion of the air; or whether we ſhould, in a ſtricter ſenſe than we did before, uſe the atmoſphere and aërial part of the world for almoſt equivalent terms; or elſe whether we ſhould allow the word atmoſphere ſome other notion in relation to its extent and limits; (for as to its ſpring and weight, theſe experiments do not queſtion, but evince them.) But we are willing, as we ſaid, to refer the matters to our Appendix, and till then to retain our wonted manner of ſpeaking of the air and atmoſphere. In the mean time (to return to our laſt-mentioned experiments) beſides that ſo little a variation may be in great part imputed to the difficulty of making experiments of this nature exactly, and perhaps a good part of it to ſomething of inequality in the cavity of the pipe, or even in the thickneſs of the glaſs; beſides this, I ſay, the proportion betwixt the ſeveral preſſures of the included air undilated and expanded, eſpecially when the dilatation was great (for when the air ſwelled but to four times its firſt extent, the mercurial cylinder, though of near 23 inches, differed not a quarter of an inch from what it ſhould have been according to mathematical exactneſs) the proportion, I ſay, was ſuitable enough to what might be expected, to allow us to make this reflection upon the whole; that whether or no the intimated theory will hold exactly (for about that, as I ſaid above, I dare determine nothing reſolutely till I have further conſidered the matter) yet ſince the inch of air, when it was firſt included, was ſhut up with no other preſſure than that, which it had from the weight of the incumbent air, and was no more compreſſed than the reſt of the air we breathed and moved in; and ſince alſo this inch of air, when expanded to twice its former dimenſions, was able with the help of a mercurial cylinder of about 15 inches to counterpoiſe the weight of the atmoſphere, which the weight of the external air gravitating upon the reſtagnant mercury was able to impel up into the pipe, and ſuſtain above twenty-eight inches of mercury, when the internal air, by its great expanſion, had its ſpring too far debilitated to make any conſiderable (I ſay conſiderable, for it was not yet ſo dilated as not to make ſome) reſiſtance: ſince, I ſay, theſe things are ſo, the free air here below appears to be almoſt as ſtrongly compreſſed by the weight of the incumbent air, as it would be by the weight of a mercurial cylinder of twenty eight or thirty inches; and conſequently is not in ſuch a ſtate of laxity and freedom as men are wont to imagine; and acts like ſome mechanical agent, the decrement of whoſe force holds a ſtricter proportion to its increaſe of dimenſion, than has been hitherto taken notice of.

I MUST not now ſtand to propoſe the ſeveral reflections, that may be made upon the foregoing obſervations touching the compreſſion and expanſion of air; partly becauſe we could ſcarce avoid making the hiſtorical part ſomewhat prolix; and partly becauſe I ſuppoſe we have already ſaid enough to ſhew what was intended: namely, that to ſolve the phænomena there is not of our adverſary's hypotheſis any need: the evincing of which will appear to be of no ſmall moment in our preſent controverſy to him that conſiders, that the two main things, that induced the learned examiner to reject our hypotheſis, are, that nature abhors a vacuum; and that though the air have ſome weight and ſpring, yet, theſe are inſufficient to make out the known phænomena; for which we muſt therefore have recourſe to his Funiculus. Now as we have formerly ſeen, that he has not ſo ſatisfactorily diſproved as reſolutely rejected a vacuum, ſo we have now manifeſted, that the ſpring of the air may ſuffice to perform greater things than what our explication of the Torricellian experiments and thoſe of our engine obliges us to aſcribe to it. Wherefore ſince beſides the ſeveral difficulties, that incumber the hypotheſis we oppoſe, and eſpecially its being ſcarce, if at all, intelligible, we can add that it is unneceſſary; we dare expect, that ſuch readers as are not biaſſed by their reverence for *Ariſtotle*, or the Peripatetick ſchools, will hardly reject an hypotheſis, which, beſides that it is very intelligible, is now proved to be ſufficient, only to imbrace a doctrine, that ſuppoſes ſuch a rarefaction and condenſation, as many famous Naturaliſts rejected for its not being comprehenſible, even when they knew of no other way (that was probable) of ſolving the phænomena wont to be explicated by it.

PART III.

Wherein what is objected againſt Mr. BOYLE'*s Explications of particular Experiments, is anſwered.*

AND now we are come to the third and laſt part of our defence; wherein we are to conſider, what our examiner is pleaſed to object againſt ſome paſſages of our Phyſico-Mechanical Treatiſe. But though this may ſeem the only part, wherein I am particularly concerned; yet perhaps we ſhall find it, if not the ſhorteſt, at leaſt the eaſieſt, part of our taſk. Partly, becauſe our author takes no exceptions at the experiments themſelves, as we have recorded them (which from an adverſary, who in ſome places ſpeaks of them as an eye-witneſs, is no contemptible teſtimony, that the matters of fact have been rightly delivered): and partly, becauſe there are divers experiments which, together with their explications, the examiner has thought fit to leave untouched, and thereby allows us to do ſo too: and partly alſo, becauſe that (as to divers of thoſe experiments, upon which he animadverts) he does not pretend to ſhew, that our explications are ill deduced or incongruous to our principles; but only that the phænomena may be explained either better, or as well, by his hypotheſis; whereof he ſuppoſes himſelf to have demonſtrated the truth, together with the erroneouſneſs of ours, in the other parts of his book, eſpecially the third, fourth and fifth chapters. So that after what we have ſaid to vindicate the hypotheſis we maintain, and take away our author's imaginary Funiculus; it will not be requiſite for us, on ſuch occaſions, to examine his particular aſſertions and explications. Which advertiſement we hope the reader will be pleaſed to bear in mind, and thereby ſave himſelf and us the trouble of a great deal of unneceſſary repetition. Wherefore, preſuming he will do ſo, we ſhall not ſtay to examine the firſt and ſecond corollaries, which in this 17th chapter he annexes to the manner of emptying our receiver by our pump. Neither ſhould we ſay any thing as to his third and laſt corollary, but that we think fit to deſire the reader to take notice, that according to what he teaches in that place, the more the air is rarefied, the more forcibly it is able to contract itſelf.

A defence of our firſt and ſecond EXPERIMENTS.

AND to proceed now to his 18th chapter, which he intitles *De experimentis Boylianis*, we ſhall find, according to what we lately noted, that againſt the firſt experiment he objects nothing, ſave that if one of the fingers be applied to the orifice of the valve, when the pump is freed from air, the experimenter ſhall feel to his pain, that the ſucker is not thruſt inward by the external air, but, as the finger, drawn inward by the internal. But this phænomenon of the intruſion of the finger into a cavity, where it finds no reſiſtance, having been formerly accounted for according to our hypotheſis, we ſhall not need to repeat our explication of it; though this miſtaken phænomenon ſupplies our adverſary with divers of his following animadverſions, and indeed with a great part of his book. And accordingly his objection againſt our ſecond experiment being of the ſame nature with that againſt the firſt, requires but the ſame anſwer: for it will not alter the caſe, that he adds upon this experiment, *hoc eſſe diſcrimen manifeſtum inter preſſionem & ſuctionem, quod ſuctio efficiat hujuſmodi adhæſionem, preſſio autem minimè:* 'That this is the difference 'between preſſion and ſuction, that ſuction makes ſuch an adheſion, and preſſion 'doth not:' Since to ſay ſo, is but to affirm, not to prove.

EXPERIMENT III.

WHAT our author would except againſt the 3d experiment he ought to have more intelligibly expreſſed: for whereas of a diſcourſe, wherein I deliver ſeveral particulars, he only ſays, that *nullatenus ſatisfacit, ut legenti conſtabit:* I would not do the reader the injury to ſuſpect him of taking this prooſleſs aſſertion for a rational confutation: eſpecially ſince, upon the review of that third experiment, I find nothing, that agrees not with my hypotheſis, however it may diſagree with the examiner's. But, to conſider the explication he ſubſtitutes in the room of our doctrine, which he rejects, he gives it us in theſe words: *Hoc quoque experimentum principis noſtris optime convenit: cum enim per illam emboli depreſſionem aër in cavitate brachii incluſus ſeparetur ab eodem brachio, deſcendatque ſimul cum embolo (uti de aqua ſimul cum argento vivo deſcendente capite decimo tertio vidimus) fit ut in tota illa depreſſione, novæ ſemper ab aëre illo deſcendente ſuperficies diripiantur ſimul & extendantur, ut ibidem de aqua eſt explicatum: cum itaque æque facile diripiantur & extendantur hujuſmodi ſuperficies in fine depreſſionis ac initio, mirum non eſt, quod eadem utrobique ſentiatur deprimendi difficultas:* 'And even this experiment doth very well agree with our principles: for ſeeing by this depreſſion of the ſucker, the air ſhut up in the cavity of 'the cylinder is ſeparated from the cylinder, and doth deſcend together with the 'ſucker (as we have, chap. 13. obſerved of water deſcending together with quickſilver) it comes to paſs, that in that whole depreſſion new ſurfaces are taken from 'that deſcending air, and ſtretched out, as we have there explained it in the caſe of 'deſcending water. Since therefore ſuch ſurfaces are as eaſily ſlipped off and extended in the end of the depreſſion as in the beginning; it is no wonder, that there 'is found the ſame difficulty of depreſſing it at both times.'

BY which thought he ſeems to intend an oppoſition to that part of the third experiment, which I oppoſed not againſt his opinion, but that of ſome learned vacuiſts; yet (not to mention, that he ſeems to have ſomewhat miſtaken my ſenſe) he offers nothing at all to invalidate my inference againſt them; but, inſtead of that, propoſes a defence of his own opinion, which ſuppoſes the truth of this diſproved hypotheſis, and is either unſatisfactory even according to that, or elſe diſagrees with what himſelf hath taught us but a little before. For it is evident, that the more the ſucker is depreſſed, the more the cylinder is exhauſted of air. And in his third corollary (which we lately deſired the reader to obſerve) ſpeaking of the air in the receiver (and the caſe is the ſame with the air in the cylinder) he affirms more than once, *Eo magis extendi ac rareſieri aërem relictum, quo plus inde exhauritur, majoremque proinde acquirere vim ſeſe contrahendi:* 'That the air is ſo much the more 'extended and rarefied, by how much the more is thence exhauſted, and ſo doth 'acquire a greater force of contracting itſelf.' Whereas here he would have us believe, that the little internal air, that was in the cavity of the ſhank of the ſtop-cock, does as ſtrongly retract the ſucker, or, which in our caſe is all one, reſiſts its depreſſion, when the ſucker is yet near the top of the cylinder (and conſequently when the included air is but a little dilated) as when the ſame ſucker being forced down to the lower part of the cylinder, the ſame portion of remaining air muſt be exceedingly more diſtended.

EXPERIMENT IV.

IN the fourth experiment, touching the ſwelling of a bladder upon the removal of the ambient air, and proportionably to that removal, our author objects nothing againſt the explication we gave of it by the ſpring of the air included in the bladder, and diſtending it according as the preſſure of the ambient air is weakened. But he endeavours alſo to explicate it his way, to which he ſays this circumſtance does excellently agree, that upon the regreſs of the external air into the receiver, the tumid bladder immediately ſhrinks, becauſe (ſaith he) by ſuch ingreſs of the external air, the air in the receiver, which drew the ſides of the bladder outward from the middle of it, is relaxed. Which explication, whether it be more natural than ours (that aſcribes the ſhrinking of the bladder to the preſſure of the air, that is let into the receiver) let the reader judge, who has conſidered what we have formerly objected againſt the examiner's Funiculus, and the relaxation of it upon the admiſſion of air.

As for the reaſon likewiſe he adds, why a perforated bladder does not alſo ſwell, namely, that by the hole, how little ſoever, the included air is ſucked out by the rarefied ambient, we leave it to the impartial reader to conſider, whether is the more genuine explication, either ours (againſt which he has nothing to object) or his, which to make clearly out he ought (according to what we formerly noted diſputing againſt his Funiculus) to ſhew us what kind of ſtrings they are; which, though, according to him, ſtrongly faſtened to the inſide of the receiver and the ſuperficies of the bladder, muſt draw juſt as forcibly one as another, how long ſoever they be without the bladder in compariſon of thoſe, that within the bladder draw ſo as to hinder the deduction of its ſides. For experience ſhews, that in a perforated bladder the wrinkles continue, as if there were no drawing at all.

And though he could deſcribe, how ſuch a ſtring may be context, yet our explication will have this advantage in point of probability above his, that whereas he denies not, that the air has ſpring and weight, as we deny his Funiculus to have any other than an imaginary exiſtence; and whereas he acknowledges, that by the inſtrument the air about the bladder is exhauſted; to ſhew, that there needs no more than that, and conſequently no Funiculus, to draw aſunder the ſides of the bladder, we can confirm our explication by the formerly-mentioned experiment of the ingenious *Paſchal*, who carrying a flaccid foot-ball from the bottom to the top of an high mountain, found it to ſwell proportionably as he aſcended, and as the weight and preſſure of the ambient air decreaſed, and likewiſe to ſhrink again as he deſcended. And yet in this caſe there is no recourſe to be had to a Funiculus of violently-rarefied air, to draw aſunder every way the ſides of the foot-ball. But however the examiner will be able to defend his explication, it may ſuffice us, that he has objected nothing againſt ours.

EXPERIMENT V.

AGAINST the cauſe we aſſign of the fifth experiment he likewiſe objects nothing, but only aſcribes the breaking of the bladder to the ſelf-contraction of the rarefied air in the receiver. And therefore referring the reader to what we have newly ſaid about the laſt experiment, we will with our author paſs over the ſixth and ſeventh, to which he has no quarrel, and proceed to the eighth.

EXPERIMENT VIII.

THIS is that, wherein we mention our having broke a glaſs-receiver, which was not globular, by the exhauſtion of moſt of the inward air, whereby its debilitated preſſure became unable to reſiſt the unweakened preſſure of the outward air. But this explication the examiner confidently rejects in theſe words: *At profecto non videtur credibile, molliſſimum hunc aërem tam vehementer vitrum (tantæ præſertim craſſitudinis, quantæ ibidem dicitur) undique ſic comprimere, ut illud perfringat:* ‘But truly ‘it ſeems not credible, that this moſt ſoft air ſhould ſo vehemently compreſs a glaſs ‘on all ſides (eſpecially one of that thickneſs there mentioned) as to break it.’ As if it were more credible, that the little air within (which, according to him, is ſo much thinner than common air) ſhould be able to act more powerfully upon the glaſs than the air without, which himſelf confeſſes to be a heavy body, and which not only reaches from the ſurface of the earth to the top of the higheſt mountains, but which (as may not improbably be argued from what we have elſewhere delivered) may, for aught we know to the contrary, be heaped upon the receiver to the height of ſome hundreds of miles, nay, to I know not how many thouſands, in caſe the atmoſphere be not a bounded portion of the air, but reach as high as it.

See part 2. c. 5.

As for the explication he ſubſtitutes in theſe words, *Verius itaque reſpondetur, ideo ſic fractum eſſe illud vitrum, quia per exhauſtionem illam latera ejus vehementiùs introrſum ſint attracta, quam ut (ob figuram illam reſiſtendo minus idoneam) reſiſtere potuerint. Cum enim incluſus aër lateribus vitri firmiſſimè adhæreat, nihil aliud erit aërem illum ſic exhaurire, quam ſatagere latera vitri introrſum flectere:* ‘It is therefore more truly ‘anſwered, that the glaſs is therefore ſo broken, becauſe by that exſuction its ſides ‘are more vehemently drawn inwards than (by reaſon of the figure unfit for reſiſtency) they were able to reſiſt. For ſeeing the included air doth moſt firmly ‘ſtick to the ſides of the glaſs, to draw out the air will be nothing elſe, but to ‘endeavour to bend the ſides of the glaſs inwards.’ By what we have already diſcourſed about the Funiculus, the reader may eaſily diſcern what is to be anſwered. Nor does our author here ſhew us any way, by which his imaginary ſtrings ſhould take ſuch faſt hold of the ſides of the glaſs, as to be able to draw them together, notwithſtanding the reſiſtance they find from the cloſe texture of the body to be broken.

EXPERIMENT IX.

OUR explication of the ninth experiment he handles very ſeverely: fer having briefly recited it, he propoſes his objection againſt it thus; *Sed profecto nimis longè videtur hoc à veritate recedere: poteſtque vel inde ſolum ſatis refutari; quia ſi tanta ſit preſſura aëris ſic per tubum illum in phialam deſcendentis, ut ipſam phialam perfringat, deberet profecto incluſam, cui immergitur ille tubus, valde quoque ante fractionem phialæ commovere, bullulaſque in eadem excitare, &c. ut conſtat, ſiquis, inſufflando per illum tubulum, aquam vel mediocriter ſic premat. At certum eſt aquam, antequam frangatur ſic phiala, nec tantillum moveri; ut experienti conſtabit.* ‘ But truly this ſeems too ‘ far removed from truth, and may be by this alone ſufficiently refuted. Becauſe if ‘ the preſſure of the air, which deſcends by that tube into the phial, be ſo great as ‘ to break the phial itſelf, it ought certainly, before the breaking of the phial, very ‘ much to move the water, in which the tube is immerſed, and to excite bubbles in ‘ it, *&c.* as appears, if any one blowing through that tube doth make but an ordi‘ nary preſſure upon the water. But it is ſure, that the water, before the phial is ‘ broken, doth not move at all; as the experimenter will find.’

BUT I do confeſs, I do for all this think our explication more true, than well conſidered by our author. For the putting of water into the phial, that was broken, was done (as clearly intimated in the beginning of our narrative) upon a particular deſign (as indeed we tried divers other things with our engine, not ſo much with immediate reference to the ſpring of the air, as to make uſe of ſuch trials in ſome other of our writings). And accordingly in the ſecond trial mentioned in the ſame experiment, the water was omitted. But, notwithſtanding this water, the ſides of the glaſs being expoſed to the preſſure of the atmoſphere, had that whole preſſure againſt them before the exhauſtion of the receiver; ſo that there needed no ſuch blowing in of the air afreſh as our author imagines, to effect the breaking of the phial, it being ſufficient for that purpoſe, that the preſſure againſt the convex ſuperficies of it was taken off by the exhauſtion of the receiver, the preſſure againſt the concave ſuperficies remaining as great as ever. And therefore we need not altogether deny what the examiner ſays, that *licet clauſus ſuperne fuiſſet tubulus ille, eodem tamen modo fracta ſine dubio fuiſſet phiala:* ‘ Though the tube had been ſhut at the top, the phial had doubtleſs been ‘ broken after the ſame manner.’ For, ſince in ſuch caſes the air (as we have often taught) is ſhut up with the whole preſſure of the atmoſphere upon it, it may almoſt as eaſily break the glaſs as if it were unſtopt. And accordingly we mention, in the 36th experiment, the breaking of a thin glaſs hermetically ſealed, upon the receſs of the ambient air. But, how confidently ſoever our author ſpeaks, I thought fit to add the word *almoſt*, becauſe we obſerved in the 39th experiment, that ſuch thin phials (and thick ones will not break) are ſubject upon the withdrawing of the ambient air to retch a little, whereby the ſpring of the air within the phial might in ſome caſes (I ſay, in ſome) be ſo far weakened, as not to be able to break it, unleſs aſſiſted by the preſſure of the atmoſphere, wherewith it communicates, and which leans upon it. And when the phial does actually begin to break, then the purſuing preſſure of the outward air upon the yielding air within the phial may help to throw the parts of the glaſs more forcibly aſunder.

ALL the experiments from the 9th to the 17th excluſively our examiner leaving uncenſured, we may with him advance to the conſideration of the 17th.

The 17*th* EXPERIMENT *defended.*

IN this we relate how, when we made the Torricellian experiment, we ſhut up the reſtagnant mercury together with the tube and the ſuſpended mercurial cylinder (of about 29 inches) in our receiver, that by drawing off and letting in the air at pleaſure upon the reſtagnant mercury, and conſequently weakening and increaſing its preſſure, we might make it more clearly appear than hitherto had been done by experiment, that the ſuſpenſion of the mercurial cylinder, and the height of it, depended upon the greater or leſſer preſſure of the air. But againſt our explication of this experiment (which has had the good fortune to convince and ſatisfy many ingenious men) the examiner objects nothing in particular, contenting himſelf to have recourſe here alſo to his Funiculus. Yet two obſervations of ours he is pleaſed to take notice of.

THE firſt is, that though the quickſilver were exactly ſhut up into our receiver after the manner newly declared, yet the ſuſpended quickſilver did not deſcend: whence having ſaid, that I argue, that it is now ſuſtained not by the counterpoiſe of the atmoſphere, but by the ſpring of the air ſhut up in the receiver, he ſubjoins only this; *Sed rectius ſane infertur, cylindrum illum nihil ibidem antea præſtitiſſe:* ‘But it is more rightly thence inferred, that that cylinder did nothing there before.’ But whether this be not *gratis dictum*, we leave the reader to collect from what we have formerly diſcourſed in the ſecond part of this defence of the ſpring of the air, eſpecially from that experiment, by which it appears, that ſpring may ſuſtain a far higher cylinder of quickſilver.

IN the ſecond obſervation he mentions of ours, he ſummarily recites our explication of the deſcent and aſcent of the mercury in the tube, by the debilitated and ſtrengthened ſpring of the air. But without finding fault with our application of that principle to the phænomena, he ſays, that he has ſufficiently refuted the principle itſelf in the fourth chapter (which how well he has done, we have already ſeen); and therefore explicates the matter thus: *Dico igitur* (ſays he) *argentum per illam exhauſtionem ſic in tubo deſcendere, quod deorſum trahatur ab aëre, qui incumbit argento reſtagnanti: ſiquidem incumbens ille aër jam per exhauſtionem valde rarefactus & extenſus ſeſe vehementer contrahit, & contrahendo conatur etiam ſubjectum ſibi argentum reſtagnans è ſuo vaſculo elevare; unde fit ut (argento illo reſtagnante minus jam gravitante in fundum ſui vaſculi) argentum quod eſt in tubo deſcendat; ut per ſe patet. Adeoque mirum non eſt quod, ingrediente poſtea aëre externo, rurſum argentum aſcendat, cum per illum ingreſſum vis illa ſic elevans argentum reſtagnans debilitetur:* ‘I ſay then, that the quickſilver doth by that exhauſtion ſo deſcend in the tube, becauſe it is drawn downwards by the air incumbent upon the reſtagnant quickſilver. For that incumbent air, being by its exhauſtion greatly rarefied and extended, vehemently contracts itſelf, and by this contraction doth endeavour to lift the reſtagnant mercury out of its veſſel; whence it comes to paſs, that (the reſtagnant mercury now leſs gravitating upon the bottom of its veſſel) the quickſilver in the tube muſt deſcend, as is manifeſt in itſelf: ſo that it is no wonder, that the external air afterwards entering, the quickſilver again aſcends, ſeeing by that ingreſs the force, which elevates the reſtagnant quickſilver, is weakened.’ But this explication ſuppoſing ſuch a Funiculus as we have already ſhewn to be but fictitious, the reader will eaſily gather what is to be judged of it from what has been already delivered. Wherefore I ſhall only ſubjoin, that by this explication, were it admitted, there is only an account given of that part of our ſeventeenth experiment, which relates to the deſcent of the mercury below its wonted height, and its reaſcent to it. But as for our having, by the forcing in ſome more air into the receiver, impelled the quickſilver to a conſiderably-greater height than it is wont to be ſuſtained at in the Torricellian experiment, I confeſs I underſtand not how the examiner gives an account of it in the following words (which are immediately annexed to thoſe we laſt recited of his, and which are all that he employs to explicate this notable phænomenon:) *Atque hinc etiam redditur ratio alterius, quod ibidem quoque notatur, nempe quod per violentam intruſionem aëris externi in recipientem aſcenderit argentum notabiliter ſupra digitos* 29½. *Nam ſicut per extractionem aëris argentum infra ſtationem detrahitur, ſic etiam per intruſionem novi ſupra eandem elevabitur:* ‘And hence is a reaſon alſo given of another thing, which is there noted, namely, ‘that by the violent intruſion of the external air into the receiver the quickſilver ‘aſcended conſiderably above 29 inches and an half. For as by the extraction ‘of the air, the quickſilver is depreſt below its ſtation, ſo by the intruſion of ‘new air it is elevated above it.’ For in this paſſage I ſee not how he himſelf does not rather repeat the matter of fact, than give any account how it is performed. And if it be alleged on his behalf, that according to his principles it may be ſaid, that, upon the preſſure of adventitious air upon the reſtagnant mercury, the Funiculus in the tube, that was not able before to draw it up above 29½ inches, is now enabled to draw it up higher; I demand upon what account this new air does thus preſs againſt the reſtagnant mercury, and impel up and ſuſtain that in the tube. It will not be ſaid, that it is by its weight; for as much mercury as may be thus impelled up above the uſual ſtation will weigh a great many times more than the air forced into the receiver. And therefore it remains, that the additional air counterpoiſes the additional-mercury by its ſpring. And if we conſider withal, that there is no reaſon to doubt (eſpecially conſidering what we have formerly delivered upon trial touching the power of compreſt air to impel up quickſilver) but that, had we not been afraid of breaking our veſſel, we might by forcing more air into the receiver have impelled it up to the top of the tube, and kept it there; we ſhall ſcarce deny, but that, ſuppoſing there could be no ſuch Funiculus as our examiner's *in rerum natura*, the preſſure of the incumbent air alone might ſuffice to keep a correſpondent cylinder of mercury ſuſpended; and that without any attraction of the reſtagnant mercury by a Funiculus of violently-diſtended air in the receiver, the quickſilver in the tube may be made to reſt at any height greater or leſſer, provided it exceed not 30 inches, only becauſe its weight is juſt able to counterbalance the preſſure of the contiguous air.

I KNOW not whether I may not add (to expreſs an unwillingneſs to omit what ſome may think proper to do my adverſary right) that it may be ſaid for the examiner, that he in the 11th page acknowledging with us a power in the air to recover its due extenſion, if it be crouded into leſs room than its diſpoſition requires; a man may from that principle ſolve the phænomena in queſtion, by ſaying, that the air in the receiver being forcibly compreſſed by the intruſion of freſh air into the ſame veſſel, does by its endeavour to recover its due expanſion preſs upon the reſtagnant mercury, and force up ſome of it into the tube. But this explication, though it agree with what the author teaches in a place very diſtant from his notes upon our 17th experiment, now under debate; yet ſtill it is not clear to me, how, by what he ſays in theſe notes, the phænomenon is accounted for as the word *hinc* imports it to be. But otherwiſe I need not quarrel with the explication, ſince without recurring to the Funiculus for the ſuſtaining of the additional mercury, the ſolution of the phænomenon is given upon the ſame principle that I employ.

EXPERIMENT XVIII.

OUR examiner in his animadverſion upon the 18th experiment, having recited my conjecture as the cauſe, why a cylinder of mercury did in winter riſe and fall in the tube ſometimes as water is wont to do in a weather-glaſs, according to the laws of heat and cold, and ſometimes quite contrary thereunto; adds, that this experiment does ſtrongly enough overthrow our hypotheſis of the atmoſpherical cylinder, and clearly ſhew, that the quickſilver is not ſuſtained by it: *Nam* (ſays he) *ſi hoc ab eo ſuſtentatum fuiſſet, debuiſſet potius frigidiore tempore aſcendere quam deſcendere, eo quod aër tunc multo denſior eſſet & gravior. Itaque non ſuſtentatur argentum ab aëris æquipondio, ut aſſeritur:* ‘For if it were kept up by that, it ought rather to aſcend ‘than deſcend in colder weather, becauſe the air then would be more denſe and ‘heavy. Therefore the quickſilver is not upheld by the æquilibrium of air, as is ‘aſſerted.’ And by the ſame argument he concludes againſt the mercury's being ſuſtained by the ſpring of the air. But in his animadverſions upon this experiment he ſeems to have been too forward to reprehend; for he neither well confutes my conjecture, nor ſubſtitutes ſo much as a plauſible one in the ſtead of it. And as to his objection, I anſwer,

First, That it doth not conclude; becauſe that as ſometimes the quickſilver in the tube did riſe in warmer, and fall in colder weather; ſo at other times it did rather emulate the aſcent and deſcent of water in a weather-glaſs.

Secondly, Though it be true, that cold is wont to condenſe this or that parcel of air, and that a parcel of air may be made heavier by condenſation; yet that is in regard of the ambient air, that retains its wonted laxity, in which the condenſed air is weighed. But our author has not yet proved, that in caſe the cold of the winter ſhould condenſe the whole incumbent atmoſphere, it would then gravitate ſenſibly more upon the reſtagnant quickſilver than before. As a pound of wool will not ſenſibly vary its weight, though the hairs, whereof it is compoſed, be made to lie ſometimes in a looſer, ſometimes in a cloſer order.

And, thirdly, his objection does as little agree with his doctrine as with my conjecture: for in the 50th page, where he gives us an account, according to his principles, of the riſing and falling of water in a weather-glaſs, and compares it with the ſuſpenſion of quickſilver, he tells us, *Hinc fit quod, contracto hoc Funiculo per frigus, aqua illa tempore frigido aſcendat, deſcendat autem tempore calido, eo quod per calorem Funiculus ille dilatetur:* ‘Hence it comes to paſs, that this Funicle being con‘tracted by the cold, the water doth aſcend in cold weather; but doth deſcend in ‘hot, becauſe by heat the Funicle is dilated.’ So that, according to the examiner himſelf, the quickſilver ought to have aſcended in colder, and deſcended in warmer weather. Now, although I propoſed my thoughts of the difficult phænomenon under conſideration but as a conjecture, and therefore ſhall be ready to alter them, either upon further diſcovery or better information; yet I ſee not why it ſhould be poſtponed to the examiner's, who, though he rejects our explication, ſubſtitutes no other than what may be gathered from theſe words; *Ego certè non dubito, quin dentur hujuſmodi occultæ cauſæ, quibus Funiculus ille ſubtilis, quo in tubo ſuſpenditur argentum (ut dictum eſt capite decimo) modo producatur, modo abbrevietur, &c. ſicque argentum nunc demittat, nunc elevet:* ‘I truly do not doubt, but there are ſome ſuch occult cauſes, by which the ſlender Funicle, that ſuſpends (as we mentioned in the tenth chapter) 'the quickſilver in the tube, is ſometimes lengthened, ſometimes ſhortened, and ſo 'doth ſometimes let down, and ſometimes lift up the quickſilver.' For, ſince we have made it probable, that the copious fumes ſometimes ſuddenly aſcending into the air, and rolling up and down in it, ſometimes ſenſibly altering (if good authors may be credited) the refraction of it, and ſince ſome other cauſes, mentioned in our eighteenth experiment, may alter the denſity and gravity of the air, that leans upon the reſtagnant mercury; I ſuppoſe the reader will think it more intelligible and probable, that alterations, other than thoſe produced by heat and cold, may happen to the incumbent atmoſphere, which freely communicates with the neighbouring air, and may thereby become ſometimes more ſtufft, and ſometimes more deſtitute of adventitious exhalations, than that ſuch changes ſhould happen to a Funiculus included in glaſs; which, according to our author, is impervious to the ſubtileſt ſteams that are, and concerning which he offers not ſo much as a conjecture, upon what other account it can happen to be ſometimes contracted, and ſometimes ſtretched.

EXPERIMENT XIX.

UPON this the examiner has only this ſhort animadverſion; *In decimo nono oſtendit aquam eodem modo per exhauſtionem recipientis deſcendere, quo in præcedente deſcendere oſtenderat argentum vivum; cujus cum eadem ſit ratio non eſt cur amplius ei inſiſtamus.*' 'In the 19th he ſhews, that water doth in the ſame manner deſcend upon 'the exhauſting the receiver, as he had ſhewn quickſilver in the foregoing chapter to 'deſcend. Of both which, ſeeing there is the ſame cauſe, there is no reaſon we ſhould 'any longer inſiſt on this.' In which words, ſince he offers nothing new or peculiar to ſhew any incongruity in our explication to our principles, which agree very well with the new phænomena of the experiment; we are content to leave the reader to judge of the hypotheſes themſelves, which of the two is more probable, either ours, that only requires, that the air in the receiver ſhould equally reſiſt a cylinder of water and of quickſilver, when their weight is but the ſame, though their altitudes be not; or the examiner's, which exacts, that (according to what we formerly elſewhere noted) bodies of ſuch differing nature and texture as quickſilver and water ſhould need but juſt the ſame weight or ſtrength to rarefy them into a Funiculus.

EXPERIMENT XX.

IN his examen of this experiment our author makes me infer from the phænomena he repeats, that not only the air, but the water alſo has a ſpring. But though I ſuſpect not, that he does wilfully miſtake my ſenſe, yet by what I write in this and the following experiments the reader may well enough perceive, that I ſpoke but very doubtfully of a ſpring in the water; nay, and that I did in the 58th page expreſsly teach, that the intumeſcence of it might (at leaſt in great part) proceed from that of the ſmall parcels of air, which I thought to be uſually harboured in the body of that liquor.

BUT whereas I aſcribe the appearance of the bubbles in the water to this, that upon the exhauſtion of ſome of the air incumbent on the water, the preſſure of what remains is much debilitated, whereby the little particles of air lurking in the water are allowed to expand themſelves into bubbles; he rejects this explication as manifeſtly

Sed contra manifeſte.

falſe: *Nam* (ſays he) *ſi ita fieret, deberent profecto hujuſmodi bullulæ, non è fundo vaſis ſic aſcendere (uti tam in hoc quam in ſequentibus experimentis, in quibus de iſtis bullis ſemper agitur, aſſeritur) ſed è ſuperiore parte aquæ, ubi minus premuntur, ut per ſe eſt manifeſtum.* ‘ For if it were done ſo, theſe bubbles ought not ſo to have aſcended from the bottom of the veſſel (as it is aſſerted they did, both in this and the following experiments that treat of bubbles) but from the upper part of the water, where they are leſs compreſt; as it is apparently manifeſt.’ But why he ſhould be here ſo peremptory, I confeſs I do not, for all this objection, yet ſee: for in the bottom of the next page he ſays, he will not deny, but that aërial particles latitant in the other parts of the water (he had before ſpoken of the bottom of it) may be extended into bubbles by his way of rarefaction. And that we particularly mentioned the riſing of bubbles, even from the bottom of the water, was becauſe that circumſtance ſeemed to deſerve a peculiar note; and not (as he ſeems to imagine) as if the bubbles did not alſo riſe from the ſuperior parts of the liquor, ſince we did take notice of it about the middle of the 56th page *. And we often in this and the following experiments obſerved, that the aſcending bubbles grew bigger the nearer they came to the top. Which agrees more clearly with our hypotheſis, wherein their conſpicuous ſwelling, as they aſcend, is attributed more to the leſſening of the preſſure of the incumbent air, than to the decrement of the weight of the incumbent water (ſince when the ſurface of this liquor is leaned upon by the atmoſphere, the aſcending bubbles ſcarce ſenſibly increaſe in veſſels no deeper than ours) than with the explication, which the examiner gives in theſe words; *Reſpondeo, aquam per illam aëris exhauſtionem non ſponte ſic aſcendere, ſed ſurſum violenter trahi, ac elevari à rarefacto illo aëre ſeſe contrahente. Quemadmodum enim aqua aliqualem patitur compreſſionem (ut experientiâ conſtat) ita & aliqualem quoque hic patitur diſtenſionem. Atque hinc clarè patet, cur potius à fundo vaſis quam à parte aquæ ſuperiore oriantur hujuſmodi bullæ. Cum enim vehemens illa ſuctio conetur aquam à fundo phialæ elevare, naſcitur ibidem ſubtilis quædam materia, quæ in bullas converſa ſic aſcendit, uti capite decimo quinto in quarto experimento dictum eſt.*. ‘ I anſwer, that the water, upon exhauſtion of the air, doth not ſo aſcend of its own accord, but is violently drawn or lifted upwards by that rarefied air contracting itſelf. For as water doth ſuffer ſome compreſſion (as appears by experience) ſo here alſo it ſuffers ſome diſtenſion. And hence it is clearly manifeſt, why theſe bubbles ſhould ariſe rather from the bottom of the veſſel, than from the upper part of the water. For when that vehement ſuction doth endeavour to elevate the water from the bottom of the phial, there ariſes there a certain ſubtile matter, which being turned into bubbles, doth ſo aſcend, as is mentioned in the 15th chapter and the 4th experiment.’ For, whatever he may think, it does not hence ſo clearly appear, how the endeavour only of the Funiculus to draw up the water from the bottom of the phial, to which, that endeavour notwithſtanding, it remains contiguous, ſhould generate in ſome parts of the bottom of the glaſs, and not in others, ſuch a ſubtile matter as he tells us of. And I ſuppoſe the reader will, as well as I, wiſh he had more intelligibly declared, how this ſtrange generation of ſubtile matter comes to be effected. And I preſume it will likewiſe be expected, that he alſo declare, why both in our caſe and in the Torricellian experiment the bubbles grow ſo much larger by being nearer the top of the liquor; if, as he rejects our explication of this circumſtance, the effect of the ſuction he ſpeaks of be greater upon the lower part of the liquor than the upper, to which alone nevertheleſs his Funiculus, that is ſaid ſo to draw the liquor, is contiguous.

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* See alſo in the 43d Experim. theſe paſſages,—*And this efferveſcence was ſo great in the upper part of the water, &c.* As alſo,—*Efferveſcence was confined to the upper part of the water, unleſs, &c.*

OUR author making no particular objection againſt the ten following experiments, we alſo ſhall paſs them by, and fall with him upon the conſideration of the 31ſt. experiment.

EXPERIMENT XXXI.

UPON this our author having recited our conjecture, as the cauſe why two very flat and ſmooth marbles ſtick ſo cloſely together, that by lifting up the uppermoſt you may take up alſo the lowermoſt, approves my way of examining that conjecture. But whereas I ſay, that the reaſon, why, though the marbles were kept together by the preſſure of the ambient air, yet they did not fall aſunder in our exhauſted receiver, no not though a weight of four ounces were hung at the lower ſtone, might be, that by reaſon of ſome ſmall leak in the receiver the air could not be ſufficiently drawn out; yet he tells us, with his wonted confidence, *Certum eſſe ſententiam illam vel hoc ſolo experimento ſatis refelli:* ‘ It is certain, that that opinion is ſufficiently ‘ refuted by this ſingle experiment.’ But poſſibly he would have ſpoken leſs reſolutely, if he had made all the trials about the adheſion of marbles, that we relate ourſelves to have made in the ſhort hiſtory we have publiſhed of *fluidity and firmneſs*. For our examiner ſpeaks, as if all that we aſcribe to the air in ſuch experiments were to ſuſtain the lower marble with the weight perhaps of a few ounces; whereas in caſe the air be kept from getting in at all between the ſtones, it may (according to our hypotheſis) ſuſtain a weight either altogether, or well-nigh equal to that of a pillar of air as broad as the baſis of the lower marble, and as long as the atmoſphere is high, or to the weight of a pillar of quickſilver of the ſame thickneſs, and about thirty inches long; theſe two pillars appearing by the Torricellian experiment to counterpoiſe each other. And therefore ſince in the ſeventeenth experiment, when we had exhauſted our receiver as far as we could, there remained air enough to keep up in the tube a cylinder of about an inch long of quickſilver; and ſince the broader the contiguous marbles are, the greater weight faſtened to the lowermoſt may be ſuſtained by the reſiſtance of the air (as is obvious to him, that conſiders the hypotheſis, and as we have proved by experiment in the forementioned tract) it need be no wonder, that the air remaining in the receiver ſhould be able to ſupport the lowermoſt marble, whoſe diameter was near two inches, and a weight of four ounces, thoſe two weights being inferior to that of a mercurial cylinder of that diameter and an inch in length. And though it were not, yet we are not ſure, that the receiver was as well emptied when we made the 31ſt experiment, as when we made the 17th. And (if my memory does not much miſinform me) it was with the ſame pair of marbles, that in the preſence of an illuſtrious aſſembly of virtuoſi (who were ſpectators of the experiment) the uppermoſt marble drew up the lowermoſt, though that was clogged with a weight of above 430 ounces.

As for the account the examiner ſubſtitutes of our phænomenon, I know not whether many readers will acquieſce in it. For, not to inſiſt upon the objection, which himſelf takes notice of, that, according to him, the diſtended air in the receiver ſhould draw aſunder the adhering marbles; his explication ſuppoſes, that there cannot naturally be a vacuum: whence he infers that, *Neceſſe erat, ut lapis ille non aliter deſcenderet, quàm relinquendo poſt ſe tenuem hujuſmodi ſubſtantiam, qualis ab argento vivo aut aquâ ſic deſcendentibus relinqui ſolet.* ‘ It muſt needs be, that that ſtone could not ‘ otherwiſe deſcend, than by leaving behind it ſuch a thin ſubſtance, as is left by quick-

‘ ſilver or water deſcending in like manner.’ But whereas he adds, that the cauſe of the obſtinate adheſion we meet with in our caſe is, that ſuch a ſubſtance is far more difficult to be ſeparated from marble than from quickſilver, or any other kind of body; that aſſertion is precarious. And though I have tried experiments of this nature with ſtones of ſeveral ſizes, perhaps an hundred times, yet I never could find, that by their coheſion they would ſuſtain a weight greater than that of a pillar of the atmoſphere that preſt againſt the lowermoſt: which is a conſiderable circumſtance, that much better agrees with our explication than our adverſary's. And whereas he further ſays, *Unde exiſtimo planè, ſi perfectè complanata fuerint duo marmora ſic conjuncta, ita ut nullus omnino aër inter utrumque mediaret, non poſſe ea ullis humanis viribus ab invicem divelli.* ‘ Whence I plainly conceive, that if two perfectly poliſhed marbles ‘ were ſo joined, that no air at all were left between them, they could not be drawn ‘ aſunder by all the power of man.’ I hope I need not tell the reader, that whether or no this agree with what he had immediately before taught of the ſeparableneſs of a ſubtile ſubſtance, even from marble, ſo bold and improbable an aſſertion requires the being countenanced with a much better proof, than the only one he ſubjoins in theſe words; *Uti etiam confirmat exemplum, quod ibidem adducit author de lamina ænea, tabulæ cuidam marmoreæ ita adhærente, ut à lacertoſo juvene, de ſuis viribus gloriante, non potuerit per annulum centro ejus affixum inde elevari.* ‘ Which alſo is confirmed by the ‘ example the author there brings of a braſs plate ſticking ſo cloſe to a marble table, ‘ that by a luſty youth, who boaſted of his own ſtrength, it could not be lifted off ‘ by a ring fixed to its center.’ For ſure there is great odds betwixt the ſtrength of a man unaſſiſted by an engine, and the utmoſt extent of human power. And indeed, according to our hypotheſis, and without having recourſe to nature's dreading of a vacuum, the caſe is clear enough. For, ſuppoſing the plate to be of any conſiderable breadth, the pillar of the atmoſphere, that leaned upon it, and muſt at the inſtant of its deſerting the ſuperficies of the table all at once be lifted up with it, may well exceed the force of a ſingle man, eſpecially in an inconvenient poſture; ſince by the coheſion of a pair of marbles of about three inches diameter, I did, with my own hands, take up above a thouſand and three hundred ounces.

EXPERIMENTS XXXII, *and* XXXIII.

AGAINST our explication of theſe two, which our author examines together, he objects nothing peculiar, but contents himſelf to explicate them by his Funiculus. Wherefore neither ſhall we need to frame any peculiar defence for it, eſpecially if the reader will be pleaſed to refer hither as much of what we oppoſed to his animadverſion on the third experiment, as is juſtly applicable to our preſent controverſy. Our author indeed endeavours to prove his explication by ſaying, that the diſtended air in the exhauſted cylinder draws up the ſucker with the annexed weight: *Eodem ferè modo, quo videmus in cucurbitulis dorſo ægrotantis applicatis, in quibus, extincta jam flamma, rarefactus aër ſe contrahens carnem tam vehementer, uti videmus, elevat attrahitque intra cucurbitulam.* ‘ Almoſt in the ſame manner as we ſee in cupping-‘ glaſſes applied to a patient's back, in which the flame being extinct, the rarefied air ‘ contracting itſelf doth ſo vehemently (as we ſee) lift up, and draw the fleſh within ‘ the glaſs.’ But that phænomenon is eaſily enough explicable in our hypotheſis, by ſaying, that upon the vaniſhing of that heat, which ſtrengthened the preſſure of the included air, the ſpring of it grows too weak to reſiſt any longer the preſſure of the ambient air; which thereupon thruſts the fleſh and neighbouring blood of the patient into the cupping-glaſs, almoſt after the ſame manner, as we formerly taught the pulp of the finger to be thruſt into the deſerted cavity of the glaſs-tube in the Torricellian experiment.

EXPERIMENTS XXXIV, XXXV, *and* XXXVI.

TO theſe our author ſaying nothing but this, *In his tribus nihil peculiariter occurrit hic explicandum, cujus ratio ex jam dictis non facile pateat:* ‘ In theſe three ‘ there is nothing occurs to be peculiarly here explicated, the account of which is ‘ not eaſy from what is already delivered:’ we alſo may be allowed to pretermit them, and paſs on to

EXPERIMENT XXXVII.

OF the appearance of light or whiteneſs, mentioned in this experiment, the examiner confeſſes, that we have aſſigned a cauſe probable enough, by referring it to the vehement and ſudden commotion of the included air. And indeed though I do ſtill look upon ſome of the things; that I heſitantly propoſed about this difficult phænomenon, but as mere conjectures, and though he annexes his explication of it, yet I ſee not but that it is coincident with ours, or not better than it. For, to what I ſaid of the commotion of the parts of the air, he adds only in two or three ſeveral places their being violently diſtended; which how it improves the explication of the phænomenon, I do not readily ſee. And whereas he ſubjoins, *Exiſtimo autem dicendum potius candorem illum eſſe lumen quoddam reflexum, quam innatum, eo quòd (uc teſtatur author) in tenebris non appareat, ſed ſolum de die aut accenſa candela:* ‘ But I think, ‘ that whiteneſs ſhould be rather called a reflex than an innate light, becauſe, as the ‘ author bears witneſs, it appears not in the dark, but only in the day, or by candle- ‘ light:’ I preſume the attentive reader will eaſily diſcern, that his opinion is much what the ſame that I propoſed and grounded on the ſame reaſon. But the chief difficulty in this abſtruſe phænomenon, namely, why we meet with it but ſometimes, our examiner's explication leaves untouched.

EXPERIMENTS XXXVIII, *and* XXXIX.

AGAINST theſe our author makes no peculiar objections.

EXPERIMENTS XL, *and* XLI.

BUT in his animadverſions upon theſe, having told the reader, that I ſeem to aſcribe the ſudden extinction of the included animals to the exceſſive thinneſs of the air remaining in the receiver, made by the receſs of what was drawn out, unfit for reſpiration; he adds reſolutely enough, *Verum impoſſibile videtur, ut hujuſmodi animalcula ob ſolum defectum craſſioris aëris tam cito moriantur:* ‘ But it ſeems impoſſible, ‘ that ſuch animals ſhould die ſo ſoon only for want of a thicker air.’ But gives no other reaſon, than that they die ſo ſoon, which is no more than what he ſaid in the newly-cited words, and beſides is grounded upon ſomething of miſtake. For the creatures he mentions were a bee, a fly, and a caterpillar, and thoſe included too in a ſmall receiver, which could be ſuddenly exhauſted: and theſe indeed became moveleſs within a minute of an hour; but that minute was not (as the word is often uſed to ſignify in Engliſh) a moment, but the ſixtieth part of an hour. And though theſe inſects did in ſo ſhort a time grow moveleſs, yet they were not ſo ſoon killed; as appears by the narrative. The ſanguineous animals, that did indeed die, were killed more ſlowly. And I remember, that having purpoſely inquired of a man (uſed to go under water by the help of an engine, wherein he could carry air with him to the bottom of the ſea) how long he could endure, before he was accuſtomed to dive, without breathing or the uſe of a ſpunge*? he told me, that at firſt he could hold out about two or three minutes at a time: which made me think, that divers become able to continue under water ſo long, either by a peculiarly-convenient conſtitution of body, or by a gradual exerciſe. And I am apt to think, that he did, as men are wont to do, when he ſaid two or three minutes, mean what is indeed a much ſhorter time than that, when exactly meaſured, amounts to. For, having purpoſely made trial upon a couple of moles, that were brought me together alive, one of them included in a ſmall, though not very ſmall, receiver, was between two and three minutes in killing; whereas the other being immediately after detained under water, did not there continue full a minute and a quarter, before it finally ceaſed from giving any ſign at all of life. By which trial it may appear, that it is not impoſſible, that the want of reſpiration ſhould diſpatch an animal in as little time as is mentioned in the experiment I am now defending. And indeed our author either ſhould have proved, that it is not poſſible for the want of air to deſtroy animals ſo ſoon, or ſhould have given us ſome better account of the phænomenon. For whereas he teaches us, that, according to his doctrine, the little animals above-mentioned were ſo ſoon killed; *Quia per rarefactam illum aërem ſeſe contrahentem extractus ſit eorum halitus:* ‘Becauſe by the ſelf-contraction of the rarefied air their breath is drawn out of their ‘bodies:’ I ſee not, that hereby, if he explicate the phænomenon otherwiſe than we, he explains it better; for he ſeems to ſpeak, as if he thought, his halitus to be ſome peculiar part of the animal, in which his life reſides. And beſides, he ſeems not to conſider, that whereas, according to me as well as according to him, the air contained in the lungs (ſuppoſing theſe animalcula have any) muſt in great part paſs thence into the receiver; (for whether that be done by the ſpring of the air itſelf, that was harboured in the lungs, or the traction of the more rarefied air in the receiver, is not material in our preſent caſe;) the examiner muſt, as well as I, render a reaſon, why the extenuation or receſs of the halitus ſhould cauſe the haſty death of the included animals; and condemning my conjecture, he ought to have ſubſtituted another reaſon. And though he ſubjoins theſe words, and concludes with them; *Atque hinc quoque ortæ ſunt vehementes illæ convulſiones, quas ante mortem paſſas eſſe aviculas quaſdam memorat ibidem author:* ‘And thence alſo aroſe thoſe vehement convulſions, which ‘the author there mentions certain ſmall birds to have endured before their ‘death:’ yet I doubt not but the reader will think it had not been amiſs, that the author had more intelligibly reduced theſe tragic ſymptoms from his aſſumption, for the ſake of thoſe, that are not anatomiſts and phyſicians enough to diſcern how his Funiculus could produce theſe effects.

For my part, as in the 41ſt experiment I tendered my thoughts concerning reſpiration but doubtingly, ſo I am yet unwilling to determine reſolvedly in a matter of that difficulty.

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* See more concerning this objection in the anſwer to it, as it is propoſed by Mr. *Hobbes*.

EXPERIMENTS XLII, *and* XLIII.

IN his examen of theſe two laſt of our phyſico-mechanical experiments, the author contents himſelf to endeavour to explicate the phænomena recited in them by the contraction of the rarefied air; which, according to him, endeavours to draw up the ſubjacent water out of the phial, whereby it vehemently diſtends the parts of that water, as he taught in the like caſe upon the 20th experiment. But ſince we have already conſidered his animadverſion upon that, although this preſumed diſtenſion of the water is not viſible, that we have obſerved, when cold water, that has been at firſt freed from his interſperſed air, is put into the receiver, notwithſtanding that the Funiculus ſhould in that caſe alſo diſtend it; we are ſo afraid of tiring out the reader's patience by the frequent repetition of the ſame things, that we will leave it to him to judge, which of the two explications, the examiner's or ours, is to be preferred, without troubling him and ourſelves with defence of accounts, againſt which our adverſary does not here make any peculiar objections.

AND thus have we by God's aſſiſtance conſidered what the examiner hath been pleaſed to oppoſe, either againſt our particular explications, or againſt the hypotheſes, that divers of them ſuppoſe: wherein I have been the more particular and prolix, becauſe I would willingly excuſe myſelf and others from the trouble of any more diſputes of this kind. I hope there is not in my anſwers any thing of aſperity to be met with; for I have no quarrel to the perſon of the author, or his juſt reputation; nor did I intend to uſe any more freedom of ſpeech in the anſwering his objections, than his reſolute way of propoſing divers of them made it, on thoſe occaſions, needful for the caution of readers, who are not acquainted with our differing ways of writing, and perhaps have not obſerved that ſome men are wont to conſider as much what they propoſe but with a perhaps, or ſome ſuch expreſſion of diffidence, as others do what they deliver far more reſolutely. And though being very far from being wedded to my opinions, I am ſtill ready to exchange them for better, if they ſhall be duly made out to me (which I think it poſſible enough they may hereafter be); yet peradventure the reader will think with me, that the examiner has not given me cauſe to renounce any of them, ſince the objections he has propoſed againſt me have been ſufficiently anſwered, and ſince the hypotheſis he would ſubſtitute in the room of ours (beſides that it is partly precarious) ſuppoſes things, which divers of the eminenteſt wits of our age (otherwiſe of differing opinions) profeſs they cannot admit, or ſo much as underſtand: whereas the weight and ſpring of the air are not denied by our author himſelf, and are demonſtrable by experiments, that are not controverted betwixt us. Which things I repreſent for the defence of what I think the truth, and not to offend my learned adverſary, who ſhall have my free conſent to be thought to have failed rather in the choice than in the management of the controverſy. Though, ſince this paſſes for his firſt book, and ſince, conſequently, he is not like to have been provoked, or engaged in point of reputation, to challenge me or any of thoſe far more eminent perſons he has named among his adverſaries, I am induced by the ſeverity wherewith I have known eminent Virtuoſi ſpeak of his attempts, and particularly of his Funiculus, to fear, that ſome of thoſe he has needleſsly oppoſed, will be apt to apply to him that of St. *Auſtin* againſt ſome of his adverſaries, that had diſputed againſt him with much more ſubtility than reaſon, *In mala cauſa non poſſunt aliter, at malam cauſam quis eos coëgit habere?* 'In a bad 'cauſe they can do no other; but who compelled them to undertake a bad cauſe?'

But this notwithſtanding, I am, as I was going to ſay, content, my adverſary ſhould be thought to have ſaid for his principles as much as the ſubject will bear; nor would I have it made his diſparagement, that I have declared that his whole book has not made me depart from any of my opinions or explications, ſince his hypotheſis and mine being inconſiſtent, it may be looked upon as a ſign rather that each of us have, than that either of us have not, reaſoned cloſely to his own principles, that the things we infer from our contrary ſuppoſitions do ſo generally diſagree.

An Explication of RAREFACTION.

THE chief arguments of the author of a certain treatiſe *De corporum inſeparabilitate*, whereby he endeavours to invalidate the hypotheſis of the weight and ſpring of the air, and to ſet up and eſtabliſh inſtead thereof an unintelligible hypotheſis of attraction, performed by I know not what ſtrange imaginary Funiculus, are only five; two againſt the former, and three for the latter. The firſt of which is, that the weight and ſpring of the air are not ſufficient to perform the effects aſcribed to them: the ſecond, that could they be performed by that hypotheſis granted, yet the way of this ſtrange ſpring itſelf is not intelligibly explained or explicable by the defenders of it. Now the former of theſe being little elſe but a bare affirmation, and the latter bearing ſome ſhew of demonſtration, I ſhall endeavour to examine it as I find it ſet down in his 20th, 21ſt, 22d, 23d, and 24th chapters, to which (eſpecially the 23d) he very often in his book refers the reader for ſatisfaction, pretending there to evince, that rarefaction cannot be made out any otherwiſe than by ſuppoſing a body to be in 2, 3, 4, 10, 100, 1000, 1000000 of places at the ſame inſtant, and adequately to fill all and every one of thoſe places.

FIRST therefore, we will examine his negative, and next his affirmative, arguments for this ſtrange hypotheſis.

HIS negative I find in the 20th chapter, where he endeavours to confute the two ways of explicating the rarefaction and ſpring of the air, namely, that of the Vacuiſts and that of the Pleniſts.

CONCERNING the firſt of theſe, we find him conclude it impoſſible, firſt, becauſe he had before proved, that there can be no vacuum, which being done by a circle (*viz.* there is no vacuum in the tube, becauſe nature abhors a vacuum, and we ſee nature abhors a vacuum, becauſe ſhe will not ſuffer a vacuum in the tube above the mercury, but, to prevent it, will continually ſpin the quickſilver into ſuperficies, and never diminiſh the body of it) will ſuffer me to paſs to his next; which is, that this way is falſe, becauſe in the experiment of the carp's bladder the air is rarefied 1000 times bigger; nay, in reſpect of the body of gold, it has 1000000 times leſs matter in equal ſpaces. And this, ſays he, is a phænomenon, that is impoſſible ever to be made out by interſperſed vacuities. Now that the Vacuiſts cannot preſently, by ſo bold an aſſertion as this, be made to forſake their principles, he may perceive by theſe following ſolutions, which I ſhall give of all the phænomena he recites, flowing naturally from an hypotheſis, that I ſhall for the preſent aſſume. Let us ſuppoſe then the particles of bodies, at leaſt thoſe of the air, to be of the form of a piece of ribbon, that is, to be very long, ſlender, thin and flexible *laminæ*, coyled or wound up together as a cable, piece of ribbon, ſpring of a watch, hoop, or the like, are: we will ſuppoſe theſe to have all of them the ſame length, but ſome to have a ſtronger, others a weaker ſpring. We will further ſuppoſe each of theſe ſo coyled up to have ſuch an innate circular motion, as that thereby they may deſcribe a ſphere equal in diameter to their own, much after the manner that a meridian turned about the poles of a globe will deſcribe by its revolution a ſphere of the ſame diameter with its own in the air. By this circular motion the parts of the *laminæ* endeavouring to recede from the center or axis of their motion, acquire a ſpringineſs outward like that of a watch-ſpring, and would naturally fly abroad until they were ſtretched out at length; but that being incompaſſed with the like on every ſide, they cannot do it without the removal of them, as not having room ſufficient for ſuch a motion. And the faſter this circular motion is, the more do the parts endeavour to recede from the axis, and conſequently the ſtronger is their ſpring or endeavour outward. Theſe ſpringy bodies thus ſhaped and thus moved are ſufficient to produce all the phænomena he names as impoſſible to be explicated. And, firſt, for the buſineſs of expanſion, it will very naturally be explained by it. As let us ſuppoſe, for inſtance, the diameter of theſe ſmall coyled particles of the air, which being next the earth, are preſſed upon by all theſe numerous incumbent particles, that make up the atmoſphere, and are thereby ſo crouded, that they can but very little untwiſt themſelves; let us ſuppoſe, I ſay, the diameter of theſe particles to be $\frac{1}{1000000000000}$ of an inch; and then to be much of the form of thoſe repreſented in the 4th figure by ABCD: and that theſe particles, when a conſiderable quantity of the preſſure of the ambient air is taken away, will fly abroad into a coyle or zone ten times as big in diameter as before; that is, they will now be $\frac{10}{1000000000000}$ of an inch in diameter, and appear in the form of thoſe in the figure expreſſed by EFGH. Theſe zones, whirled round as the former, will deſcribe a ſphere 1000 times as big in bulk, and thereby fence that ſpace from being entered by any of the like zones. This it would do, ſuppoſing thoſe ſpheres did immediately always touch each other: but becauſe of their circular motion, whenever they meet, they muſt neceſſarily be beaten, and fly off from one another, and ſo require a yet greater ſpace to perform their motion in. This ſuppoſed, there are no phænomena of rarefaction (which is enough at preſent to anſwer what he objects) but may be naturally and intelligibly made out. As firſt, for that of the ſwelling of a carp's bladder, if we ſuppoſe ſome ſmall parcels of the former compreſſed *laminæ* to lie latitant within the folds of it, and being much coyled up together ſcarce to take any ſenſible room, this bladder in the air will appear to contain very little or nothing within it; whereas when the preſſure of the air is taken off in good part from the outſides of it, then thoſe formerly latitant particles diſcloſe themſelves by flying open into much bigger zones, ſo as perhaps to be able to defend a thouſand times bigger ſpace from being entered into by their like or any other groſs particles, ſuch as thoſe of the bladder. Now becauſe the pores of a bladder are ſuch as are not eaſily permeable by the particles of air, therefore theſe lurking particles ſo expanding themſelves muſt neceſſarily plump out the ſides of the bladder, and ſo keep them turgid, until the preſſure of the air, that at firſt coyled them, be re-admitted to do the ſame thing for them again.

NEXT, as for rarefaction by heat, that will as naturally follow as the former from this hypotheſis. For the atoms of fire flowing in, in great numbers, and paſſing through with a very rapid motion, muſt needs accelerate the motion of theſe particles; from which acceleration their ſpring, or endeavour outward, will be augmented; that is, thoſe zones will have a ſtrong nitency to fly wider open (for we know, that the ſwifter any body is moved circularly, the more do the parts of it endeavour to recede from the center of that motion) from whence, if it has room, will follow a rarefaction. As for the conveyance of light, that being, according to

Epicurus, performed by the local motion of peculiar atoms, their motions to and fro through this medium will be leſs impeded by the rarefied air than by the condenſed; as indeed upon experiment we ſhall really find them.

As for his third objection, drawn from his ſuppoſed attractive virtue of the thus rarefied air, that is quickly anſwered, by denying it to have any power at all of attraction; and by ſhewing (which is already done) that what effects he would have to be performed by the attraction of the included, are really done by the preſſure of the ambient air.

AND, laſtly, the phænomena of my Lord *Bacon*'s experiment are ſufficiently obvious, and eaſy to be deduced.

So then, by granting *Epicurus* his principles, that the atoms or particles of bodies have an innate motion; and granting our ſuppoſition of the determinate motion and figure of the aërial particles, all the phænomena of rarefaction and condenſation, of light, ſound, heat, &c. will naturally and neceſſarily follow: and the author's objections againſt this firſt way of rarefaction will ſignify very little.

As to the ſecond way of rarefaction by the intruſion or intervention of ſome ſubtile matter or æther, into the ſpaces deſerted by the rarefying particles, which is that propoſed by the aſſertors of a *Plenum*, this alſo is by the author condemned, and branded with impoſſibility. And why? firſt, becauſe 'tis (he ſays) impoſſible, that the above-mentioned phænomena of the carp's bladder can be explained by it. Secondly, becauſe it is impoſſible to give a reaſon from it of the impetuous aſcent of water admitted into an exhauſted receiver. And, thirdly, becauſe it is impoſſible to explicate the phænomena of gun-powder. His reaſons to confirm which three impoſſibilities, becauſe drawn from a mere miſtake, or ignorance of thoſe hypotheſes which have been invented by the aſſertors of that opinion, I ſhall paſs over, and content myſelf to explain a way how theſe impoſſibilities may become poſſibilities, if not probabilities.

AND the way, that I ſhall take, ſhall be that of the moſt acute modern philoſopher Monſieur *Des Cartes*, publiſhed in his Philoſophical Works: which is this, That the air is a body conſiſting of long, ſlender, flexible particles, agitated or whirled round by the rapid motion of the *globuli cæleſtes*, and the ſubtile matter of his firſt element, whereby they are each of them enabled to drive or force out of their vortice all ſuch other agitated particles. Now the ſwifter theſe bodies are whirled round, the more do their flexible parts fly aſunder and ſtretch themſelves out, and the more forcibly do they reſiſt the ingreſs of any other ſo agitated particles into their vortice; and conſequently the ſlower their motion is, the leſs will be their reſiſtance. And becauſe there is a vaſt number of theſe whirled particles lying one above another, and each particle having its peculiar gravity; it will neceſſarily follow, that the undermoſt (which to maintain their vortice muſt reſiſt ſo great a preſſure) muſt very much be hindered from expanding themſelves ſo far as otherwiſe they would, were there none of thoſe incompaſſing agitated particles, that lay in their way: and that thoſe being by any means removed, or they themſelves by a more rapid motion of the particles of their vehicles, the firſt and ſecond element (which is according to that hypotheſis an effect of heat) more ſwiftly and ſtrongly whirled round, they preſently begin to expand themſelves, and maintain a bigger vortice than before. Now to perform what I juſt now promiſed, I ſhall endeavour to give a poſſible, if not a probable, cauſe of the objected phænomena. And, firſt, for that of the carp's bladder, where the air is rarefied (ſays the author) 1000 times, it will eaſily be explained by ſuppoſing the few particles of the air, which (whilſt they ſuſtain the preſſure of all the incumbent atmoſphere) inconſpicuouſly lurk within the bladder, (each of them being able to maintain but a very ſmall vortice) to be by the ſubſiding mercury in the Torricellian experiment freed from the preſſure of the air, and their motion continuing the ſame (by reaſon that the tranſcurſion of their vehicles is not at all, or very little, hindered, either by the glaſs or bladder) their parts having room to expand themſelves, will fly abroad to ſuch extenſions, as may perhaps make a vortice 1000 times as big in bulk as what they were not able juſt before to exceed. Hence the particles of the air (being ſo groſs as not eaſily to pervade the pores of the bladder) muſt neceſſarily drive out the ſides of the bladder to its utmoſt extent, and ſerve to fill the receiver in the Magdeburgick experiment. Now, whereas theſe particles will by the ſame preſſure of the air be reduced to the ſame ſtate they were in at firſt, that is, to be thronged into a very little room, and thereby be able to maintain a very ſmall vortice; the air let in, in the Torricellian experiment, reduces the air in the bladder to its former inconſpicuouſneſs, as the admiſſion of the water in the *Magdeburg* experiment does that receiver full of rarefied air into the bigneſs of a hazel-nut. Now the water in this laſt-mentioned experiment enters with a great impetuoſity, becauſe driven on with the whole preſſure of the atmoſphere, and reſiſted only by the ſmall force of the ſo far rarefied air.

As for the author's objection againſt this way of rarefaction drawn from the phænomena of gun-powder, I ſhall endeavour to anſwer it by ſhewing them poſſibly explicable by a Carteſian hypotheſis. For ſuppoſing thoſe terreſtrial parts of the gun-powder to be firſt at reſt, and afterwards agitated by the rapid motion of its firſt element, there will be ſufficient difference of the former and latter condition in reſpect of extenſion; and ſuppoſing the particular conſtitution of gun-powder (ariſing partly from the ſpecific forms of the particles of its ingredients, nitre, ſulphur and charcoal, and partly from their proportionate commiſſion) to be ſuch as will readily yield to the motion of his *materia ſubtilis*, ſo ſoon as an ingreſs is admitted to it by the firing of any particular parcel of it, the expanſion will be ſpeedy enough.

So then let us ſuppoſe a barrel of gun-powder placed in ſome cloſe room, to ſome grains of which we will ſuppoſe ſome actual fire to be applied, by which actual fire (the texture of the powder being ſuch) thoſe grains are ſuddenly fired; that is, many millions of parts, which before lay ſtill and at reſt, are by the action of the burning-coals ſhattered, as it were, and put into a poſture ready to be agitated by the rapid motion of the *materia ſubtilis:* into which poſture they are no ſooner put, than agitated and whirled ſufficiently by it; whence follows a vaſt expanſion of that part of gun-powder ſo fired. For each of its parts being thus whirled and hurried round, expel and beat off with great violence all the contiguous particles, ſo as that each particle takes up now 1000 times as much elbow-room (if I may ſo ſpeak) as juſt before ſerved its turn; and conſequently thoſe that are outermoſt, take every one their way directly from the parcel or corn they had lain quiet in, being hurried away by the ſudden expanſion of the particles that lay next within them: ſo that whatever grain or parcel of gun-powder they chance to meet with, before they have loſt their motion, they preſently ſhiver, and put into ſuch a motion, as makes them fit to receive the action of the *materia ſubtilis*. Which ſubtile matter being every where preſent, and nothing ſlow in performing its office, immediately agitates thoſe alſo like the former; ſo that in a trice the particles of the whole barrel of gun-powder are thus diſordered, and by the motion the *materia ſubtilis* muſt needs be hurried away with ſo great an impetuoſity on all ſides, as not only to break in pieces its ſlight wooden priſon, and remove the lighter particles of the ambient air, but huge beams, nay, vaſt accumulated maſſes of the moſt compacted ſtructures of ſtone, and even ſhake the very earth itſelf, or whatever elſe ſtands in its way; whoſe texture is ſo cloſe, as not to give its particles free paſſage through its pores. This underſtood, I ſee not, firſt, what the author's three arguments, brought to prove his objection, ſignify; for there are no more corpuſcles in the room before the gun-powder is fired than after, nor is there any more matter or ſubſtance, before the ſides of the room, by yielding, give place for the external fluid bodies to ſucceed, and the only change is this, that the *globuli ſecundi elementi* (as he calls them) are expelled out of the room, and the *materia primi elementi* ſucceeds in the place of it. Nor do I ſee, ſecondly, what great reaſon he had for his grand concluſion. *Hæc abundè demonſtrant, rarefactionem per hujuſmodi corpuſcula nullatenus poſſe explicari.*

Having thus examined the author's firſt arguments, that rarefaction cannot be made out by any other way than his; we ſhall find his other, which he brings to eſtabliſh his own hypotheſis, much of the ſame kind. As, firſt, that his way of rarefaction implies no contradiction: for if the affirming a body to be really and totally in this place, and at the ſame time to be really and wholly in another, that is, to be in this place, and not to be in this place, be not a contradiction, I know not what is. Next, that ſome learned ſchoolmen have thought ſo; to which I anſwer, more learned men have thought otherwiſe. And, laſtly, that there are very plain examples of the like nature to be found in other things; of which he only brings one, *viz.* that of the *Rota Ariſtotelica*, which upon examination we ſhall find to make as little to the purpoſe as any of the other.

An Explication of the Rota Ariſtotelica.

THE great problem of the *Rota Ariſtotelica*, by his explication of which he pretends, not only to ſolve all the difficulties concerning local motion, *quæ philoſophorum ingenia hactenus valde exercuerunt*, but to give an inſtance for the confirmation of his unintelligible hypotheſis of rarefaction, wherein there is *extenſio ſeu correſpondentia ejuſdem rei ad locum nunc majorem, nunc minorem*; We may upon examination find to be either a paralogiſm, or elſe nothing but what thoſe philoſophers ſaid, whom he accounts gravelled with it. Of this ſubject he begins in his 25th chapter, where, after he has ſet down a deſcription of it, he makes an inſtance in a cart-wheel; *Rem ante oculos ponit rota alicujus currus, ejuſque umbo ſeu lignum illud craſſum & rotundum, cui infiguntur radii; ſiquidem dum progrediente curru ipſa rota circumducta deſcribit in ſubjecta terra orbitam ſibi æqualem, umbo ille deſcribit in ſubjecto aëre orbitam* (I ſuppoſe both here and before he means *lineam*) *ſe multo longiorem, utpote æqualem orbitæ totius rotæ, licet ipſe non niſi ſemel quoque fuerit circumvolutus.* (As for what he ſays, that the nave muſt be ſuppoſed to paſs through the air, and not to touch the ſolid plain, I do not yet underſtand the force of his reaſon, nor why he ſets it down, making nothing to his preſent purpoſe, unleſs it were becauſe he did not well underſtand the thing.) In which, ſays he, the great difficulty is to explain how the nave ſhould be ſo turned about its axis, *ut partes ſuas ſucceſſivè applicet lineæ duplo plures partes habenti, idque motu perpetuo ac uniformi nè vel ad oculum inſtar interrupto.* Which how true, and what great occaſion he had to wonder at the ſolution of that problem by the example of a man ſtanding ſtill and another walking, we ſhall find by and by, when we come to explain the problem: but firſt I ſhall examine his hypotheſis and explication. And firſt, he ſuppoſes time to conſiſt of a determinate number of indiviſibles (that is, ſuch as have neither *prius* nor *poſterius* included in them) which he calls inſtants. And next he ſuppoſes the *præſentiam localem ſeu ubicationem cujuſlibet partis indiviſibilis & virtualiter extenſæ eſſe quoque indiviſibilem & virtualiter extenſam.* Which ſuppoſition, ſo ſtrangely expreſſed, is no more than this; that the extenſion or ſpace of his indiviſibles is alſo indiviſible.

But as for his virtual extenſion, I confeſs I underſtand as little what it is, as I verily believe he did: and therefore I will proceed to his following ſuppoſition. His third therefore is, that by how much more rare a body is, by ſo much the more are its indiviſibles virtually extended. Hence his fourth is, that though theſe indiviſibles be really indiviſible, yet they are virtually *in quotvis partes diviſibiles.* Whence he deduces his fifth principle, that ſince theſe indiviſibles are really indiviſible and virtually extended, they muſt neceſſarily be moved after the ſame manner that other indiviſible and virtually-extended things are. His inſtances are in the motions of an angel and an indiviſible piece of wood, which, he ſays, are both of the ſame kind. As for that of angels, having no immediate revelation, and a ſpirit and its actions not falling under ſenſe, and not having any third way by which to be informed. I ſhall leave him there to enjoy his fancies. But as for that of his piece of wood, we ſhall find it ſufficiently full of abſurdities and contradictions. And firſt, he calls it indiviſible, but why I know not, for it is neither really nor yet mentally ſo: not mentally ſo, by his fourth principle, where he ſays that it is *virtualiter in quotvis partes diviſibiles*, by which word *virtualiter* he means the ſame thing with *mentaliter*, or nothing. Nor, ſecondly, is it really ſo: for then (according to the main buſineſs of his book, as may be gathered from the firſt words of his title-page, *Tractatus de corporum inſeparabilitate)* it would be impoſſible, that any thing in the world ſhould be diviſible; for he making an ſeparable continuity, and that bodies will rather be (I cannot tell how) ſtretched beyond their own dimenſion *in infinitum*, than part from one another; a body may as ſoon paſs through the dimenſions of any one indiviſible, as paſs between two. Next, he grants in the ſtrange ſtretching or rarefaction of theſe indiviſibles a temporary motion of the condenſed dimenſion; whence there will follow, that there muſt be diſtinct places or *ubi's*, it muſt be *terminus à quo*, *terminus ad quem*, *& medium.* And next, it were impoſſible to divide a line into two parts, ſuppoſing it conſiſted of an unequal number of indiviſibles; as if 101 indiviſibles of exceedingly-rarefied air ſhould be extended in length an inch, it were impoſſible to divide that inch into two equal parts. I might run over many more, but it would be too tedious to be here recited. As for his indiviſible parts of time, thoſe alſo muſt neceſſarily be *in quotvis partes diviſibiles*; for elſe the ſame body or indiviſible muſt neceſſarily be in divers places at the ſame inſtant. But becauſe he can ſwallow, nay confidently affirm, this and many other ſuch like contradictions and abſurdities, I am not willing to mention them; and I think it would have made more for the author's reputation, if he had done ſo too. As for his laſt chapter, where he applies theſe principles to the explication of the *Rota Ariſtotelica*, I have not here time to ſet down all the abſurdities, that any one that has but a ſmattering in the mathematicks may obſerve: as, ſometimes half an indiviſible part of a circumference may touch an indiviſible of a line, ſometimes one may touch half, a quarter, a hundredth part, a whole one, two, ten, a hundred, *&c.* at the ſame inſtant; nay, an indiviſible of a circle may be all of it in a thouſand places together, and the like. And this he brings as a great argument to eſtabliſh his hypotheſis of rarefaction, pretending it to compriſe many ænigmas and very great difficulties; whereas the thing is very plain and eaſy, and contains no ſuch obſcurities. For if, for example, we ſuppoſe a wheel A B C D to be moved in a direct motion from A I C to K L M, every point of it retaining the ſame poſition to that line, that they had at the beginning of their motion, each of the points A E I G C will on a plain, or in the medium it pervades, paſs through or deſcribe a line equal to the line I L, and not only all the points lying in the line A I C, but all and every point of the whole ærea of the circle: this muſt neceſſarily happen, if the diameter A I C be moved parallel to itſelf: but if whilſt it be thus moved with an equal progreſſion, it be likewiſe moved with an equal circulation, the caſe will be altered. For then, firſt, each point will by this compound motion deſcribe on the plain or medium either a perfect cycloid, as when the wheel makes one perfect revolution, whilſt the whole is progreſſively moved from I to L; or ſome piece, as when the wheel has not perfected its revolution; or more than a whole one, as when the circle has made more than one whole revolution, whilſt it is moved in its determinate length. I ſhall here only conſider the firſt, as pertaining more eſpecially to my preſent purpoſe, and in regard the two latter on occaſion may be eaſily explicated by it. Next, each point of this circle acquires from its compounded motion various degrees of celerity as to its progreſſion, according to its various poſition to a point, which is always found in ſome part of the line drawn through the center of the circular motion perpendicular to the progreſſive. And it is found thus, As the circumference to the radius, ſo is the line of the progreſſive motion to the diſtance of the point from the centre. And this happens, becauſe the line of progreſſion is equal to the circle deſcribed on that diſtance as radius; each point therefore of this ſmaller circle, when it comes to touch the perpendicular, muſt, as to its progreſſive motion, ſtand ſtill: this point therefore will be the centre of this compounded motion. Now becauſe for the explication of the *Rota Ariſtotelica* we need not conſider any other than thoſe points, which are tranſient through or croſs the perpendicular line, we ſhall only examine them. Let then in our example A be the centre or immoveable point; the circumference therefore A B C D will be equal to I L or A K by our hypotheſis. Now becauſe the point I, which is the centre of the rotation, has only one motion, *viz.* that of lation, its celerity will be equal to the ſingle celerity of the lation; we will therefore put it to have one degree C, becauſe it is moved with two motions, both tending the ſame way, and each equal to the velocity of I, muſt needs have two degrees of velocity. The point F, becauſe moved with two motions, both tending the ſame way, the one (*viz.* its lation) being equal to that of I, and the other (becauſe it is but half as far diſtant from the centre of rotation as C, and therefore is moved but with half the celerity of C, which was equal to that of I) but half as quick, we will put to have one degree and an half. By the like method we might find the velocity of all the points in the perpendicular, *viz.* ſuch as we have there marked ſome of them; but it would be too tedious, we needing not to conſider more than the two points A and E. The point at E being moved forward by its progreſſion with the ſame velocity that I, but by its rotation (which is but half as ſwift as that of the circle ABCD that is double the circle EFGH) being moved the contrary way or backwards with half the velocity, loſeth half of its progreſſion forwards. The point in A being by its progreſſion moved forwards equally ſwift with I, and by its rotation (the circle ABCD being equal to the line IL) being carried backwards with equal velocity, muſt neceſſarily ſtand ſtill as to its progreſſion. Now having ſhewn, that the point A (being by reaſon of its two equal oppoſite motions at reſt) does only touch a point of the line AK, and is not at all moved on it; and that the point E (being carried forward twice as faſt by its progreſſion as it is carried backward by its rotation, and thereby moved half as faſt as the point I) does not only touch the line E K, but whilſt it touches it, is moved on it with a progreſſive motion half as ſwift as that of I: it will neceſſarily follow, that each point ſituate in E muſt neceſſarily deſcribe a ſmall line, which is a part of the whole E C. Now both the contact of the former, and the contact and progreſſion of the latter, being performed by an infinite ſucceſſion of points in the ſpace of an infinite ſucceſſion of inſtants; I ſee not any one difficulty of this problem, but may ſatisfactorily be given an account of by it, without having recourſe to the hypotheſis of the determinate number of indiviſibles of ſpace and time, which at beſt will only come to this, that *in ſuch a determinate moment or minute ſpace of time* (which conſiſts of an infinite conſecution of inſtants, and has *prius* and *poſterius* in it; though yet he will call it an inſtant, and have it to have the ſame proprieties with an inſtant uſed in the common philoſophical ſenſe) *ſuch a determinate minute corpuſcle* (which though it have extenſion in length, breadth and thickneſs, yet will he not admit it to be diviſible or have parts, no not though, according to his hypotheſis, the indiviſible of one body may be rarefied to be as big in bulk as a million of the indiviſibles of another, but will have it to be called and to be a real indiviſible) *will ſucceſſively paſs over ſuch a determinate ſpace or length* (which yet he will not admit to be diviſible, though according to his principles it may equalize the length of millions of his other indiviſibles, nor admit a ſucceſſive motion, but inſtantaneous, though that does neceſſarily put a a body into two, three, ten, a hundred, *&c.* places at once; but will have theſe alſo to be indiviſible.) Haſte makes me paſs over the abſurdities about the contact of a circle and a line, and to compriſe in ſhort all that great explication he has given of this and other intricate (as he calls them) problems; which is this, that the reaſon of the celerity of the motion of ſome one of theſe indiviſibles above another is, that it paſſes through a greater part of an indiviſible in the ſame inſtant than the ſlower; that is, in plain ſenſe, no more than this, One body is ſwifter than another becauſe it is moved faſter. From whence he draws ſeveral corollaries, as that, Hence may be given a reaſon, why an eagle is ſwifter than a tortoiſe, *viz.* becauſe it moves faſter. I ſhould have ſolved ſeveral objections, which may be brought againſt the diviſibility of quantity *in infinitum*; but that as all the ſcholaſtic writers are full of them, ſo it is a ſubject, which we are leaſt able to diſpute of, having very little information of the nature of infinity from the ſenſes.

MVSEVM
BRITAN
NICVM

AN

EXAMEN

OF

Mr. *T. HOBBES* his

Dialogus Phyſicus de Natura Aëris;

As far as it concerns Mr. BOYLE's Book of New Experiments
touching the SPRING of the AIR, *&c.*

With an APPENDIX touching Mr. HOBBES's Doctrine of Fluidity
and Firmneſs.

The PREFACE.

HAVING ſoon after I had begun the following examen of Mr. *Hobbes*'s dialogue, been diverted for a good while by divers urgent avocations from purſuing it; I was in the mean time informed by learned men (ſome of whom keep great correſpondences with the virtuoſi abroad) that my publiſhing any thing againſt his objections would not be neceſſary, nor was much expected. Whereupon I ſhould perhaps have declined reſuming an employment, that to a perſon of my humour could not be delightful; but that beſides thoſe inducements mentioned at the beginning of the following treatiſe, it came into my mind, that my adverſary, not content to fall upon the explications of my experiments, has (by an attempt, for aught I know, unexampled) endeavoured to diſparage unobvious experiments themſelves, and to diſcourage others from making them. Which if he could by his dialogue effect, I dare be bold to ſay, he would far more prejudice philoſophy by this one tract, than he (and that it may not ſeem ſaid to undervalue him, I ſhall add, or any man elſe) can promote it by all his other writings. Wherefore, though his diſparaging of experiments would probably have much more authority (eſpecially with conſidering men) if he had been the author of conſiderable ones, or did appear to be more than ordinarily ſkilled in them: yet leſt, for all this, his fame and confident way of writing might prejudice experimental philoſophy in the minds of thoſe, who are yet ſtrangers to it, I thought it might not be amiſs, both to go on with the diſcourſe I had begun, and to enlarge it beyond what I firſt deſigned; and accordingly, to the intended vindication of the main points of our doctrine, *The weight and ſpring of the air,*

air, which (if I miſtake not) we have firmly eſtabliſhed, we have added an Examen, that otherwiſe we ſhould ſcarce have made, of the greateſt part of the phyſiological paſſages in Mr. *Hobbes*'s book, moſt of which I thought might be rationally queſtioned, and many of them clearly diſproved. And in purſuance of this, though I did not perhaps always think myſelf obliged to proſecute things further than the nature of my deſign required, or to forget, that the matters in diſpute were not all of an equal weight; yet the reflections I have employed will, I preſume, be found ſufficient to ſhew, both that it is eaſy even for a great wit frequently enough to miſtake, and much more frequently to miſs of clearly demonſtrating what he pretends in matters phyſical, for want of having ſufficiently conſidered the experiments he would be thought to deſpiſe; and that Mr. *Hobbes*'s adverſaries need not be much aſhamed of the name he is pleaſed to give them, of experimentarian philoſophers. It was alſo ſuggeſted to me, that the dangerous opinions about ſome important, if not fundamental, articles of religion, I had met with in his *Leviathan*, and ſome other of his writings, having made but too great impreſſions upon divers perſons (who, though ſaid to be for the moſt part either of greater quality, or of greater wit than learning, do yet divers of them deſerve better principles) theſe errors being chiefly recommended by the opinion they had of Mr. *Hobbes*'s demonſtrative way of philoſophy; it might poſſibly prove ſome ſervice to higher truths than thoſe in controverſy between him and me, to ſhew, that in the Phyſics themſelves, his opinions, and even his ratiocinations, have no ſuch great advantage over thoſe, of ſome orthodox Chriſtian Naturaliſts. But for all this, as little as I could grudge to write a much longer than the following diſcourſe to do religion the leaſt ſervice; yet thinking it fit to leave controverſies of this kind to thoſe, whom they more particularly concern, I ſhould ſcarce, in the introduction to a diſpute about the air, have at all mentioned any thing of this nature, but that Mr. *Hobbes*, in the preface to his dialogue, is pleaſed (though I know not to what purpoſe in that place) to ſpeak without limitation or diſtinction (and conſequently unwarily enough at leaſt) of the things ſaid in the books of naturaliſts concerning immaterial ſubſtances (and ſure ſome things true, at leaſt that there are ſuch beings, as well as ſome things erroneous, are there ſaid) though he hath been, by the learned Dr. *More*, and others, publickly accuſed to have taught, that it is abſurd to believe, that there either are or can be any. Which yet, methinks, he could not do, ſince elſewhere, and in this very dialogue, he builds ſeveral things in his[*] philoſophy upon the creation of the world, and an infinite power: and how a thing material can create matter, and have an infinite power, I confeſs I do not underſtand.

I DOUBT not but critical readers will think I might have excepted againſt many more particulars in Mr. *Hobbes*'s book than I have examined; and indeed, about this, I dare not contend with them. For beſides that I may, through haſte and indiſpoſedneſs to quarrel, have overſeen ſeveral things, which an eye either ſeverer or more attentive would have obſerved; I purpoſely paſt by divers things I did not altogether overlook; partly, becauſe I thought it needleſs to queſtion them (having no want of other objections) and partly, becauſe I could not do ſo in few words, and was loth to engage in needleſs and long-winded diſputes: and, perhaps, I was too weary of my employment, to be willing to ſpend many words, when I could ſafely ſpare them. And though others will poſſibly think it ſtrange, that a member of the ſociety he is ſo ſevere to, ſhould not take notice of ſuch paſſages as theſe; *Nam conveniant* (ſays he, ſpeaking of the virtuoſi, that meet at *Greſham* college) *ſtudia conferant, experimenta faciant quantum volunt, niſi & principiis utantur meis, nihil proficient.* And again, *Næ illi quæ dicerent non videntur cogitaſſe, ſed ſortitos eſſe.* And elſewhere, *Conjicere hinc licet, quam ſint boni ratiocinatores, & quæ ſit ab illis expectanda philoſophià naturalis.* And (to trouble you no more) *Ad cauſas autem, propter quas proficere ne paululum quidem potuiſtis nec poteritis, accedunt etiam alia, ut odium Hobbii, quia nimium liberè ſcripſerat de academiis veritatem: nam ex eo tempore irati phyſici & mathematici veritatem ab eo venientem non recepturos ſe palam profeſſi ſunt*; though, as I ſaid, ſome may wonder I ſhould ſilently pretermit ſuch paſſages as theſe; yet beſides what I elſewhere ſay by way of account of my ſo doing, I ſhall here tell them, that I preſume ſome ſorts of readers will more eaſily pardon me for neglecting ſuch expreſſions, than they will Mr. *Hobbes* for uſing them. And I confeſs, I thought I ſhould find it more eaſy to ſay nothing at all to ſuch paſſages, than ſay any thing without ſaying ſomewhat, that would offend a perſon, that could allow himſelf to ſay ſuch things. Though I ignore not, that divers readers will much the leſs reliſh the following diſcourſe, for my having, perchance not altogether for want of knowing how to write otherwiſe, forborn to furniſh it with quick and ſmart expreſſions, which are wont to be employed in diſputes, to expoſe or depreciate an adverſary's perſon or cauſe, and which are uſually not the leaſt things, that ſerve to amuſe ſuch readers, and engage their attention. But I fear I have much leſs need to make excuſes for my omiſſions, than for having, in the following examen, been reduced by the nature of my taſk, to ſay ſo many things which intelligent readers need not be taught by me. And therefore ſuch ſhall have my conſent to ſkip, if they pleaſe, the whole diſcourſe: which though I could ſcarce upon ſuch an occaſion make a very inſtructive one; yet if they will be pleaſed to forgive me its barrenneſs, I hope hereafter to avoid the like temptations of writing again at the like rate.

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[*] — *Neque hominis philoſophi eſſe cenſeo corporum quorundam, ut ſolis & ſtellarum, mirabiles ſupponere magnitudines, contra vero mirabiles exiguitates non admittere, cum virtutis ejuſdem infinitæ ſit utraque creare tam maxima quam minima.* Mr. *Hobbes* in his Dialogue of the Air, page 11. See alſo page 5. and elſewhere.

And having ſaid thus much as to the reaſons of my penning the following diſcourſe, I muſt add ſomething, though but little, touching the manner of it; wherein I hope I have not much, if at all, ſwerved from what I propoſed to myſelf; namely, to give an example of diſputing in print againſt a provoking, though unprovoked, adverſary, without bitterneſs and incivility, and without purſuing thoſe things, which how much more ſoever they belong to the perſon of an antagoniſt than to his cauſe, are wont to make up a great part (if not the greateſt) of books divulged on ſuch occaſions. But ſince I intend what I write for intelligent and ingenious readers, I dare expect, that my forbearing to inſiſt on ſuch things, as I judged wholly extrinſical to the opinions and arguments I examine, will be aſcribed to the true cauſe, that my diſcourſe will not be thought to have the leſs of reaſon for having the leſs of paſſion; and (eſpecially) that my ſilence as to thoſe things, that are ſpoken to the diſparagement of the illuſtrious company, that meets at *Greſham* College, will be looked upon only as an effect of my judging it fit to leave them the full liberty to right themſelves, if they think it worth while, by ſome better pen than mine. And if Mr. *Hobbes* thinks fit to ſay any thing to the following diſcourſe, it will not be amiſs, that his reply be as inoffenſive as I have endeavoured to make my examen. For having diſpatched as much as I think requiſite to ſay of this controverſy myſelf, and having other (and I hope better) employments for my leiſure hours, if I can get any; I muſt leave the further diſputes, if any ſhall ariſe, to be managed by others, who, if Mr. *Hobbes* refuſe to imitate my way of writing, will poſſibly make no ſcruple to imitate his, and put him in mind of that law of *Veſpaſian*, upon which himſelf would be thought to ground that heap of ſtrange titles he beſtows upon the two learned *Savilian* Profeſſors [*], *That it is unlawful to give ill language firſt, but civil and lawful to return it.* I have but one thing more to add; which is, that I would not be ſo far miſtaken, as to be, upon the account of what I have written againſt my two adverſaries, looked upon as a perſon wedded to his opinions: for not having hitherto learned, that either of their books has yet made proſelytes; I preſume it will not be wondered at, that they have not made me one. And though the two learned authors I have anſwered, have given me no cauſe to retract any of my opinions; yet as it is not improbable, that others reaſoning upon better principles may do what theſe have not done, ſo I am ſtill of the ſame temper I was of, when I uſed to propoſe my thoughts but as conjectures.

AN
EXAMEN
Of the greateſt part of Mr. *HOBBES*'s
Dialogus Phyſicus de Natura Aëris.

CHAP. I.

The occaſion and ſcope of the preſent Treatiſe.

MEETING the other day with a treatiſe then newly publiſhed by Mr. *Hobbes*, and intitled *Dialogus phyſicus de naturâ aëris:* the name of the author, the ſubject of the book, and the information I had a good while before received from his friends, that he was writing againſt me, invited me to peruſe it, as a diſcourſe, wherein I might probably find myſelf concerned: nor was I deceived in my expectation. For having curſorily paſſed through it, I readily found, that though I be not expreſsly named there, and though ſome things in the title-page, and ſome others in the book itſelf, ſeem to make the chief deſign of it to be the diſparagement of the ſociety, that is wont to meet at *Greſham* College; yet the arguments are for the moſt part levelled at ſome writings of mine, publiſhed, ſome of them, the year before, and ſome of them this laſt ſpring; as the Experiments, whoſe explications he is pleaſed to cenſure, do all along declare. I confeſs I was ſomewhat ſurprized to find, that Mr. *Hobbes*, whom if my books have at all mentioned, it has been with reſpect, ſhould fall upon a perſon, that had not provoked him; whilſt ſuch mathematicians as Dr. *Wallis*, Dr. *Ward*, *Tacquet* and *Moranus* (men much too famous to be deſpicable adverſaries) having a good while ſince profeſſedly and unchallenged written againſt him, he hath yet the whole diſcourſes of ſome, and ſo great a part of the objections of the others, to reply to. And it ſomewhat added to my wonder, that a writer of politics ſhould cauſeleſsly and needleſsly, for aught I can learn, fall upon a ſociety, whereof, beſides many other perſons of quality and men of parts, his own great patron, and my highly honoured and learned friend, the Earl of *Devonſhire* himſelf, is an illuſtrious member. And as for me, I ſhall not ſcruple to confeſs, that I could have been well enough contented Mr. *Hobbes* had ſpared this dialogue, partly becauſe I have a natural indiſpoſedneſs to contention, partly becauſe I am at preſent diſtracted by ſtore of other employments both of a public and a private nature (and particularly by the publiſhing of three or four books of differing ſubjects, and printed in ſeveral places;) partly becauſe Mr. *Hobbes*'s objections are of ſuch a nature, that perhaps my replies, though as ſhort as (my deſign mentioned in the preface conſidered) I can conveniently make them, will amount to a longer diſcourſe, than moſt readers will think the objections needed; and partly too, becauſe Mr. *Hobbes* is pleaſed to write of divers worthy and learned men in ſo depreciating, and of himſelf in ſo differing a way, that I fear I ſhall find it ſomewhat uneaſy to retain (under ſuch provocations to decline it) the civility I am wont, and am deſirous to write with; and that I muſt almoſt deſpair of diſſenting without an abſolute rupture from a perſon, whoſe way of diſcourſing is ſuch, that though I ſhall not give it any epithet, yet I confeſs it leaves me but little hope, that I can oppoſe him without angering him.

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[*] So go your ways (ſpeaking to Dr. *J. Wallis* and Dr. *S. Ward*), you uncivil eccleſiaſtics, inhumane divines, dedoctors of morality, unaſinous collegues, egregious pair of *Iſſachars*, moſt wretched *Vindices* and *Indices Academiarum*; and remember *Veſpaſian*'s Law, &c. Mr. *Hobbes*'s Leſſon 6. p. 64.

BUT however, becauſe if I can (as I intend to do) ſo far comply with my inclinations and my cuſtom, as to wave perſonal and intrinſic matters, and reſtrain myſelf to the examen of the argumentative part of his diſcourſe, my reply will not need to be prolix; and becauſe he has vouchſafed rather to ſingle out a young writer, whoſe books (at leaſt of matters philoſophical) do but begin to appear in the world, than to defend himſelf againſt thoſe illuſtrious enemies, upon whom he might expect to gain much more honour; and becauſe Mr. *Hobbes*'s name may with ſome readers give his arguments an efficacy, which their own nature could not confer on them; I muſt reſolve to ſubmit to what he and my concern for the truths he rejects impoſe upon me. But to ſhorten as much as I can a work, to which I can allow but very little time, it will be expedient, before I deſcend to the examination of particulars, to premiſe three or four advertiſements touching the occaſion and the nature of the controverſy, that I might not be reduced to a frequent and unwelcome inculcation of the ſame things.

CHAP. II.

Of ſome miſtakes of Hobbes *touching matters of fact, and the author's doctrine.*

AND firſt, whereas Mr. *Hobbes* is pleaſed to write, as if the explications and experiments to be met with in the phyſico-mechanical treatiſe he cenſures, were thoſe of the whole ſociety at *Greſham* College; I muſt do them that right to declare, that this way of proceeding is manifeſtly grounded upon a miſtake, I will not affirm, that the miſtake was wilful, that Mr. *Hobbes* might give himſelf a pretence to quarrel with them (who have hitherto ſuſpended the declaring themſelves as a Society) in the controverted points. But there are ſome, that think Mr. *Hobbes* might very eaſily have avoided this miſtake, ſince the book he cenſures was publiſhed (and perhaps taken notice of by moſt of the virtuoſi here) ſome months before the ſociety was begun. And the experiments themſelves had been, long before the book came forth, not only ſeen and diſcourſed of by divers learned men and illuſtrious perſons, but had the honour to have our great monarch of the virtuoſi, as well as of *Great Britain*, for a ſpectator. And though poſſibly divers of the learned members of our aſſembly may have no unfavourable opinion of what I have delivered in that book; yet the aſſembly, as ſuch, has been ſo far from adopting or owning my opinions as theirs, that it has with approbation been propoſed among them, to repeat the experiments, and take a review of the explications, that upon a ſtrict examen of the ſeveral opinions, and the objections that could be brought in againſt them, they might ſee, what judgment will be fit to be paſſed on them. And although there be very few philoſophers, whoſe parts may make their judgment more formidable to me; yet to comply with their deſign, whatſoever the event might be, I preſented them the engine itſelf, I had made uſe of and deſcribed in my book; chuſing rather to undergo their cenſures, than want their inſtructions. By which it may appear, upon how little ground Mr. *Hobbes* has thought fit to impute to the ſociety thoſe opinions, which (how erroneous ſoever he is pleaſed to think them) I muſt own to be mine. And this juſtice I the rather do it, becauſe it is all, that I am to do in this treatiſe on their behalf, not only for the reaſons above intimated, but becauſe the vindication of ſuch an aſſembly againſt Mr. *Hobbes* deſerves a better pen than mine, though it doth not need it.

Secondly, Undertaking then the defence of my own cauſe, without intereſting them in my quarrel, I muſt next admoniſh the reader, that whereas Mr. *Hobbes* writes, as if the new experiments were deviſed, or at leaſt employed, to prove a vacuum; he is in this likewiſe miſtaken. For neither has the ſociety declared either for or againſt a vacuum, nor have I: nay, I have not only forborn to profeſs myſelf a Vacuiſt, or a Pleniſt, but I have in a fit place of my epiſtle expreſsly ſaid, that I reſerved the declaring of my own opinion touching that point to another diſcourſe (which as yet is not publiſhed). Wherefore Mr. *Hobbes* either injures or miſtakes thoſe, whom he will needs make his adverſaries, when he repreſents the new experiments as demonſtrations alleged by profeſſed Vacuiſts to diſprove the fulneſs of the world. And though I ſhall be obliged in the following diſcourſe to reject Mr. *Hobbes*'s ſuppoſition of a plenum; yet I intend not thereby to declare, whether or no I do abſolutely allow a vacuum. But that, which I drive at, and which alone my preſent work exacts, is to ſhew, that I may reaſonably oppoſe the hypotheſis of a plenum, as it is ſtated by Mr. *Hobbes*: and conſequently, unleſs he had better proved it, I may very well refuſe to let him take it for demonſtrated. But I intend not to queſtion, whether or no, other Pleniſts may not have better arguments than his principles have ſuggeſted to him; nor to deny but that the Carteſians may, without granting a vacuum, give a more plauſible account (whether true or no) of divers of the phænomena of our engine, if they will add, as ſome of them of late have done, the ſpring of the air to their hypotheſis, that the celeſtial matter, of which the air does in great part conſiſt, is ſuitable enough freely to paſs through the pores of the cloſeſt bodies, and even glaſs itſelf.

As for the aſſertion *non dari vacuum*, though, as I ſaid, I need not in this place declare myſelf either for or againſt it, yet I confeſs I do not find, that Mr. *Hobbes*, though all along this diſcourſe he argues from this principle againſt thoſe he thinks Vacuiſts, has demonſtrated it. For in his book *De corpore* (though a main part of it *Cap.* 26. depends upon the plenitude of the world) he has, that I remember, but one poſitive *Sect.* 2. argument (indeed he thinks that unanſwerable) to evince it. And that is drawn from this experiment: 'That if a gardener's watering-pot be filled with water, the hole 'at the top being ſtopped, the water will not flow out at any of the holes in the him, he hath yet the whole diſcourſes of ſome, and ſo great a part of the objections of the others, to reply to. And it ſomewhat added to my wonder, that a writer of politics ſhould cauſeleſsly and needleſsly, for aught I can learn, fall upon a ſociety, whereof, beſides many other perſons of quality and men of parts, his own great patron, and my highly honoured and learned friend, the Earl of *Devonſhire* himſelf, is an illuſtrious member. And as for me, I ſhall not ſcruple to confeſs, that I could have been well enough contented Mr. *Hobbes* had ſpared this dialogue, partly becauſe I have a natural indiſpoſedneſs to contention, partly becauſe I am at preſent diſtracted by ſtore of other employments both of a public and a private nature (and particularly by the publiſhing of three or four books of differing ſubjects, and printed in ſeveral places;) partly becauſe Mr. *Hobbes*'s objections are of ſuch a nature, that perhaps my replies, though as ſhort as (my deſign mentioned in the preface conſidered) I can conveniently make them, will amount to a longer diſcourſe, than moſt readers will think the objections needed; and partly too, becauſe Mr. *Hobbes* is pleaſed to write of divers worthy and learned men in ſo depreciating, and of himſelf in ſo differing a way, that I fear I ſhall find it ſomewhat uneaſy to retain (under ſuch provocations to decline it) the civility I am wont, and am deſirous to write with; and that I muſt almoſt deſpair of diſſenting without an abſolute rupture from a perſon, whoſe way of diſcourſing is ſuch, that though I ſhall not give it any epithet, yet I confeſs it leaves me but little hope, that I can oppoſe him without angering him.

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Sect. 2.

bottom; but if the finger be removed to let in the air above, it will run out at them all; and, as ſoon as the finger is applied to it again, the water will ſuddenly and totally be ſtayed again from running out. The cauſe whereof (ſubjoins he) ſeems to be no other but this, that the water cannot by its natural endeavour to deſcend, drive down the air below it, becauſe there is no place for it to go into; unleſs either by thruſting away the next contiguous air it proceed by continual endeavour to the hole at the top, where it may enter and ſucceed in the place of the water that floweth out; or elſe by reſiſting the endeavour of the water downwards, penetrate the ſame, and paſs up through it.'

But this experiment, as an obvious one, and without dreaming that Mr. *Hobbes* had laid ſuch ſtreſs upon it, I have incidentally anſwered in what I ſay in two or three paſſages on the thirty third experiment of my epiſtle; but after found, that it had been more fully anſwered (but upon grounds, ſome of which I do not need) by my learned friend Dr. *Ward*, with whom I thus take Mr. *Hobbes*'s argument to pieces. The cauſe, according to Mr. *Hobbes*, of the ſuſpenſion of the water in the veſſel is, that the water cannot thruſt away the air. 2. And it cannot thruſt that away, unleſs air ſucceed in its place. 3. But air cannot ſucceed in its place, unleſs either by getting in at the upper orifice, or at the holes that perforate the bottom. By which view of the argument it appears, that the main force of it lies in the ſecond propoſition: but neither doth he demonſtrate that (which omiſſion might excuſe us from any further anſwer) nor indeed do I think it true. For if the watering-pot were tall enough, what reaſon is there why the water ſhould not run out at the holes of it? as Monſieur *Paſchall*'s experiment mentioned in my epiſtle manifeſts; that though in a glaſs-tube hermetically ſealed at one end, and ſeveral times as long as a watering-pot, the water will not fall down, yet it will, if the tube exceed two or three and thirty foot, or thereabouts. And indeed the ſuſpenſion or deſcent of the water depends upon the proportion betwixt the weight of the aqueous pillar, that tends downwards, and the reſiſtance or preſſure of the air, that can come to bear againſt it. For as, on the one ſide, when the height of that pillar is ſo increaſed, as that it can outweigh the atmoſpherical cylinder, that oppoſes its deſcent, it will flow out till thoſe two cylinders come to an æquilibrium; ſo, on the other ſide, if inſtead of increaſing the length of the cylinder of the water, you leſſen the preſſure and reſiſtance of the air, the water will likewiſe deſcend, though the pillar be very ſhort, as I have ſhown in the nineteenth experiment; where, by withdrawing ſome of the air in the receiver, and thereby weakening its ſpring, the water, in a tube hermetically ſealed at one end, of but about four foot long, ſubſided about three foot; though that the ſpace relinquiſhed by the water was not full of air, as Mr. *Hobbes*'s argumentation requires it ſhould be, may be proved by what is there added, 'that by letting in the outward air, when the water was ſunk ſo low, it was immediately impelled up again to the higher parts of the tube.'

In ſome Dialogues of heat and flame.

And indeed (as I elſewhere diſcourſe) it ſeems to me a difficult matter for thoſe, that reject, as Mr. *Hobbes* juſtly does, that conceit of nature's abhorring a vacuum, and making it, as it were, her buſineſs to hinder it, to prove there can be no vacuum at all by any particular experiment. For if the fulneſs of the world be not made neceſſary, either by the nature of body in general, or by the deſign of the author of the univerſe, it can ſcarce be eaſy to prove by a particular experiment, that no human force or art can contrive a way of overcoming, at leaſt for ſome time, and as to ſome ſpace, either the gravity of fluid bodies, or whatever other quality of the air or water it is, by which the contiguity of the neighbouring parts of the world is wont to be maintained. As we ſee the water, that will not deſcend even in a tube of thirty foot,

foot (and thereby has made men think it will never deſcend whilſt the air is not permitted to ſucceed it) may, by our engine, be brought to ſubſide in a tube of about a foot long. And I ſhall here add this out of my (yet unpubliſhed) Dialogues of flame and heat; that whilſt only particular experiments are brought to aſſert the impoſſibility of a vacuum, perhaps the Vacuiſts will have the advantage on their ſide. For a thouſand experiments are not of that force, to prove univerſally, that a thing cannot be effected, as one, that ſhews it may be, is to prove the contrary. And the Vacuiſts have as well as the Pleniſts ſtore of experiments on their ſide, that ſeem to favour their hypotheſis; according to which, were it true, I ſee not why they may not ſolve the objections drawn from either the aſcenſion of liquors upon ſuction, or the non-deſcenſion of liquors in watering-pots cloſed at the top, or from any of the like experiments I have yet met with, in caſe the weight and ſpring of the air be taken in to ſolve the phænomena. And the Vacuiſts will have this advantage, that if Mr. *Hobbes* ſhall ſay, that it is as lawful for him to aſſume a plenum, as for others to aſſume a vacuum; not only it may be anſwered, it is alſo as lawful for them to aſſume the contrary; and he but barely aſſuming, not proving a plenum, his doctrine will ſtill remain queſtionable. But I think I could ſay more in favour of the Vacuiſts experiments; namely, that whereas in ſome phænomena of the Torricellian experiment, and in many of thoſe of our engine, Mr. *Hobbes* proves the ſpace deſerted by the quickſilver or the air to have no vacuity, becauſe according to his ſuppoſition the world is full; (and not by any ſenſible phænomena, that prove the ſpace in queſtion to be perfectly full:) for no leſs fulneſs is requiſite to the truth of his hypotheſis: the Vacuiſts on the other ſide need not go about to prove, that thoſe ſpaces are not full by their hypotheſis: but they prove it by this, that it appears by ſenſible phænomena, that the quickſilver deſerts the upper part of the tube; and that much air is pumped out of our receiver. (The firſt of which is evident to the eye; and ſo is the other too, when the pump is kept under water.) But it does not appear by the like phænomena, that the air (as Mr. *Hobbes* would have it in his elements) does ſucceed to fill, I ſay, perfectly to fill the deſerted ſpace; which alſo they will confirm hence, that in the Torricellian experiment, by inclining the tube, the relinquiſhed ſpace may be again readily filled with mercury; and if our exhauſted receiver be plunged under water, that liquor, when acceſs is given it to the cavity, violently ruſhes into it, and almoſt fills it up.

From all which it ſeems probably deducible, that it is a very hard thing, by Mr. *Hobbes*'s way of managing the controverſy, to prove, that there can be no vacuum. But as for the Carteſians more ſubtile and plauſible way of aſſerting a plenum, it concerns me not here to diſpute againſt it, or declare for it.

I will add this, and but this, on the occaſion of Mr. *Hobbes*'s building a great part of his philoſophy upon no ſurer a ground, that we may hence learn, how little reaſon there is to blame me, as he is pleaſed to do, for making elaborate experiments; and that though (as I have elſewhere purpoſely and amply diſcourſed) obvious experiments are by no means to be deſpiſed, yet it is not ſafe in all caſes to content one's ſelf with ſuch; eſpecially when there is reaſon to ſuſpect, that the phænomenon they exhibit may proceed from more cauſes than one, and to expect, that a more artificial trial may determine, which of them is the true.

Thirdly, whereas Mr. *Hobbes* is pleaſed to find much fault with the ſociety, and me, for not aſſigning the cauſe of ſprings in general; that omiſſion ſeeming to him very unworthy of philoſophers: I anſwer, that the ſociety having hitherto, for weighty reaſons, forborn to determine the particular cauſes of things, there was no reaſon they ſhould alter their method, for experiments, that were not made or publiſhed by them or by their order. And as for me, the title of my book promiſes ſome experiments touching the ſpring of the air and its effects, not ſpeculations of the cauſes of ſprings in general. My avowed intention was candidly to communicate with the curious ſome experiments which, I thought, their novelty would render acceptable to them, wherein I have the good luck not to have been miſtaken; nor can I be juſtly cenſured for not performing what I did not undertake, nor was obliged to. And perhaps Mr. *Hobbes* would more prejudice the commonwealth of learning by his ſeverity, than he has yet advantaged it by any other way, if he could obtain, that none ſhould publiſh an experiment or obſervation that cannot, by deduction from the firſt and catholic principles of philoſophy, aſſign the true cauſe of it.' But when I take upon me to write, as Mr. *Hobbes* has done, elements of philoſophy, then perhaps I ſhall be able to give an account of ſprings, not much more unſatisfactory than others think his. For though he refers us to his explication given of the motion of reſtitution in his book *De corpore*; yet in the 22d chapter, and 30th ſection, which profeſſedly contains his theory of it, after having premiſed (what rightly interpreted may be true enough) that the cauſe of the reſtitution proceeds not from the taking away the force, by which they were compreſſed or extended (' the ' removing of impediments not having the efficacy of a cauſe') that which follows to the end of the ſection is only this: ' The cauſe therefore of their reſtitution is ſome ' motion either of the parts of the ambient, or of the parts of the body compreſſed ' or extended. But the parts of the ambient have no endeavour, which contributes ' to their compreſſion or extenſion, nor to the ſetting them at liberty or reſtitution. ' It remains therefore, that from the time of their compreſſion or extenſion, there be ' left ſome endeavour (or motion) by which the impediment being removed, every ' part reſumes its former place; that is to ſay, the whole reſtores itſelf.' Now this notwithſtanding, I am ſo dull or ſo wary, that though I had met with this paſſage, and all the praiſes the author in his Dialogues gives it, yet I ſhould have made ſome ſcruple to undertake the aſſigning the true cauſe of ſprings in general. For firſt, the learned *Gaſſendus*, and the Epicureans both ancient and modern, together with divers other Naturaliſts, do not admit what Mr. *Hobbes* ſuppoſes a few lines before, that ' that which is at reſt cannot be moved, but by a moved and contiguous mo' vent.' For they think motion, or at leaſt *conatus ad motum*, an unlooſeable property, congenit to matter. And, by the way, whatever exceptions I have to this opinion, yet I am not ſatisfied with that principle of Mr. *Hobbes*, though it be the fundamental one of his philoſophy, unleſs it be more warily propoſed. For to aſſert univerſally and without exception, as he does in his Elements, that nothing can be moved but by a body contiguous and moved; I do not take to be true, nor conſiſtent with his other aſſumptions. This I elſewhere (in a diſcourſe againſt another than Mr. *Hobbes*, about the Chriſtian religion) prove more at large: but now it will ſuffice to repreſent, that Mr. *Hobbes* not only admitting, but making uſe in his philoſophy of the creation of the world, either he muſt allow, that motion is natural to ſome, if not all parts of matter; or that God put them into a motion not included in their nature. From which it will neceſſarily follow, that at leaſt ſome bodies may have motion, though it be not given them by any body contiguous and moved, as an attentive conſiderer may eaſily diſcern. But to return to the cauſe of ſprings.

SECONDLY, whereas Mr. *Hobbes* aſſumes, that the parts of the ambient have no endeavour, which contributes to their compreſſion or extenſion, nor to the ſetting of them at liberty or reſtitution; he ſays this indeed, but does not go about to prove it. And I ſhould the leſs have made this precarious aſſertion, becauſe that after the celebrated *Des Cartes* himſelf, the Carteſian philoſophers generally aſcribed the motion of reſtitution to the paſſage of a ſubtile æthereal ſubſtance (and an æther Mr. *Hobbes* alſo admits) through the pores of the ſpringy body, which ſtriving to obtain its wonted freedom of paſſage, reſtores them to the ſhape and bigneſs, from which they were forced. Nay, I ſhall have occaſion to ſhew anon, that Mr. *Hobbes* himſelf, whatever he ſays in this place, does elſewhere aſcribe a motion of their own to multitudes of terreſtrial corpuſcles. And I might add, that elſewhere he ſpeaks of the rekindling of the fire taken out of the receiver after this manner: *Quanquam vis illius* Page 18. *motus in recipiente (ut loquimini) evacuato diminuta ſit, oppreſſa ab aëris intus commoti conſiſtentia, non tamen extinguitur*; *& propterea levata oppreſſione, ſatis habebit virium ad excitandum phantaſiam lucis quanquam debiliorem*: ‘ Although the force of that motion in the evacuated receiver be diminiſhed, being oppreſſed by the conſiſtence of ‘ the air moved within, yet it is not extinguiſhed: and therefore that oppreſſion ‘ being taken off, will have ſtrength enough to excite an appearance of light, though ‘ ſomewhat weaker than ordinary.’ But I ſhall rather ſubjoin, that yet, thirdly, I do not think it improbable what the learned *Gaſſendus* had taught, and what Mr. *Hobbes* here teaches, that the reſtitution of bent ſprings may proceed from a certain endeavour or motion in their internal parts (left from the time of their compreſſion or extenſion) which when the impediment is removed, makes every part reſume its former place, and thereby makes the whole reſtore itſelf. But this notwithſtanding, I feared others might be as inquiſitive as myſelf, and might expect from him, that would undertake to ſettle a general theory of the motion of reſtitution, the clear and diſtinct explication of ſeveral phænomena, that I had met with, which are not touched, nor perhaps were, ſome of them, thought upon by Mr. *Hobbes*. As firſt, why ſuch a determinate temper of iron and ſteel is requiſite to make it elaſtical; ſo that if after having been hardened and gradually heated, it be ſuddenly cooled, at an inconvenient point of time, it will be brittle, and fit to make gravers and other rigid tools, not ſprings? Next, why bows and other elaſtical bodies, if they be kept too long bent, loſe in proceſs of time almoſt all their elaſtical power, and continue crooked? Thirdly, why, not only divers ſolid bodies as well as lead and gold, which before trial one would think as likely, as many ſpringy ones, to have their parts put into a due motion by the force that bends them, ſhould be devoid of an elaſtical power? Fourthly, what kind of motion, and what kind of texture it is, by virtue whereof, the parts of a body being for a very ſhort time put into motion, do ſome months, perhaps ſome years, retain in great part a ſmart motion, without in ſo long a time communicating it to the ambient bodies, to ſome or other of which multitudes of them are perpetually contiguous, and thereby loſing it themſelves? Why upon ſuch a bare and inartificial change made in the texture of a body, as is ſcarce at all diſcernable to the eye, it ſhould acquire a ſtrong ſpring, that it had not before (as I have tried upon ſilver and copper, which though flexible before they were hammered, yet being beaten into thin plates obtained a notable ſpring:) and why (which may ſeem more ſtrange, upon another light change of texture) the acquired ſpring may preſently be loſt again; as I have tried in ſilver, that, chymiſts teach us, loſes nothing in the fire, which having by being hammered acquired a ſtrong ſpring, we have preſently made flexible again as before, by only heating it red-hot, without ſo much as melting it; which argues, that in ſprings texture is as well to be conſidered as motion? To theſe I might add other particulars, that I had either made or obſerved (and mention in another treatiſe) concerning ſprings; all which phænomena perhaps every one, that has read what we have lately recited out of Mr. *Hobbes*, will not preſently be able ſatisfactorily to explicate. So that I hope the equitable reader will not think it a fault, that (contenting myſelf to propoſe the two explications of ſprings, I ſaw moſt liked among the curious; to which I ſhould have added Mr. *Hobbes*'s, if I had found it as much eſteemed) I declined engaging myſelf in controverſies about the origin of motion, and ſuch other high ſpeculations, as, had my abilities enabled me, neither my deſign exacted, nor my leiſure permitted that I ſhould proſecute. And though Mr. *Hobbes* be pleaſed to ſpeak thus of his notion concerning the reſtitution of bodies; *Sine qua hypotheſi quantuſcunque labor, ars, ſumptus, ad rerum naturalium inviſibiles cauſas inveniendas adhibetur, fruſtra erit:* 'Without which hypotheſis, let never ſo much labour, art, coſt be beſtowed for the finding out of the inviſible cauſes of natural things, all will be in vain.' Yet whether that bold aſſertion ſhould paſs for an argument, for an hyperbole, or for a compliment to himſelf, I am content to let the reader judge. Page 8.

FOURTHLY, Mr. *Hobbes* in divers paſſages, wherein he diſputes againſt me, ſeems to have miſapprehended my notion of the air. For when I ſay, that the air has gravity and an elaſtical power, or that the air is, in great part, pumped out of the receiver, it is plain enough, that I take the air in the obvious acception of the word, for part of the atmoſphere, which we breathe, and wherein we move. Nor do I find, that any other of my readers do otherwiſe underſtand me. But Mr. *Hobbes* ſeems to think he has ſufficiently confuted me, if in ſome caſes he have proved (which whether he have done well or no, is not here to be examined) that there is a ſubtile ſubſtance, which he calls æther (but which I wiſh he had better explained) in ſome places, which I take not to be filled with air; and that the æther has or has not ſome accidents, which I deny or aſcribe to the air. Whereas I deny not, but that the atmoſphere or fluid body, that ſurrounds the terraqueous globe, may, beſides the groſſer and more ſolid corpuſcles wherewith it abounds, conſiſt of a thinner matter, which for diſtinction-ſake I alſo now and then call ethereal. And therefore though I did not think myſelf obliged to declare againſt either the atomical or the Carteſian hypotheſis, touching the nature of the air, yet I propoſed the latter too as probable (which as it excludes a vacuum, ſo it makes the air conſiſt in great part of a celeſtial matter). And my incidental explications of the rarefaction and condenſation of the air, together with my comparing it to a fleece of wool, ſufficiently declare, that I take it not to be a homogeneous body; and though there be air intercepted betwixt the hairs of wool, yet in caſe I ſhould prove, that a box were not ſo full of wool as before, becauſe the moſt part of the hairs had been taken out, I ſhould not think he argued well againſt me, that ſhould only prove, that the box contained as much of matter, conſiſting of air and wool together, afterwards as before. Nor do I think Mr. *Hobbes* has in divers paſſages, wherein he ſuppoſes he diſputes againſt me, much more directly contradicted what I teach concerning the air, if that word be rightly and in my ſenſe underſtood. And on this occaſion I muſt crave leave to add, that whereas he is pleaſed to intimate, that I miſrepreſent the Carteſian hypotheſis, aſcribing that to the air, which *Des Cartes* does to water; if the reader think it worth while to compare the ſummary account I give of that hypotheſis, with what *Des Cartes* himſelf has taught in his 45th, 46th, and 47th articles of the fourth part of his Principles, wherein that author compriſes his doctrine of the nature of the air; he will quickly find, that whether or no Mr. *Hobbes* be miſtaken, I am not, unleſs it be in eſtimating his hypotheſis by what he teaches in his principles, which were publiſhed after his meteors, and more elaborately written. And as for that particular, which alone Mr. *Hobbes* alleges, namely, that he makes not the parts of air but of water ſo flexible; *Des Cartes*'s words in the 46th article are theſe: *Cum ejus particulæ ferè omnes ſint flexiles inſtar mollium plumularum vel tenuium funiculorum, &c.* 'Seeing almoſt all its parts are flexile, like little ſoft feathers or fine threads.' And as for what Mr. *Hobbes* ſubjoins, *Sed quiſquis talis ſuppoſitionis author fuit, parum refert. Nam ipſa hypotheſis, in qua motus ſupponitur materiæ ſubtilis ſine cauſa velociſſimus, & præterea corpuſculorum innumerabiles vertigines diverſæ ab illius materiæ unico motu generatæ, vix ſani hominis eſt.* 'But it matters not, who was the author of that ſuppoſition. For 'the very hypotheſis itſelf, wherein is ſuppoſed a motion of ſubtile matter, which is 'ſwift without any cauſe aſſigned, and hath moreover divers innumerable circulations 'of corpuſcles generated from the ſingle motion of that matter, is not the conceit 'of a man of wit or ſenſe.' I cannot but in gratitude to ſuch a perſonage declare my diſlike, to find him upon ſo ſlight an occaſion ſo coarſely uſed for an opinion the cenſurer of it does no better confute, and which is thought to be in ſome particulars not ſo unlike his own. And perhaps I ſhould be afraid, that Mr. *Hobbes*'s ſpeaking ſo ſeverely of one, that was at leaſt a famous geometrician, might reflect upon the Engliſh civility in the opinion of ſtrangers, if I did not hope, that thoſe, who have read Dr. *Ward*'s exercitation, will look upon this cenſure of the Carteſian doctrine by Mr. *Hobbes*, as provoked by that ſevere judgment of *Des Cartes* mentioned by the doctor in theſe words: *Nempe hoc eſt, quod alicubi admiratus eſt magnus Carteſius; nuſquam eum, ſive verum, ſive falſum poſuerit, rectè aliquid ex ſuppoſitionibus ratiocinando inferre:* 'This is the thing, that the great *Des Cartes* ſomewhere admired, that he, 'whether his poſitions are true and falſe, doth never in argumentation make any right 'inference from his ſuppoſitions.'

Wardi Exer. in Philoſoph. Hobbian. p. 188.

CHAP. III.

Wherein the weight and ſpring of the air are aſſerted, againſt Mr. Hobbes.

HAVING thus diſpatched thoſe general conſiderations, I thought expedient to premiſe, my propoſed method leads me, in the next place, to conſider, that Mr. *Hobbes* does not, that I remember, deny the truth of any of the matters of fact I have delivered. Nor does he, if my memory fail me not, labour to prove, that the explications I have given of my experiments are not agreeable to the doctrine I propoſed: but rather thinks fit to reject our two grand hypotheſes themſelves, the weight, and the ſpring of the air. And therefore it will ſuffice us in this chapter briefly, but not ſlightly, to prove what he is unwilling to grant.

AND firſt, that the air (in the ſenſe wherein we take the word) is not devoid of weight, we have proved by divers experiments: which having more fully delivered in the book itſelf, it may in this place ſuffice now to name them.

ONE then of theſe experiments, that prove the air's gravity, is, that we found a blown bladder carefully weighed in an exact pair of ſcales manifeſtly heavier, when full of air, than when the air was let out.

NEXT, it has been obſerved in our 36th experiment, that an æolipile, being well heated, and the little hole left at the top of the pipe being ſtopt, when it was thus hot; upon the opening of that hole, when the æolipile was grown cold again, the external air ruſhing in with a whiſtling noiſe at the forementioned orifice, made the æolipile weigh ſo much more than it did juſt before the external air got in, that it amounted, by computation, to near a thouſandth part of the weight of an equal bulk of water. And though ſome difficulty may perhaps be moved touching the accurateneſs of the proportion this way found out, betwixt the gravities of thoſe bodies; yet that the one as well as the other is actually heavy (which is all that we here need contend for) the experiment ſufficiently manifeſts.

Exper. Magdeburgicum apud Schotum, p. 446.

THIRDLY, in the Magdeburgic experiment (mentioned at the beginning of our epiſtle) the ingenious makers of it found, that, having before weighed the great receiver they were to exhauſt, and having done the like after the extraction of the air, they found it to weigh one whole ounce and $\frac{3}{10}$; *quod ſane* (ſays the learned publiſher, though a Peripatetic) *luculentiſſimum eſt argumentum gravitatis aëris*; ‘which is indeed ‘a moſt evident argument of the weight of the air.’

FOURTHLY, in our 36th experiment we relate our having weighed the air, and that ſhut up in bodies in our exhauſted receiver, wherein of two bodies of differing natures (the one a blown bladder, and the other a glaſs bubble) that were æquiponderant each to a more ſolid weight before the air was pumpt out, that which included a good quantity of air did manifeſtly preponderate after the exhauſtion.

AND to theſe four we might add other proofs to the ſame purpoſe; but that theſe contain in them ſuch a variety of caſes, that I think it would be ſuperfluous.

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BUT now let us ſee, what Mr. *Hobbes* objects againſt the newly mentioned experiment of the bladder weighed in the exhauſted receiver for the others he quarrels not with:) *Quod quidem lanx* (ſaith he) *in qua eſt veſica, magis deprimitur quam altera, certi eſſe poſſunt, oculis teſtibus: quod autem id à gravitate aëris naturali accidit, certi eſſe non poſſunt*; *præſertim ſi quæ ſit gravitatis cauſa efficiens neſciunt*: ‘that the ſcale, in which ‘the bladder is, is more depreſt than the other, they may be certain, their eyes ‘bearing them witneſs; but that this comes from the natural gravity of the air he ‘cannot be aſſured; eſpecially if they are ignorant what is the efficient cauſe of ‘gravity.’ But I know not whom Mr. *Hobbes* will perſuade, that a man cannot be ſure, that lead is *in ſpecie* heavier than cork, unleſs he knows what is the efficient cauſe of gravity. And Mr. *Hobbes* ſpeaks in his 30th chapter (where he expreſsly treats of that ſubject) as if that had not been explained by any man, and conſequently not by any writer of ſtatics; (and perhaps I am therein ſomewhat of his mind) and yet ſure all theſe writers, treating of the proportion of heavy bodies, did not write they knew not what. And, though he mentions his own hypotheſis, as that *than which nothing is more likely*; yet I think I could frame objections againſt it, that would not eaſily be anſwered, if my preſent taſk required it; or if I found his opinion, in this point, embraced, as yet, by men of note. Wherefore I ſhall now ſay no more of it than he himſelf doth, namely, that according to his doctrine, ‘It may well be ‘thought to determine (for it is a certain conſequent) that heavy bodies deſcend ‘with leſs and leſs velocity, as they are more and more remote from the æquator; ‘and that at the poles themſelves they will either not deſcend at all, or not deſcend ‘by the axis: which whether it be true or falſe, experience muſt determine.’ Which till it have done in his favour (an event I do not expect) I hope he will allow me to diſtruſt his hypotheſis.

Elem. of Phil. Ch. 30. *Sect.* 4.

Page 16.

BUT to return to our experiment. The account he gives, why the bladder does propend (for ſo he loves to ſpeak) is this; *Quod veſica ſive follibus ſive flatu oris diſtenta ſit, gravior ſit quam eadem veſica non diſtenta, negare nolo, propter majorem quantitatem atomorum follibus vel corpuſculorum fuligineorum ab halitu inflatorum. Ab experimento autem quod fit à veſica inflata nihil colligunt quod ſit ſatis certum. Oportuit lancibus imponere duo vaſa pondere æqualia, quorum alterum eſſet accuratè clauſum, alterum apertum: ſic enim non inflatus ſed incluſus tantum aër ponderatus eſſet. Quando igitur aërem ſic ponderatum videbis, meditabimur poſtea quid dicendum ſit de phænomeno quod retuleris*: ‘that

‘ the bladder, whether it be blown up with a pair of bellows, or with the breath of ‘ one's mouth, is heavier than when it is not blown up, I will not deny, becauſe of ‘ the greater quantity of atoms from the bellows, or of fuliginous corpuſcles, that ‘ are blown in from the breath. But notwithſtanding they gather nothing of ſufficient ‘ certainty from this experiment of a blown bladder; they ought to have put into ‘ the ſcales two veſſels of equal weight, whereof one ſhould be ſhut and the other ‘ open: for by this means air not blown in, but only incloſed, had been weighed. ‘ When therefore you ſhall ſee air ſo weighed, we will afterwards conſider ‘ what may be ſaid concerning the phænómenon you bring.’ But as to the firſt part of this paſſage, it does not deny the gravity of what we call the air; but only endeavours to ſhew what parts they are that make it heavy. And as to the ſecond, he ſeems to miſtake the preſent caſe. For, there is no need, that the air in the bladder be, before the exhauſtion of the receiver (in which the foregoing, fifteenth page, he declares he ſuppoſes the experiment to be made) heavier than the outward air. Wherefore when he ſubjoins, that from this experiment we collect nothing *quod ſit ſatis certum*, the affirmation is not an inference, but precarious. And as for the annexed way, whereby he would wiſh to have an experiment made fit to infer the gravity of the air, if he had not overlooked what I have delivered in the beginning of the 36th experiment, he would eaſily have perceived, that we did make a trial much of the ſame nature with that he deſires. For we weighed in our receiver the air, in a glaſs hermetically ſealed; wherein it was not (to uſe his expreſſion) *inflated*, but only included. This is what he here objects againſt the gravity of the air in the other place (pag. 8, and 9.) where he ſays ſomething to this controverſy: he inculcates alſo, that we ſhould firſt explicate what is gravity, and then adds, *Quod* Page 9. *atmoſphæræ inſunt permiſtæ corpori æthereo multæ tum aquæ tum etiam terræ particulæ, facile perſuadeor*; *ſed quod in medio æthere, ſurſum, deorſum, quaquaverſum motæ, nec ſemper alteræ alteris innitentes gravitent, inconceptibile eſt*: ‘ That many particles, both ‘ of earth and water, mingled with the æthereal body, are in the atmoſphere, I am ‘ eaſily perſuaded: but that in the middle of the æther they ſhould move upwards, ‘ downwards, every way, and that one leaning on the back of another they ſhould ‘ not gravitate, is a thing utterly unconceivable.’ To which he adds two or three reflections, whoſe examen being here unneceſſary, would require more time than perhaps it would (in reference to the preſent controverſy) deſerve: for we are now enquiring, not how the air comes to gravitate, but whether or no it have gravity. And Chap. 30. Sect. 10. ſince in his Elements of Philoſophy he grants, and gives his reaſon for it, that ‘ if ‘ air be blown into a hollow cylinder, or into a bladder, it will increaſe the weight of ‘ either of them a little:’ and ſince here he likewiſe confeſſes (as we have juſt now ſeen) that there are mingled with the æther many aqueous and earthly (and conſequently heavy) particles: he confeſſes that, which we labour to evince; namely, that the air is not devoid of weight. And it concerns us no more than himſelf, to ſhew how the corpuſcles, upon whoſe account the air is heavy, make it ſo. And this being what Mr. *Hobbes*, in ſeveral places, thinks fit to object againſt the gravity of the air; the reader will, I ſuppoſe, eaſily take notice, that he has left the experiment of the æolipile, and ſome others, unanſwered. Though theſe alone prove, that the air is a manifeſt weight even when it is not compreſt, but retains its laxity. Having ſaid thus much to evince againſt Mr. *Hobbes* the gravity of the air, let us now examine, whether it have not alſo a ſpring (in the ſenſe we take that word in *.)

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* *Utraque enim illa phantaſia, tum gravitatis atmoſphæræ, tum vis elaſticæ ſive antitupiæ aëris, ſomnium erat.* Dial. pag. 21.

This, though Mr. *Hobbes* be pleaſed to call (as he alſo does the weight of the air) *a Dream*; yet he does himſelf grant, in effect, as much as is requiſite to prove the ſpring of the air, in the ſenſe I contend for it. For taking upon him to give account (how good an one we ſhall ſee anon) of that known experiment, wherein the air is compreſt in a glaſs-bottle by the forcible injection of water; which water, when the glaſs is unſtopt, the air does again throw out in recovering its former dimenſions; of this experiment (I ſay) he gives this account (pag. 24.) *Aër, quo ab initio ſphæra plenus erat à corpuſculis illis terreis motus motu circulari ſimplice, vi injectionis coactus, qui quidem purus eſt, exit (aquam injectam penetrans) in aërem extrinſecum, locum relinquens aquæ; ſequitur ergo corpuſculis illis terreis minus relinqui loci, in quo motum ſuum naturalem exercere poſſint: itaque in ſe mutuo impingentes aquam urgent ad egreſſum; egredientem aër externus (quia univerſum ſupponitur eſſe plenum) penetrat, locumque egredientis aëris ſucceſſivè occupat, donec corpuſcula, quantitate aëris eadem reſtituta, libertatem motui ſuo naturalem recipiant.* 'The air, with which in the beginning the ſpherical glaſs was full, being moved by thoſe earthy corpuſcles in a ſimple circular motion, and being compreſt by the force of the injection, that of it which is pure (penetrating the injected water) gets out into the open air, and gives place to the water. It follows therefore, that thoſe earthy corpuſcles have leſs place left, in which they can exerciſe their natural motion: therefore beating one upon another, they force the water to go out; it thus going out, the external air (becauſe the univerſe is ſuppoſed to be full) penetrates it, and ſucceſſively takes up the place of the air, that goes out, until the corpuſcles, the ſame quantity of air being reſtored, regain a liberty natural to their motion.'

But how little this comes ſhort of granting as much ſpring to the air as the Carteſians do, and as I need require, may eaſily be judged by divers paſſages in our book; and particularly by our propoſing, as not improbable, the Carteſian way of explicating the ſpring of the air; according to which, the corpuſcles, that ſwim in the æther, being each hindered by the neighbouring ones from the freedom of its motion, they beat off one another (which Mr. *Hobbes* would have them do:) whence it comes to paſs that, in any aſſigned portion of air here below, the corpuſcles, that compoſe that portion, beaten off by one another, do make the whole portion tend to obtain (though not exactly to fill up) more room, and conſequently to emulate a ſpring, like that which we ſcruple not to aſcribe to a compreſt fleece of wool, becauſe of a like endeavour to expand itſelf.

We may inforce this by another paſſage of Mr. *Hobbes*'s, that ſpeaks expreſsly enough to our preſent purpoſe, where he gives this reaſon of one of the phænomena of our engine: *Quoniam per ſuctoris retractionem aër purus impulſus erat, partes autem terreæ impulſæ non erant; major erat ratio particularum terrearum, quæ extra cylindrum ſuctori contiguæ erant, ad aërem purum, in quo motum ſuum exercebant, poſt revulſionem quam ante: quare particulæ illæ motæ minus habentes loci ad motum ſuum naturalem exercendum, aliæ aliis impingebant, & propellebant: neceſſe ergo erat, ut particulæ, quæ ſuctoris ſuperficiei contiguæ erant, ſuctorem propellerent**: 'becauſe by the drawing back of the ſucker the pure air was thruſt in, but the earthy parts were not thruſt in; there was a greater proportion of earthy particles, which, without the cylinder,

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* The motions therefore of thoſe ſmall bodies (ſpeaking of the earthy particles in the air) will be leſs and leſs free, by how much the quantity of the injected water is greater and greater: ſo that by their motion falling upon one another, the ſame bodies will mutually compreſs each other, and have a perpetual endeavour of regaining their liberty, and of depreſſing the water that hinders them. *Elem. chap.* 30. *ſect.* 9.

‘ were near the ſucker, unto the pure air, in which they exerciſed their motion as ‘ well after this revulſion as before. Wherefore theſe particles ſo moved having leſs ‘ place to exerciſe their natural motion in, ſome of them fell foul and beat upon the ‘ reſt. So that of neceſſity the particles, that were near the ſurface of the ſucker, ‘ muſt drive it upwards.’ To which we may add, that Mr. *Hobbes* himſelf ſeems rather to reject other men's ways of propoſing the ſpring of the air, than reſolutely to deny the thing itſelf. For, *Vidiſti* (ſays he) *jam elaſtrum illud aëris, quod ſupponunt, aut impoſſibile eſſe, aut recurrendum eſſe ad hypotheſin Hobbianam:* ‘ you ſee now, that ‘ the ſpring of the air, which they ſuppoſe, is either an impoſſible thing; or they ‘ muſt (for its defence) have recourſe to the hypotheſis of Mr. *Hobbes*.’

But beſides Mr. *Hobbes*'s conceſſions in the paſſages newly recited, and ſome others, we can prove the ſpring of the air by many of the phænomena of our engine, which we have deduced from it, and of which he does not offer any other way of explication. Wherefore we ſhall now content ourſelves to prove the ſpring of the air by two experiments; the one not mentioned in our epiſtle, and the other much oppoſed by Mr. *Hobbes*.

And firſt, if you make the Torricellian experiment in a tube of between two foot and a half and three foot in length; and if, when the mercury reſts at its wonted ſtation, you dextrouſly ſtop the orifice of the tube with your finger (that orifice being lifted up as near the ſurface of the reſtagnant mercury as it can be, without giving admiſſion to the external air) and if then you quite lift up the tube thus ſtopt into the free air; you ſhall feel, upon your finger, little or no gravitation or preſſure from the weight of the mercurial cylinder, diſtinct from the weight of the tube: becauſe (as we have more fully explicated this phænomenon elſewhere) the gravity of the quickſilver is balanced by that of the outward air, that thruſts the finger againſt it. But if you invert the tube, and having let in the air at the orifice, ſtop it again with your finger, and again let the mercurial cylinder lean upon that finger; you ſhall then find your finger ſtrongly preſt, and endeavoured to be thruſt away: which new preſſure, ſince it cannot come from the mercury, that being the very ſame, that was in the tube before, nor from the weight of the admitted air, which perhaps may not amount to ſo much as a grain, to what can we rationally aſcribe it, but to the ſpring of the included air, whoſe force will be as well manifeſt to the eye as the finger, if the tube be unſtopt under the ſurface of the reſtagnant mercury; for then that in the glaſs will not reſt, as before, at the uſual ſtation, but be depreſt beneath it a good way, perhaps ſome inches? And if you make the Torricellian experiment in a ſhort tube ſealed at neither end, but ſtopt above and below with your fingers, you ſhall find, upon the unſtopping of the upper orifice a new and forcible preſſure upon the finger, that keeps the lower orifice ſtopt, made by the gravitation of the external air, which was before kept off from leaning upon the mercurial cylinder by the upper finger; the pulp of which finger, by that gravitating air, was before thruſt into the deſerted cavity of the tube (as we have elſewhere diſcourſed, in a fuller meaſure, of theſe experiments). Which will evince, againſt Mr. *Hobbes*, both the ſpring of the air and gravity of the atmoſphere; ſince he is, as little as I, for aſcribing theſe phænomena to the efficacy or abſence of my other antagoniſt's imaginary Funiculus.

In our Defence, againſt Linus.

The other experiment I ſhall mention is the fourth in our epiſtle; namely, that of the ſwelling and ſhrinking of a bladder hung in our receiver, according as the ambient air, and conſequently its preſſure, is withdrawn or ſuffered to return. But though this experiment be ſo congruous to our hypotheſis, that it is generally acquieſced in by thoſe ingenious men, that have hitherto ſeen it; yet Mr. *Hobbes* is

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pleaſed to reject our explication, and ſubſtitute another in theſe words, which is all he has concerning this matter: *Quia cuticula omnis ex filiculis conſtat, quæ propter figuras contactum per omnia puncta accuratum habere non poſſunt, pervia ergo eſt veſica, cum ſit cuticula, nec aëri tantum, ſed etiam aquæ, qualis eſt ſudor. Eadem ergo aëris per vim incuſſi eſt compreſſio intra veſicam, quæ extra, cujus conatus, propter viam motuum undiquaque decuſſatum, tendit undiquaque ad ſuperficiem veſicæ concavam. Quare neceſſarium eſt, ut undiquaque intumeſcat, & creſcente conatûs vehementiâ tandem laceretur.* 'Becauſe every ſkin is made up of ſmall threads or filaments, which by reaſon of their figures cannot accurately touch in all points; the bladder therefore, being a ſkin, muſt be pervious not only to air, but to water alſo, as to ſweat: therefore of the air beat in by force, there is the ſame compreſſion within the bladder, that there is without. The endeavour of which, the way of its motions being every way croſs, tends every way to the concave ſuperficies of the bladder. Wherefore it is of neceſſity, that it muſt ſwell every way, and the vehemency of the endeavour increaſing, be torn at laſt.' But, if this be a ſufficient anſwer to ſuch an experiment, I confeſs I fear it will be harder than we are yet aware of, to prove any thing by experiments.

FOR, firſt, how unlike is it to be true what he affirms, and what his reply ſuppoſes, namely, that ſuch bladders as we uſed are readily pervious to the air; when eaſy experience ſhews us, that by leiſurely compreſſing ſuch blown bladders betwixt our hands, we ſhall rather break them (as we have tried) than ſqueeze out the air at the pores? So that the reſt of his anſwer being built upon what is ſo repugnant to common experience, will not need a particular confutation: but however, *ex abundanti* we will add, that in our 36th experiment we relate, that by the exhauſtion of the air we likewiſe broke a glaſs hermetically ſealed; and to ſay that glaſs alſo is pervious to air, were to affirm what the greateſt part of his book ſuppoſes to be falſe. Beſides, whereas there is not any ſenſible and unqueſtionable phænomenon to prove, that the receiver is full of any ſuch *aër per vim incuſſus* as he would have, we ſee plainly, that when the air does manifeſtly get into the receiver, the bladder is not thereby made to ſwell, but ſtrangely to ſhrink. Moreover (ſince according to Mr. *Hobbes*) the bladder is pervious to the air, and the air within the receiver is univerſally compreſt, as well that which is within the bladder as that which is without it; how comes it to paſs, that the air, that bears againſt the convex ſurface of the bladder, does not reſiſt that which is contiguous to the concave ſuperficies of the ſame; and at leaſt, how comes the bladder to be broken by the air; which, according to Mr. *Hobbes*, can get in and out at pleaſure? And laſtly, to ſhew, that to the ſwelling of the bladder there needs nothing but the ſpring of the included air, and not ſuch vehement agitation of the ambient air, as Mr. *Hobbes* ſuppoſes to be made in our engine; it appears by the elſewhere-mentioned experiment of Monſieur *Paſchal*, that in the free and ordinary air a foot-ball half blown up will ſwell more and more, the nearer it is carried to the top of an high mountain; where the incumbent cylinder of the atmoſphere is ſhorter, and its weight lighter; and will, for the contrary reaſon, grow more and more flagged, the nearer it approaches again to the foot of the mountain.

THOUGH I doubt not, but the arguments employed in this chapter will be ſufficient to convince impartial readers; yet I ſhall add, by way of inforcement, that whereas Mr. *Hobbes* aſcribes the weight of the air in bladders to the earthy corpuſcles intruded by him, that blows them up; and attributes the ſpring of the air in the wind-gun, and in the phænomena of our engine, to the violent motion the air is put into by the vehement impulſes of the rammer or ſucker; our doctrine may be evinced by experiment, wherein the air in its natural and wonted ſtate operates, without being forcibly compreſt or put into motion by us. This may appear by the two ſorts of experiments to be made upon high mountains, which we have mentioned and urged in the *ſecond part* of our *defence againſt the learned* Linus. Wherefore referring the reader thither, we ſhall now only, in very few words, mention the ſubſtance of them.

The firſt experiment is, That it has been found upon trial, both formerly in *France* and ſince in *England*, that the quickſilver in the Torricellian experiment falls notably lower at the top of a mountain than at the foot, (by Monſieur *Paſchal*'s obſervation upon a hill, far higher than thoſe the experiment was tried on here, the difference was ſo great, as to amount, as the moſt ingenious *Pecquet*, a happy promoter of experimental learning, informs us, to above three inches) which we ſay is cauſed by this, that the atmoſpherical cylinder is much lighter, as well as ſhorter at the top of the mountain than at the bottom. And Mr. *Hobbes* diſallows not the experiment, but yet gives only this account of it: *Sed & particulæ illæ, quæ interſperſæ aëri ita moventur, ut ſuppoſuimus, magis confertæ ſunt ad radicem montis quàm in ſummo, nam hoc quoque ſuppoſuimus.* But what then? How does the plenty of theſe interſperſed particles hinder the mercurial cylinder from deſcending at the bottom of the hill as much as at the top, unleſs by their gravity or preſſure? And it is very unlikely, that the earthy atoms, contiguous to the reſtagnant mercury at the bottom of the hill, ſhould be able, by their weight, to keep ſuſpended a cylinder of mercury of above three inches, unleſs the contiguous air were gravitated upon by the weight of other incumbent parts of the atmoſphere. *Page* 12.

The other of the two mentioned experiments is briefly this, That a thermoſcope being carried from the bottom to the top of a hill, the included air, inſtead of ſhrinking in that colder region, manifeſtly dilated itſelf, and notably depreſſed the water. An effect which I ſee not to what it can well be attributed but to the ſpring of the included air, which having not near ſo great a preſſure againſt it from the atmoſphere incumbent on the reſtagnant and ſuſpended water, was able to make itſelf more room than before it could; and ſince that preſſure of the atmoſphere depends, for aught appears, upon its gravity, the ſame experiment may argue both the ſpring of the air and its weight.

And this may ſuffice for our third chapter, wherein having evinced againſt Mr. *Hobbes* our grand hypotheſis of the weight and ſpring of the air, I hope we have diſpatched the chief part of our work; ſince as for the particular explications we deduce from theſe hypotheſes, there are but very few, if any, that he endeavours to prove incongruous to them. Yet after we ſhall have (in the following chapter) conſidered upon what grounds he prefers his doctrine before ours, we ſhall (God permitting) in two or three other chapters gather up the things, that he objects againſt ſome particular opinions and explications by us delivered, and examine them.

CHAP. IV.

Wherein Mr. Hobbes'*s principal explications of the phænomena of the author's engine are examined.*

OF the hypotheſes, that Mr. *Hobbes* aſſumes, to explicate the phænomena of our engine, himſelf gives us a ſummary in this paſſage, (*pag.* 10.) *Intelléxti ergo hypotheſes meas?* 1. *Quod aëri interſperſæ ſunt particulæ multæ terreæ præditæ motu circulari ſimplice, naturæ congenito.* 2. *Quod major eſt quantitas earum particularum in aëre propè ad terram, quàm in aëre à terra remotiore:* 'Have you underſtood my hypotheſes? 1. That there are with the air interſperſed many earthy particles, endued with 'a ſimple circular motion, congenite to its nature. 2. That there is a greater 'quantity of theſe particles in the air that is near the earth, than in that which is 'more remote from it.'

Now here I might at the beginning take notice, that there are other things, which he takes for granted. As, firſt, *Non dari Vacuum*, which, as we have already ſeen he has not well evinced, nor I think eaſily will, upon the grounds he proceeds on. Next, that our common air is chiefly compoſed of an æthereal ſubſtance, which methinks he ſhould have proved; ſince for the moſt part the Vacuiſts (and ſuch he will needs have his adverſaries to be) admit not that pure air of his. Thirdly, that the air, at leaſt the pure air, is eaſily diviſible into parts always fluid and always air. Indeed he ſays of this aſſumption, *Nec ſuppono tantùm, ſed credo*; but neither to ſuppoſe nor to believe, is to prove. And what he adds, *Neque eſt qui hactenus ullam adduxit rationem, quare ita eſſe non poteſt:* 'Nor is there any one, that hitherto has brought 'any reaſon why it may not be ſo:' if it were true, would conclude little, ſince many things have not been, and perhaps cannot be, proved to be true; of whoſe not being poſſible no proof has been given. We might, I ſay, mention and examine theſe other aſſumptions of our author, but for brevity ſake we will conſider thoſe two lately recited from him.

Page 4.

Ibid.

AND as for the ſecond of them, bating the peculiar motion he is pleaſed to aſcribe to the earthy particles, I ſhall not contend with him about that hypotheſis; and therefore ſhall now only conſider the other. The *Motus circularis ſimplex* itſelf, which he imagines, in the ſun and the terreſtrial globe, I ſhall not need to examine, ſince Dr. *Ward* (a perſon whom, without diſparagement to a famous man, I may affirm to be at leaſt as eſteemed for aſtronomy as Mr. *Hobbes*) has expreſsly endeavoured to confute it, and that not without ſome deriſion (which yet I willingly forbear to imitate) by arguments, that I cannot learn Mr. *Hobbes* has yet anſwered. And I am informed, that the learned Dr. *Wallis*, and others, intend ſome animadverſions on this motion. But reſtraining our preſent conſideration to what this Dialogue ſuggeſts to me, this aſſumption to me ſeems very precarious, ſince I know not any unqueſtionable example or experiment, whereby it can be made out, that any ſmall parcel of matter has ſuch a *motus circularis ſimplex*, as he aſcribes to each of theſe innumerable earthy, and (as himſelf adds in the ſame page) aqueous particles. The only argument he brings in that page to prove, that each atom would have this motion, if all the reſt of the earth were annihilated, does not to me ſeem clear. For, not to mention, that it is ſtill by many learned men doubted, whether the terreſtrial globe itſelf have it; nor to examine, whether or no he aſſigns a good natural cauſe of it; it is not always true, that each minute part of an homogeneous body (which yet it will be hard to prove the terreſtrial globe to be) has in every reſpect the ſame qualities with the whole; as the roundneſs, which a ſmall drop of water or quickſilver is commonly obſerved to have when it leans upon a dry or greaſy plain, is not to be met with in great portions of either of thoſe liquors, though placed upon the ſame plain. And Mr. *Hobbes*, as well as we, makes the terrene atoms in the air to have gravity, which yet is a quality, that does not properly belong to the whole globe of the earth; nor is it manifeſt, why, becauſe the terreſtrial globe moves in a vaſt circle about the ſun, each particular atom of it muſt deſcribe a ſmall circle in the air about I know not what center. And ſince he teaches in his ſecond hypotheſis, and a few lines before it, that the air, near the earth, abounds with ſuch terrene corpuſcles, it is not likely they ſhould be permitted to exerciſe ſuch a regular motion as he attributes to them; but hitting againſt one another, they muſt in probability be put into almoſt as various and confuſed a motion, as *Des Cartes* aſcribes to his terreſtrial particles ſwimming in the atmoſphere. That which ſome will, I doubt not, peculiarly wonder at in Mr. *Hobbes*'s hypotheſis, is, that he makes this regular motion of each atom *naturæ ſuæ congenitus:* for philoſophers, that are known to wiſh very well to religion, and to have done it good ſervice, have been very ſhy of having recourſe, as he has, to creation, for the explaining of particular phænomena. And the Carteſians will think it at leaſt as allowable for them to ſuppoſe the motion he will not grant in their *materia ſubtilis*, as for Mr. *Hobbes* to aſſume it in his *particulæ terreæ*; eſpecially ſince he ſeems to make each ſuch atom put into and kept in a regular motion; whereas they aſſume but the having of one general impulſe given to the whole maſs of matter. Thoſe likewiſe, that fancy a ſpring properly ſo called in particular aërial corpuſcles, will hence perhaps take occaſion to think they may ſuppoſe an ingenite motion fit for their turn, as well as he an ingenite *motus circularis ſimplex*. How well likewiſe this hypotheſis will agree with his fundamental doctrine, that *Nihil movetur niſi à corpore contiguo & motu:* 'Nothing is moved but by a contiguous body that is in motion:' I leave to him to conſider. As alſo whether or no *Gaſſendus*, and thoſe other Atomiſts that admit creation, may not hence countenance their grand ſuppoſition of the congenite motion of atoms, which granted, would deſtroy the beſt part of Mr. *Hobbes*'s philoſophy. But whatever becomes of this *motus circularis ſimplex*, I need not be much ſollicitous, having formerly ſhewn, that the admiſſion of it would not diſprove what I have delivered concerning the ſpring of the air: and therefore leaving Mr. *Hobbes* to diſpute it out, if he think fit, with his other adverſaries, I will proceed to the main explications, wherein Mr. *Hobbes* endeavours to prefer his doctrine about the phænomena of our engine before ours. And theſe I find to be the four that enſue.

THE firſt and principal of theſe is that, wherein he ſtrives to prove, that by the exhauſtion of our cylinder no vacuum is produced, and to give of the experiment itſelf a very differing account from ours. This he does in the following paſſage; which, by reaſon of its importance in our preſent controverſy, we ſhall ſet down *verbatim: Dum ſuctor* (ſays he) *retrahitur, quanto relictus locus major fit, tanto minus* Page 5. *loci relinquitur aëri externo, qui retruſus à ſuctore moto verſus externa, proximum ſibi aërem ſimiliter movet, & hic alium, & ſic continuè, ita ut neceſſe ſit aërem tandem compelli in locum deſertum à ſuctore, & intrare inter ſuperficiem ſuctoris convexam & cylindri concavam: ſuppoſito enim aëris partes eſſe infinitè ſubtiles, impoſſibile eſt, ut viâ illâ, qua retrahitur ſuctor, illæ non ſe inſinuent. Primo enim, contactus ſuperficierum iſtarum per omnia puncta perfectus eſſe non poteſt, quia ipſæ ſuperficies fieri infinitè læves non poſſunt.*

Deinde vis illa, quæ ad ſuctorem revellendum adhibetur, cavitatem cylindri aliquantulum diſtendit. Poſtremo, ſi in confinio duarum dictarum ſuperficierum ingrediatur una tantum atomus dura, aër purus eâ viâ ingreditur conatu quantumvis debili. Poteram etiam computaſſe aërem illum, qui propter eandem cauſam inſinuaſſet ſe per cylindri valvulam. Sublatam ergo vides conſequentiam à retractione ſuctoris ad locum vacuum. Sequuturum hoc quoque eſt, aërem illum, qui eſt in locum à ſuctore deſertum impulſus, quia magnâ vi impulſus eſt, motu valde celeri & per circuitum inter ſummum & imum in cylindro moveri; cum nondum ſit, quod motum ejus poſſit debilitare: ſcis autem nihil eſſe, quod ſibi motum aut impertiri poſſit aut diminuere. ‘ While the ſucker is drawn back, by how much a greater ‘ place is left (within) by ſo much a leſſer place is left to the external air; which ‘ being thruſt backwards by the motion of the ſucker towards the outmoſt parts, doth ‘ move in like manner the air that is next itſelf, and that air the next, and ſo for‘ wards: ſo that it is of neceſſity at laſt, that the air muſt be compelled into the ſpace ‘ deſerted by the ſucker, and to enter between the convex and ſurface of the ſucker, ‘ and the concave of the cylinder. For it being ſuppoſed, that the parts of the air ‘ are infinitely ſubtile, it is impoſſible but they ſhould inſinuate themſelves that way, ‘ by which the ſucker is drawn down. For firſt, the contact of thoſe ſurfaces cannot ‘ be perfect in all points, becauſe the ſurfaces themſelves cannot be made infinitely ‘ ſmooth. Then, that force, which is applied to draw back the ſucker, doth di‘ ſtend in ſome meaſure the cavity of the cylinder. Laſtly, if in the confines of ‘ (that is, betwixt) the two ſurfaces, any one ſingle hard atom ſhould enter, pure air ‘ will enter at the ſame way, although with a weak endeavour. I might alſo have ‘ accounted that air, which for the ſame cauſe inſinuates itſelf through the valve of ‘ the cylinder. You ſee therefore the conſequence from the retraction of the ſucker, ‘ to the being of an empty place, is taken away. It will follow alſo, that the air, ‘ which is driven up into the place deſerted by the ſucker, becauſe it is driven up ‘ thither by a great force, is moved with a very ſwift and circular motion betwixt the ‘ top and the bottom in the cylinder, becauſe there is nothing there, that can weaken ‘ its motion: and you know, that there is nothing, that can give motion to its own‘ ſelf, or diminiſh it.’ But this ratiocination containeth divers things liable to exceptions: and in order to the examining of it I muſt premiſe, that I know not, why Mr. *Hobbes* ſhould here confine his diſcourſe to the pump, without taking notice of the glaſs, for whoſe evacuation it was deſigned. Wherefore, for eaſier conſideration ſake we will conſider, how this diſcourſe will account for the exhauſtion of the receiver, as well as for the cylinder; for we uſually empty them both in the ſame trials. And he being obliged to explicate the exhauſtion of the one as well as the other, it will be convenient to take into conſideration the receiver alſo; becauſe that being of glaſs and tranſparent, we can better ſee what happens in it than in the opacous cylinder. This premiſed, we may now proceed to the exceptions themſelves. And, firſt, I do not clearly ſee by this explication how he avoids a vacuum: for, according to his firſt words, the external air is diſplaced by the motion of the ſucker outward, and this diſplaced air muſt move that which is next to it, and that the next, and ſo onward (whether *in infinitum* or no, he declares not) ſo that at length *(tandem)* the air muſt be compelled into the place deſerted by the ſucker: ſo that till this returning air get in betwixt the ſucker and the cylinder, how appears it from this diſcourſe, that the deſerted ſpace was not empty for ſome little while? For, certainly, all theſe motions of the air forward and backward could not be performed in an inſtant; as may appear by the motion of ſounds and echoes, whoſe velocity is reducible to meaſure. Secondly, though he takes his adverſaries to be Vacuiſts, yet (to give an account of theſe phænomena)

phænomena) he ſuppoſes the plenitude of the world; as may appear both by expreſs paſſages in his dialogue, and by his here rendering no other probable cauſe of the air's getting into the room relinquiſhed by the ſucker. But, becauſe I have not here taken upon me the perſon of a Vacuiſt, I ſhall offer ſome other conſiderations. I wiſh then, thirdly, that Mr. *Hobbes* had declared from whence the regreſs of the air's impulſion ſhould begin; for that may well be required from one, that, making the world full, and for aught appears (the celeſtial globes excepted) fluid, allows us to believe it infinite, if the magiſtrate ſhall pleaſe to enjoin us that belief. Fourthly, I demand what neceſſity there is, there ſhould be ſuch a forcible return of the impulſe as is requiſite to thruſt in the air at ſo narrow a paſſage, as that between the ſucker and cylinder. For, why may not that impulſe, when diffuſed in the vaſt ambient medium, be ſo communicated and blended among the differing motions of the other parts of it, as not to return again from whence it begun? As we ſee that a voice, though ſtrong, will not move the air beyond a certain diſtance, ſmartly enough to be reflected in an echo to the ſpeaker: and a ſtone caſt into a lake will have the waves it makes diverted from returning to the place they began at. Fifthly, I do not likewiſe ſee, that it is proved, or probable, what Mr. *Hobbes* affirms of ſo thick a cylinder as ours, that it ſhould be diſtended by the depreſſing of the ſucker. But this I inſiſt not on; for the main thing, that is peculiar in Mr. *Hobbes*'s explication, is, that as much air as is driven away by the ſucker, gets preſently in again betwixt it and the cylinder: wherefore let us examine that a little. I ſay then, that by the air, which is ſo ſuppoſed to get in, he either means, in the uſual ſenſe and in ours, the common air, ſuch as we live and breathe in, or he does not.

If he do ſpeak of ſuch air, I can plainly prove by ſeveral experiments, that our engine is in great part devoid of it.

For firſt, if there be a contrivance made, whereby the whole pump may be covered with water, one may, as we have tried, plainly ſee the air, that is drawn out of the receiver, at each reciprocation of the ſucker, paſs in great bubbles out of the valve through the water.

Next, it appears by the *Magdeburg* experiment formerly mentioned, that by reaſon of the receſs of the air, the globe of glaſs, whence it went out, was diminiſhed in weight above an ounce. Thirdly, the ſame truth may be proved by the experiments formerly mentioned of the ſwelling of a bladder, and the breaking of an hermetically-ſealed glaſs upon the receſs of the ambient air; theſe experiments having been already vindicated from Mr. *Hobbes*'s very improbable explications of them. Fourthly, the ſame may be proved by the breaking of weak or ill-figured receivers inwards; of which in our hypotheſis the reaſon is clear, but not in Mr. *Hobbes*'s. But, fifthly, (not to multiply inſtances, though that were eaſy for me) what I contend for may be ſufficiently proved by this one phænomenon, that though, if the receiver being full of common air the key be turned under water, the water will not at all be ſpurted up at the open orifice: yet the like being done after the exhauſtion of the receiver, we have had divers gallons of water violently impelled into the cavity of the glaſs; which could not happen if it were full of air, both in regard there can be no probable cauſe aſſigned, why the water ſhould be thus ſpurted up; and becauſe the receiver being already full of air, either two bodies muſt be contained in one place, and ſo we muſt allow penetration of dimenſions; or elſe common air, to which glaſs is impervious, muſt paſs through the water, which we conclude it does not, becauſe no ſuch bubbles are made in the external water as would appear if common air paſſed through it. Nay, ſo little of this common air was ſometimes left in the globe uſed at *Magdeburg*,

burg, that when the water was ſuffered to ruſh in, it reduced the air into leſs by the beholder's eſtimate, than the thouſandth part of the capacity of the globe: and even if our receiver be unſtopped, not under water, but in the open air, the ambient air will violently preſs in, with a noiſe great and laſting enough to argue, that the glaſs was far from being full of ſuch air before.

AND thus we may argue againſt Mr. *Hobbes*, if he would have the engine, when we call it exhauſted, filled with common air, as his words in the recited paſſage (where he talks of the external air, and that impelled into the cylinder, without differencing them) ſeem to intimate. But becauſe by ſome other paſſages of this Dialogue he may be favourably thought to mean, that the pure air (as he ſpeaks) is that, which gets in by the ſides of the ſucker into the pump, and ſo into the receiver; let us conſider his explication in this ſenſe alſo. And not to urge, that it had not been amiſs, if, to avoid ambiguity, he had more clearly expreſſed himſelf, and named that other here, as well as he elſewhere calls it ſo; not to urge this, I ſay, I deſire it may be taken notice of, that if Mr. *Hobbes* take the air in this ſecond notion, he oppoſes not what I have delivered; the air I pretend to be pumped out of the receiver, being the common air, which conſiſts in great part of groſſer corpuſcles than the æthereal ſubſtance: and therefore I might ſafely paſs on to another ſubject. But I conſider further, that even this explication of Mr. *Hobbes*'s will be liable to the two firſt inconveniences lately objected againſt the other in favour of the Vacuiſts; and to divers of thoſe things beſides, that are objected in the following parts of that diſcourſe. Next I obſerve again, that though the pump be all the while kept under water, yet the exhauſtion of the cylinder and receiver will be made as well as in the open air. I demand then of Mr. *Hobbes*, how the pure air gets in by the ſides of the ſucker that is immerſed in water? I preſume, that for want of a more plauſible anſwer he will here ſay (as he elſewhere does in an almoſt parallel caſe) that the air paſſes through the body of the water to fill up that deſerted ſpace, that muſt otherwiſe be void: but then I appeal to any rational man, whether I am obliged to believe ſo unlikely a thing upon Mr. *Hobbes*'s bare affirmation. If I be, I muſt almoſt deſpair to prove things by experiments; and if he will allow me to expect from him as much as he ſeems to do from me, I ſhall ſcarce deſpair to maintain almoſt any hypotheſis I pleaſe: for, beſides that he does not ſo much as pretend by any phænomenon to countenance this bold aſſertion, there are phænomena, that make againſt it. For I know not how many experiments ſhew us, that when air paſſes through water, it makes bubbles there, which in our caſe do not appear. And beſides, I ſee not, why the outward air ſhould not rather impel the water (as we ſee it frequently does in ſuch caſes) than be ſuppoſed to dive ſo ſtrangely and unperceivedly through it. When alſo the diligently-exhauſted receiver is unſtopped under water, he that obſerves, how the water ruſhes in with a ſtream as big as the paſſage will give leave, will hardly imagine, that at the ſelf-ſame time, as much air, as there gets in water, can paſs through the ſame hole without being perceived. But it may by Pleniſts be ſaid in Mr. *Hobbes*'s behalf, and it ſeems the moſt that can be ſaid, that either his explication, or a vacuum muſt be admitted. To which I reply, firſt, that he has not evinced there can be no vacuum; having endeavoured to prove it but by a ſingle experiment, which at beſt does not more ſtrongly plead againſt a vacuum than this does for it. Next, that we have lately made it probable, that by his explication he does not avoid the neceſſity of a vacuum. And thirdly, that a Pleniſt, without having recourſe to Mr. *Hobbes*'s precarious *diving* of the air, may more probably decline the neceſſity of yielding a vacuum by ſaying, according to the principles of the Carteſians (the ſubtileſt and warieſt champions for a plenum I have yet met with) that the æther is, by the impulſe of the depreſſed ſucker, and the reſiſtance of the ambient bodies, ſqueezed in at the pores of the glaſs or cylinder into the cavity of the veſſel, as faſt as room is there made for it. And I confeſs, I ſomewhat wonder at Mr. *Hobbes*'s being averſe to this way of ſolving the objected difficulty, ſince (a little above the middle of that paſſage of his we have ſo long been examining) he ſuppoſes the parts of the air to be infinitely ſubtile; which if they are, I know not what pores can be too narrow for them to inſinuate themſelves into. But, to preſs this no farther, I muſt here take notice, that whether the cavity of the receiver be reſolved to be (totally or in part) empty or full of Mr. *Hobbes*'s æthereal body, or the Carteſians celeſtial matter; the violent ruſhing in of the water, when the veſſel is unſtopped under that liquor, and divers other phænomena, which will not be aſcribed to the ſubtile matter within (to which they attribute not any attraction) ſufficiently argue, that there is in the external air a far greater power of preſſing inwards, than there is within of reſiſting; and conſequently ſuch a weight or ſpring in that air as my epiſtle challenges to it. I had almoſt forgot to anſwer the laſt lines of Mr. *Hobbes*'s ſo often mentioned paſſage, where he would have the air, that he ſuppoſes to be impelled into the ſucker, to move very ſwiftly betwixt the top and bottom of it. And ſo elſewhere he would have the ſame air, when it gets into the cavity of the receiver. But having tired myſelf, as I fear I have you, by dwelling ſo long upon one paſſage; I will ſkip ſomewhat, that I here approve not in the ratiocination, and only ſay, that when a light bladder is ſuſpended in the cavity of the receiver, it betrays no ſuch motion as is here imagined; nay, the flame of a taper, as our epiſtle mentions, was not blown out, nor (for aught appeared) ſtirred by this ſuppoſed wind: to which I ſhall add, that ſmoke produced in the receiver, whilſt it remained exhauſted, was not by this vehement motion of the air blown about the receiver, as is particularly ſet down in our Appendix, promiſed by the tranſlator of the newly-mentioned treatiſe. But if you let in the common external air at the ſtop-cock, that indeed will ruſh in with noiſe and violence, and whirl about the bladder, that hung quietly enough before.

Having thus examined Mr. *Hobbes*'s firſt ſolemn explication, I now proceed to the next; wherein he undertakes to give an account by his hypotheſis of the reaſon, why in our engine, if the manubrium of the depreſt ſucker happens to ſlip out of the pumper's hand, the ſucker is carried up again towards the top of the cylinder. But ſince this explication is ſuch, that though he mentions it as his firſt, preſently after the recital of his two hypotheſes, he himſelf is pleaſed to confeſs in the laſt page of his book, that it is erroneous; I ſhall forbear to vex it, thinking ſuch acknowledgments more fit to be imitated, whenever there is the like occaſion, than to be diſcouraged. But as for the explication, which at the end of his Dialogues he ſubſtitutes for his retracted one, I confeſs to me it is ſo obſcure, that I know not well what to make of it. But, as far as upon conſideration I can underſtand it, it is coincident with that, which in our method will be called his fourth explication; with which, that it may the better agree, ſeems one of the chief reaſons of his altering it from what we had propoſed at firſt. Wherefore we ſhould preſently fall upon examining them both together; but that between them I meet with an explication (which in our reckoning is the third) of the Torricellian experiment.

And here he ſpends many words to prove the opinion he had, whether propoſed or adopted, in his Elements of Philoſophy; namely, that the place deſerted by the ſuſpended mercury is not empty, but full of air. But becauſe this expoſition aſſumes what he has not yet demonſtrated, *viz. non dari vacuum*, and becauſe the Torricellian experiment, as it is wont to be made, is none of the phænomena of our engine; I ſhall refer you to what thoſe learned men Dr. *Ward* and *Moranus* have profeſſedly, and the firſt of them largely enough, written againſt Mr. *Hobbes*'s Explication (yet without making all that either of them teaches mine:) becauſe, for my part, it will ſuffice me to argue, as I did before, that if he takes the air in the common ſenſe of the word (and that wherein his readers generally underſtand him) his conceit is manifeſtly repugnant to ſeveral ſuch phænomena as theſe. That if the experiment be very well made, we may, by inclining the tube, impel the mercury from its wonted ſtation to the top of the tube; which will not happen, in caſe the air were before inclination let into that deſerted ſpace. That if, when the mercury is ſettled at its wonted ſtation, the tube be lifted up out of the reſtagnant quickſilver, the outward air will drive up the heavy mercurial cylinder oftentimes with force enough to beat out the ſealed end. To which we ſhall add only this experiment: the quickſilver reſting at its wonted ſtation, if you carefully ſtop the lower orifice under the ſurface of the reſtagnant quickſilver, and then lifting up the tube (that which we uſed was about three or four foot long) into the air, keep it well ſtopped; if, I ſay, you firſt depreſs one end and then the other, you ſhall find the quickſilver fall againſt the depreſſed extreme of the tube with ſuch ſwiftneſs and force, as will perhaps ſurprize you, and make you apprehend, that the tube will be either beaten out of your hand or broken: whereas, if unſtopping the tube, whilſt the ſelf-ſame quantity of mercury remains in it, you let the outward air into the cavity unpoſſeſſed by the mercury, and then if you again ſtop the orifice with your finger, and proceed as formerly, you ſhall perceive the motion of the included liquor to be very much ſlower and leſs violent than formerly, by reaſon of the reſiſtance of the admitted air: which will alſo manifeſtly diſcloſe itſelf by the conflict and bubbles, that will be produced betwixt the air and quickſilver in their haſty paſſing by one another to the oppoſite end of the tube.

IF any friend of Mr. *Hobbes*'s, ſeeing the manifeſt inconveniences of this opinion, ſhall on his behalf pretend, that it is what he calls the pure air, that paſſes through the body of the quickſilver to the deſerted part of the glaſs-cane; the anſwer is ready, that Mr. *Hobbes*'s expreſſions look ſo much another way, that his readers (for aught I have found) do generally underſtand him of ſuch common air as is diſplaced by the deſcent of the mercury. And therefore I had reaſon to argue againſt what he wrote, as I have newly done; and however, this aſſertion is clearly precarious, and liable to the objections formerly alleged againſt the paſſing of the air through the water. To which we may add this circumſtance, that in our preſent caſe it muſt deſcend into a far heavier and cloſer liquor than water. But perhaps it will be thought, I have already ſaid more than needed againſt an opinion, which has been rejected as well by Pleniſts as Vacuiſts; and though mentioned as to the main by ſeveral writers, as well before Mr. *Hobbes* aſſerted it as afterwards, has been thought ſo unlikely, as not to have been (that I know of) approved by any man, even before the diſcovery of the phænomena of our engine. Which laſt words I add, becauſe that Mr. *Hobbes* not pretending, that any attraction intervenes in the caſe, I ſee not how he can poſſibly make out, to omit other phænomena, the deſcent of the mercury, in the tube further and further beneath its wonted ſtation, upon the exhauſtion of the receiver, and the reaſcenſion of the ſame mercury in the ſame tube, as we pleaſe to let in more or leſs of the outward air; without admitting as much of ſpring or preſſure in the air, as I need contend to have here allowed me. The weight of the terrene particles, by which, at the end of the third expoſition, he is reduced to endeavour the ſolution of the quickſilver's falling lower at the top than at the bottom of a hill (for I am willing to think, that is his meaning, and that it is by the tranſcriber's fault rather than his, that he reſolutely affirms the quite contrary) will by no means ſerve his turn; it being utterly improbable to imagine, that the air contained in ſo little a veſſel as one of our receivers can by its weight counterbalance ſo ponderous a cylinder of quickſilver: whence we may be allowed to argue that the air ſuſtains it by ſuch a preſſure or ſpring as we plead for, whether that proceed from the texture of the aërial particles, or from their motion, or from both.

The fourth and laſt of Mr. *Hobbes*'s principal expoſitions is of that experiment of ours, wherein 100 and odd pound weight being hung at the depreſſed ſucker, the ſucker was notwithſtanding impelled up again by the air to the top of the cylinder. Of this phænomenon (which has not hitherto proved unwelcome to the virtuoſi) Mr. *Hobbes* gives us the following account.

Hærent hic noſtri: (which, why he ſays, I know not) *quomodo hæc expedies tu?* Page 22.

A. Expedivi ante. Aër enim à retractione ſuctoris retrò pulſus, nec locum in mundo (ut ſupponimus pleno) quò ſe recipiat inveniens, niſi quem ipſe, corpora contigua ſuis locis pellens, ſibi faceret, perpetuâ pulſione in cylindrum tandem cogitur, tantâ velocitate inter cylindri concavam & ſuctoris convexam ſuperficiem, quanta reſpondere ſolet viribus illis magnis, quas ad ſuctorem revellendum neceſſarias experti eſtis. Aër autem ille, quâ velocitate ingreditur, eandem ingreſſus retinet, ſimulque latera cylindri ænei (vi elaſticâ præditi) undiquaque diſtinet. Conatar ergo aër in cylindro vehementer motus contra omnes partes ſuperficiei cylindri concavæ; fruſtra quidem dum ſuctor retrahitur: ſed quamprimum ſuctor manu emiſſus aërem impellere ceſſat, aër ille, qui ante incuſſus erat, propter conatum in omne punctum ſuperficiei cylindri internæ & vim aëris elaſticam, inſinuabit ſe inter eaſdem ſuperficies eâdem velocitate, quâ impulſus fuerat, id eſt, eâ velocitate, quæ reſpondet viribus impulſionis. Si ergo tanta ponderis vis ſuctor inappendatur, quanta manuum vis erat, quâ impellebatur, velocitas, quâ idem aër è cylindro exit, locum in mundo pleno nullum habens quò ſe recipiat, ſuctorem rurſus ad cylindri ſummitatem impellet, propter eandem cauſam quæ effecit, ut ſuctor paulo ante impulerit aërem.

‘ Here our men are at a ſtand: how will you expedite this difficulty?

‘ *A.* I have done it already: for the air being beaten back by the retraction of the
‘ ſucker, and finding no place in the world (which we ſuppoſe to be full) where it
‘ might diſpoſe itſelf, beſides that, which by driving out other bodies from their places
‘ it may make for itſelf, is by perpetual pulſion at length forced in the cylinder with
‘ ſo great ſwiftneſs, between the concave ſurface of the cylinder and the convex ſurface
‘ of the ſucker, as may anſwer that ſtore of power, which you found neceſſary
‘ to the drawing back of the ſucker. Now the air, with what ſwiftneſs it enters, retains
‘ the ſame within, and then diſtends every way the ſides of the braſs cylinder,
‘ which is *(of itſelf)* elaſtical. Therefore the air in the cylinder being vehemently
‘ moved, endeavours or thruſts againſt all parts of the concave ſurface of the cylinder;
‘ but in vain, until the ſucker is drawn back. But as ſoon as the ſucker,
‘ having ſlipt the hand, ceaſes to make its impulſe upon the air, that air, which was
‘ before driven in, by reaſon of its endeavour againſt every point of the internal
‘ ſuperficies of the cylinder and of the elaſtical force of the air, will inſinuate itſelf
‘ between the ſame ſurfaces with the ſame ſwiftneſs, as that by which it was impelled,
‘ that is, with that velocity, which anſwers the ſtrength of the impulſion. If therefore
‘ ſo great a power of weight be hung upon the ſucker, as may anſwer the power
‘ of the hands by which it was impulſed; the ſwiftneſs, with which the ſame air goes
‘ out of the cylinder, finding no place in the world (which is full) where to diſpoſe

'itſelf, will again impel the ſucker to the top of the cylinder, for the ſame reaſon, 'that the ſucker a little before made an impulſe upon the air.' Thus far our author's paſſage; againſt whoſe ſolution it is eaſy to draw divers arguments, from what we have diſcourſed againſt the firſt of his four explications. But though we refer you thither, yet we will here alſo obſerve, that this whole conceit of the air's running in and out with ſtrange velocity between the ſucker and the cylinder is precarious; nor does he propoſe any one phænomenon to countenance it. To which general advertiſement I ſhall add the three following particulars. Firſt, that in an engine, ſo contrived, that the pump lay covered with water, when the ſucker was retracted, the atmoſphere would ſtrongly preſs the water againſt it; and, if the manubrium were let go, would ſwiftly enough repel up the ſucker into the deſerted cavity of the cylinder. Which being a caſe parallel to that under conſideration, let any unbiaſſed perſon judge, how likely it is, that the air could perform all theſe excurſions without exciting bubbles, notwithſtanding the water's conſtant interpoſition betwixt it and the cylinder. Secondly, that there is as little probability in what our author teaches in thoſe words *conatur ergo aër*, &c. I might here repeat what we formerly mentioned of the breaking of our receivers inwards, not outwards; and I might add, that I ſee no reaſon, why the conatus of the included air, if its conatus were granted, ſhould be fruſtraneous, when the ſucker is depreſſed. But I will rather demand, why, if the air within have ſo ſtrong an endeavour outwards, as to ſtretch the thick ſides of the braſs cylinder, as Mr. *Hobbes* (with what probability, let any man judge) would have it; I demand, I ſay, why this air does not throw out the wooden peg or valve, which we have often to our trouble ſeen thrown out with great force and noiſe, when the depreſt ſucker being thruſt up again, while there was air in the cylinder, we forgot to leave the valve open; though in this caſe the air, that drove out the peg, was far enough from ſtretching the cylinder. And I further demand, how it comes to paſs, that, if having ſtopt the hole of the cylinder with your finger inſtead of the peg, you ſwiftly depreſs the ſucker, you ſhall be ſo far from feeling a preſſure outwards againſt the pulp of the finger from any thing contained in the cavity of the cylinder, that your finger will be ſtrongly, and perhaps not without ſome pain, preſſed in by the ambient air; in ſo much that it was this phænomenon, and one ſomewhat like it in the Torricellian experiment, that ſeem to have engaged my other adverſary, the learned *Linus*, to maintain a conceit quite contrary to Mr. *Hobbes*'s, and imagine in the deſerted cavity of the cylinder, not a diſtending, but violently contracting ſubſtance. Thirdly, that as to the laſt part of the paſſage under conſideration, beginning at *ſi ergo*, &c. which ſeems to me ſomewhat intricate; I do not ſo clearly underſtand, why the air, that is impelled in ſo ſwiftly betwixt the cylinder and the ſucker, ſhould not reſiſt the ſwift egreſs Mr. *Hobbes* aſcribes to the included air by the ſame paſſage: nor why this impelled air, that has ſo ſtrong an endeavour outward, ſhould never depreſs the ſucker (againſt whoſe upper part it muſt bear as well as againſt the cylinder) as well as the ſame air, diffuſing its motion through the vaſt ambient medium, can enable the external air to thruſt up the ſucker again; eſpecially, ſince during ſuch a depreſſion of the ſucker (as we have mentioned not to happen) made by the rebound of the air, forcibly impelled in from the cloſe bottom of the cylinder, the air from without may all the while, with congruity enough to Mr. *Hobbes*'s principles, get in between the ſaid ſucker and the cylinder. But not to inſiſt upon theſe niceties; I ſay, that the lifting up of the ſucker either is not neceſſary to prevent a vacuum; or that in ſome caſes it will be hard to ſhew, how a vacuum can by Mr. *Hobbes* be avoided. For when the depreſt ſucker is ready to be thruſt up again, if you hang a ſomewhat greater weight at it than an hundred and odd pounds, it ſhall not be lifted up at all. And I ſhall make this further improvement of the experiment under conſideration, that whereas this progreſs and regreſſion of the impulſe of the external air cannot reaſonably be ſuppoſed to be very laſting, you may, by a competent weight, detain the ſucker depreſt, till the ambient air is as quiet as it uſes to be; and yet if then you take off the overplus of weight, and perhaps a little more, in caſe the pump have been very ſtanch, the ſucker and the great weight appended will, notwithſtanding, be carried up; which it is no way likely it could be by the impulſe of the outward air, which had time to decay and be confounded. And as for the inward air, beſide that we have proved, that it has no ſuch conatus outwards as Mr. *Hobbes* pretends, why ſhould not that, were it granted, throw out the ſucker rather than cauſe it to be impelled inward; it being no way likely, that in caſe ſome air ſhould get out of the cavity of the cylinder, it could ſo move the outward air, as that the reflex of that impulſe ſhould make that free outward air bear more ſtrongly againſt the outſide of the ſucker than the inſide of the ſame ſucker is preſt againſt by the included air, whoſe *impetus* is incomparably leſs diffuſed? But not to be thought a more nice and diligent opponent than the matter requires, I ſhall drive this diſcourſe no further, but rather deſire it may be obſerved in general, that whatever be reſolved to be in the cavity of the cylinder when the ſucker is depreſt, yet ſince it is manifeſt, that it is at leaſt, in great part, devoid of common air; and ſince the ſucker, with the appended weight may, if the inſtrument leak not, be impelled up, when, in all probability, thoſe forced undulations of the air, that may be ſuppoſed to have been made by the ſucker, have ceaſed; the Carteſians, Mr. *Hobbes*, and thoſe others, that will not have recourſe to the unintelligible attraction of ſome rarefied ſubſtance within, muſt aſcribe ſo ſtrange a phænomenon to the preſſure of the air without. But I ſhall no further preſs this fourth objection, partly becauſe it is added to the other three, only *ex abundanti*, and partly becauſe this chapter is grown ſo long already.

I know indeed that after the expoſition laſt recited out of Mr. *Hobbes*, he makes the Academian Dialogiſt confeſs, that the reſt of the phænomena of our engine may alſo not uneaſily be reduced to his principles. But perhaps they, that take notice of the variety of thoſe phænomena we have ſet down in our treatiſe, will ſcarce be of his mind; and thoſe, that have conſidered what has been diſcourſed in this chapter againſt his four principal explications, and what I am about to ſubjoin in the following part of this treatiſe, concerning divers other ſolutions that he gives, will perhaps be inclined to think, that others may be like theſe, without being therefore neceſſarily true.

CHAP. V.

In which divers ſcattered explications, and other paſſages in Mr. Hobbes's *Dialogue, are examined.*

I Proceed then to the fifth chapter, in which and the next I glean up and examine divers ſcattered paſſages, wherein he offers at ſomewhat, by way of argument, againſt ſome things we had delivered in our epiſtolical treatiſe: I ſay, what he offers by way of argument; for as to thoſe paſſages, that do but either praiſe himſelf or diſparage his adverſaries, I have almoſt as little leiſure as inclination to take notice of them, and do not much apprehend that the virtuoſi (eſpecially ſuch as know us both)

both) will think what I write the leſs rational for being civil; or will let me ſuffer, in their opinions, for neglecting to trouble them, in a philoſophical controverſy, with matters, that do but very little belong to it.

To ſkip then what Mr. *Hobbes* is pleaſed to ſay, in the firſt page of his Dialogue, concerning ſome diſputable diſcoveries about ſenſation, which he challenges to himſelf; and to paſs by divers other things in the ſecond or third following pages, which relate to him, or to the ſociety he writes againſt, rather than to the nature of the air; we ſhould begin with the opinion he thinks fit, in the fifth page, to impute to us, as if we diſtinguiſhed what is fluid from what is not ſo, only (for ſo his ratiocination imports) by the bigneſs of the parts of which a body conſiſts: but deſigning, in an Appendix to be ſubjoined to this diſcourſe, to examine what I find in this Dialogue diſperſed touching fluidity, I ſhall now only ſay, that he does very much miſtake and miſrepreſent my doctrine of fluidity; wherein I expreſsly teach, that the principal cauſe or condition of it is not the ſize, but the motion of the ſmall parts that compoſe the fluid body.

To take up then the particulars we are to examine, in the order (as far as conveniently may be) wherein I find them lie in the author's Dialogue, and paſſing by at preſent thoſe things, which either we have conſidered already, or are not to conſider in this place; the firſt particular, that offers itſelf to be taken notice of, is this paſſage at the bottom of the twelfth page:

Page 12. B. *In vas apertum infudimus aquam, in aqua fiſtulam ſtatuimus erectam, longam, exiliſſimam; obſervavimus autem aquam è vaſe ſubjecto in erectam fiſtulam aſcendiſſe.*

A. *Nec mirum; nam ſuperficiem aquæ, particulæ aëri interſperſæ aquæque contiguæ motu ſuo verberabant, ita ut aqua non potuit in fiſtulam non aſcendere, & ſenſibiliter quidem in fiſtulam valde anguſtam.*

'*B.* We poured water into an open veſſel; we placed in the water a long ſtreight, 'ſlender tube; and we obſerved, that the water did aſcend from the veſſel under'neath into the erected tube.

'*A.* No wonder: for the ſmall particles, that are interſperſed in the air near the 'water, did, by their motion, beat upon the ſurface of the water, ſo that the water 'muſt of neceſſity aſcend into the pipe, and that ſenſibly into a pipe, that was ſo 'exceeding ſlender.'

To this I ſay, that it is manifeſt, by what I write in my epiſtle, that I did not then take upon me, nor do I undertake in this place, to aſſign the true reaſon of the propoſed phænomenon. An attempt of this kind has been ſince addreſſed to me, which being ingenious, if not alſo true, may be conſulted. In the mean time I cannot but declare, that I am no way ſatisfied with Mr. *Hobbes*'s expoſition: for, to ſay nothing of the motion he aſcribes to the particles diſperſed through the air, he leaves the difficulty unſolved, ſince there being common air as well within the cavity of the ſlender pipes as without it, he neither ſhews, nor ſo much as offers at, a reaſon, why the preſſure of the air within ſhould not reſiſt the preſſure of the ſame kind of air without; as we ſee it does in greater pipes. And poſſibly he would have paſt by this particular, if he had not overlooked the advertiſement I gave towards the cloſe of the 35th experiment; that it would concern thoſe, who ſhould undertake to ſhew the cauſes of this phænomenon, to bethink themſelves alſo of a reaſon, why, if the experiment be tried with quickſilver inſtead of water, the ſurface of the liquor will, inſtead of being higher, be lower within the pipe than without it. Whereas if Mr. *Hobbes*'s explication be ſufficient, why ſhould not the contrary happen in quickſilver as well as in water?

Page 13, 14.

The next paſſage I have to conſider is in the 13th page, thus ſet down: *Siquis poſt impulſionem revulſionemque ſuctoris aliquoties repetitam, epiſtomium ſuperni orificii recipientis conetur extrahere, inveniet illud valde gravitare, tanquam ſi multarum librarum pondus ab eo penderet. Unde contingit hoc?*

A. *Ab aëris, qui eſt in recipiente fortiſſimo conatu circulari facto à violento ingreſſu aëris inter ſuperficiem ſuctoris convexam & cylindri concavam, generato per iteratam illam impulſionem revulſionemque ſuctoris, quam vos perperam vocatis exſuctionem aëris. Nam propter naturæ plenitudinem, epiſtomium extrahi non poteſt, quin aër qui eſt in recipiente (epiſtomio contiguus) una extrahendus ſit. Qui quidem aër, ſi quieſceret, facillimè epiſtomium ſequeretur: ſed dum velociſſimè circuit, ſatis difficulter ſequitur; id eſt, videtur eſſe valde gravis.*

B. *Veriſimile eſt: nam ut aër novus in recipiens paulatim admittitur, etiam apparentem illam gravitatem paulatim perdit.*

‘ If any one after the frequently-repeated impulſe and retraction of the ſucker,
‘ endeavour to draw out the ſtopple of the upper orifice of the receiver, he ſhall find
‘ it gravitates very much, as if a weight of many pounds hung upon it. Whence
‘ comes this?

‘ *A.* From a ſtrong circular endeavour of the air within the receiver, made by the
‘ violent ingreſs of the air between the convex ſurface of the ſucker and the concave
‘ of the cylinder, procured by the repeated impulſe and revulſion of the ſucker,
‘ which you improperly call the exſuction of the air. For by reaſon of the fulneſs
‘ of nature the ſtopple cannot be drawn out, but the air that is in the receiver (con-
‘ tiguous to the ſtopple) muſt be drawn out too: which air, if it were ſettled and at
‘ reſt, the ſtopple would eaſily be drawn out; but while that does moſt ſwiftly circu-
‘ late, it comes out very hardly, that is, it ſeems to be very heavy.

‘ *B.* Very likely: for as ſoon as freſh air is by degrees let into the receiver, it like-
‘ wiſe by degrees loſes this ſeeming gravity.’

But, I do not much fear, that this explication will keep the experiment from continuing to be thought, by ingenious men, a notable confirmation of our hypotheſis. For, to paſs by ſomething, that, though I am no way ſatisfied with, cannot well be examined in few words; I anſwer, firſt, that if there be ſuch a vehement circular endeavour, as he imagines, of the air in the receiver, by which motion he elſewhere teaches (as we have ſeen above) that the air ruſhes out with violence enough to make the atmoſphere lift up in our cylinder above an hundred pound weight; I ſee not, why it ſhould not rather throw out the ſtopple under conſideration, than hinder its extraction. And I ſee not why, when the external air is readmitted at the ſtop-cock into the exhauſted receiver, and thereupon there does ſenſibly follow for a little while a whirling about of the included air, the ſtopple, that juſt before ſeemed ſo much to reſiſt the being drawn out, ſhould ceaſe to make any ſuch reſiſtance. Nor do I ſee, how the plenitude of nature ſhould, as is here intimated, hinder the extraction of the ſtopple; for, according to the Pleniſts, the world and the receiver muſt be at all times equally full. And if the contiguous air muſt for Mr. *Hobbes*'s reaſon neceſſarily be extracted with the ſtopple in one caſe, I ſee not, why the like ſhould not happen in another. But ſince Mr. *Hobbes* is pleaſed to call us experimentarian philoſophers, let us ſhew, that ſuch explications as theſe of his need not make us aſhamed of the name. I ſay then, that it appears by our experiments, that there is no ſuch *fortiſſimus conatus circularis* in the exhauſted receiver as he pretends; but that there is indeed an endeavour of the ambient atmoſphere to preſs inwards the parts of the glaſs and cover, that are contiguous to it. For, as I have alſo noted already, a light bladder ſuſpended in the cavity of the receiver betrayed no ſuch motion as Mr. *Hobbes* here ſuppoſes. To which I ſhall now add, that neither were a pair of ſcales ſuſpended within the ſame cavity; nor was a long magnetical needle, that reſted upon the point of another needle, at all whirled about by this imaginary motion of the air. Beſides, if you leiſurely looſen the braſs ſtopple, ſo that it may be very near, but not contiguous, to the ſides of the ſocket, you ſhall manifeſtly perceive a ſtrong current of air to flow into the receiver at that paſſage: and more than once, when inſtead of that piece of braſs we ſtopt the hole in the cover with our cement, one might obſerve, ſometimes whilſt we were pumping, ſometimes after we had done pumping, that the outward air by degrees depreſſed the ſuperficies of the cement expoſed to it, and made it concave, and now and then would break through it, thruſting it inward with great violence and noiſe.

In the ſame page our author rectifies, after his way, another of our explications in Page 13. theſe words; *Vidimus item aquam demiſſam in recipiens poſt ſuctoris aliquot reciprocationes ita bullire, ac ſi ſuppoſito igne ferveſceret.*

A. *Id quoque accidit propter velocitatem aëris, ut dictum eſt, in recipiente circumeuntis; niſi forte aquam illam dum bullit calidam quoque eſſe deprehendatis. Nam ſi certi eſſemus illam caleſcere, alia cauſa phænomeni excogitanda eſſet.*

B. *Imo certi ſumus, quod non caleſcit ſenſibiliter.*

A. *Quid ergo tali aquæ motui conferre poſſe putas majorem vel minorem atmoſphæræ gravitatem?*

B. *Neque illum motum attribuunt, puto, atmoſphæræ.*

‘We ſaw alſo water, being let down into the receiver, after ſome returns of the motion of the ſucker, to bubble ſo as if it had boiled over a fire.

‘*A.* This likewiſe happens, as we ſpake, by reaſon of the ſwiftneſs of the circulating air; unleſs perhaps you find the water hot too whilſt it bubbles. For if we were ſure it was hot, we muſt find out ſome other cauſe of the phænomenon.

‘*B.* We are certain it is not ſenſibly hot.

‘*A.* In what therefore can the greater or leſſer motion of the atmoſphere promote ſuch a motion as this?

‘*B.* I ſuppoſe they do not attribute this motion to the atmoſphere.’

But, I confeſs, I ſee not, how the circular motion of the air within the receiver could in a phial with a long neck produce ſuch effects as in my epiſtle are recited; eſpecially I ſee not, how ſuch a wind paſſing along the ſurface of the water could raiſe ſo many and ſo ſtrangely big bubbles, which ſeemed many of them to riſe from the lower parts of the water, and ſwelled notably as they aſcended; and how ſuch a wind ſhould carry up the moſt part of the water through the long neck of the phial, and as it were ſpout into the receiver.

As for what he ſays about the gravity of the atmoſphere, it is plain enough, that my conjecture aſcribes the phænomenon to the taking off, not the gravity of the external air, but of the preſſure of the air within the receiver; though I ſee not why the removal of the weight of the atmoſphere, if it could be out of the engine effected, ſhould not have a like operation.

And (to anſwer Mr. *Hobbes*'s queſtion as it ſhould have been put) that, which I think the greater or leſſer preſſure of the air confers to this phænomenon is this; that whereas common experience ſhews us, that water by being heated is expanded, and has bubbles generated in it; and whereas our former experiments, eſpecially the 28th, have made it appear, that there is wont to be in water and other liquors aërial particles, which tend to expand themſelves, and do actually do ſo, in numerous bubbles, when the preſſure of the incumbent air is conſiderably leſſened: in the preſent phænomenon, that preſſure being, by the exhauſtion of the receiver, taken off, the aërial particles and agitated vapours, that abound in the hot water, are allowed to expand themſelves, as before they could not, and to make ſuch numerous and great bubbles, that thereby a good part of the water is carried out of the phial. So that I ſomewhat wonder, what makes Mr. *Hobbes* ſpeak, as if there were no ſenſible heat of the water under conſideration, ſince it is expreſsly ſaid, that it was put in hot; and if it were put in cold, could by no pumping be brought to the leaſt ſhew of efferveſcence. And as for his explication of the phænomenon, the experimentarian philoſophers need not the objection lately made againſt it; for I have already evinced by experiments, that there is in our exhauſted receivers no ſuch peculiar motion of the air as he aſcribes the phænomenon to; nay, when there is manifeſtly a whirling about the air in the glaſs upon the admiſſion of the external air, the production of numerous bubbles in the water preſently ceaſeth. And therefore I ſee not, why Mr. *Hobbes* might not have let alone my conjecture (for I propoſed it, and look upon it, as no more) unleſs he could either have diſproved it better, or ſubſtituted a more probable one than he has in its place.

As for what he adds in theſe words, *Ab hoc experimento manifeſtum eſt, quod recipiens per exuctionem hanc quam vocatis aëris, non ſit vacuum. Nam moveri aqua non potuit niſi à movente aliquo moto & contiguo. Itaque phænomenum hoc demonſtrationem ſuppoſitionis meæ continere videtur non infirmam:* ‘It is manifeſt from this experiment, that the receiver is not made empty by this exſuction of air, as you call it: for the water could not be moved but by ſome contiguous mover, that was itſelf in motion: therefore this phænomenon ſeems to contain no weak demonſtration of my hypotheſis:’ I am not obliged to anſwer it, but leave that to thoſe, that are profeſt Vacuiſts; againſt whom I muſt doubt whether his ratiocination will conclude, though the conſequence be not manifeſt to me. For himſelf allows his terreſtrial atoms an innate circular motion, which conſequently needs not depend upon ſome body contiguous and moved; and the Vacuiſts will ſay, that the particles of the water being ſtrongly agitated, when it was put into the receiver (whether by fiery corpuſcles ſwarming in it, or otherwiſe) and the reſiſtance of the incumbent air being taken off, the phænomenon would be produced juſt as it is, though we ſhould ſuppoſe no other body to ſucceed in the room of the exhauſted air. And beſides, though ſome ſubtile particles of active matter ſhould get in, to agitate the aqueous and aërial corpuſcles, yet (they may ſay) there is no neceſſity, that ſuch minute particles ſhould be numerous enough to fill up exactly all the little ſpaces deſerted by the air. And even upon this ſuppoſition, as it would not follow, that ſuch relinquiſhed ſpaces were all of them quite empty; ſo would not the phænomenon at all prove, much leſs manifeſtly prove, that they were quite full. And ſince an actual heat, that is, a briſk and various agitation of its ſmall parts, is requiſite to the boiling of the water in this experiment; perhaps others will not think it more abſurd, that the removal of the preſſure of the air ſhould occaſion this expanſive motion in the water, than that, which Mr. *Hobbes* muſt allow, that in air compreſſed by quickſilver, or ſome other weight, the removal of that preſſure is ſufficient to make that air expand itſelf by the flying abroad of its parts. Page 3.

AND whereas Mr. *Hobbes* urges this other argument againſt the Vacuiſts, *Præterea, dic mihi, bullientem aquam potuiſtin' conſpicere?* Page 13, 14.

B. *Quidni?*

A. *Nonne viſionem fieri concedunt veſtri per actionem continuam ab objecto ad oculum? Nonne etiam putant actionem eſſe motum, & omnem motum eſſe corporis? Quomodo ergo* *potuit ab objecto, nempe aqua, ad oculos tuos motus per vacuum (id eſt, per non corpus) derivare ?*

B. *Non affirmant noſtri ita vacuum eſſe recipiens, ut nullus omnino aër relictus ſit.*

A. *Nil refert an totum recipiens vacuum ſit, an magna ejus pars; nam utrumvis ſupponatur, derivatio motus ab objecto ad oculum intercipietur.*

B. *Ita videtur, nec habeo quod reſpondeam.*

‘ Beſides, tell me, could you ſee the water bubbling in that manner ?

‘ *B.* What elſe ?

‘ *A.* Do not your aſſociates grant, that viſion is made by a continued action from the object unto the eye ? Do they not alſo think action to be motion, and all motion to be of ſome body ? How therefore could the motion be derived from the object, the water, unto your eyes through a vacuum, that is ſomewhat, that is not a body ?

‘ *B.* Our friends do not affirm the recipient to be ſo empty, that no air at all is left.

‘ *A.* No matter, whether the receiver be wholly, or for the greater part empty; for which ever you ſuppoſe, the derivation of the motion from the object to the eye will be intercepted.

‘ *B.* It may be ſo; I cannot tell what to anſwer.’

The Vacuiſts will perhaps anſwer him, as I anſwered *Franciſcus Linus* to an almoſt like objection. And thoſe of them (which make far the greateſt number) who plead but for an interſperſed vacuum, will perhaps tell him, that they take viſion to be made, not by ſuch a propagation of impulſe as he does, but by a trajection of effluvia, that iſſuing out of the ſun, and traverſing the diaphanous bodies interpoſed, rebound from the object to the eye. And according to this doctrine they may aſk Mr. *Hobbes*, why a motion may not be made through a vacuum or *non corpus ?* Nay, how it can naturally be ſtopt *in vacuo*, where there is nothing to reſiſt it? But controverſies of this nature it lies not upon me to proſecute.

Page 14.

IN the 14th page, Mr. *Hobbes* having recited that experiment of ours, of killing animals included in our receiver by the exhauſtion of the air in two or three minutes of an hour, ſubjoins theſe words; *Credin' tu animalia iſta tam cito interempta eſſe eo quod carerent aëre? Quomodo ergo ſub aquam vivunt urinatores, quorum aliqui (aſſueti à pueritiâ) caruere aëre per horam integram? Incluſa in recipiente animalia occidit motus ille idem vehementiſſimus, quo diſtenduntur rumpunturque incluſæ veſicæ.* ‘ Do you think theſe animals were therefore ſo quickly killed, becauſe they wanted air ? How then do they, who make a trade of diving, live under water, of whom there be ſome, who being accuſtomed from their childhood, have wanted air a whole hour ? No, that moſt vehement motion, by which bladders ſhut therein are diſtended and broken, kills theſe animals ſhut up in the receiver.’ But, though he ſays no more in this place concerning this matter, yet it ſeems he either much liked his own conjectures, or greatly diſliked mine, ſince in his epiſtle dedicatory to the learned *Sorberius* he ſingles out this ſole phænomenon to explicate; *Ego contra* (ſays he there) *neque aërem exugi poſſe, neque incluſum animal (etſi exuctus eſſet) tam cito moriturum eſſe exiſtimo. Actio quidem, quam mors illa ſequitur, videri poteſt vel ſuctio quædam & propterea exſuctione concluſi aëris interfici animal, reſpiratione ſublatâ) vel etiam compulſio aëris ab omni parte verſus centrum ſphæræ, cui animal includitur; & ſic videri poteſt mori à tenacitate compreſſi aëris, quaſi aqua ſuffocatum; nimirum hauſtum in intima pulmonum aërem ſolito tenaciorem, inter arteriam & venam pulmonis, curſum ſanguinis intercipiendo ſiſtere.* ‘ I, on the contrary, think, that neither the air can be ſucked out, nor that the animal would ſo ſoon die, if it were ſucked out. The action indeed, to which this death

‘ is a conſequent, may ſeem either a certain ſuction (and ſo, that the animal is killed by the exſuction of the included air, its reſpiration being taken away) or a compulſion of the air from all parts towards the center of the ſpherical glaſs, in which the animal is incloſed, and ſo may be ſeen to die ſtifled by the tenacity of the compreſſed air, as it were, with water; the air more tenacious than uſual being drawn into the inwards of the lungs, and there, between the pulmonary artery and vein, ſtopping the courſe of the blood.’ But, as I propoſed by conjecture doubtingly, and profeſs myſelf to be in a further inquiry about the uſe of the air and of reſpiration; ſo I muſt ſtill think, that we want ſome further or clearer diſcovery about that matter, notwithſtanding what has been delivered concerning it by Mr. *Hobbes*. For his argument againſt my conjecture is, in the paſſage that propoſes it, anſwered by himſelf: for he plainly intimates, that divers, who can live without air (which yet I might queſtion, if he means without any air at all) for a whole hour, are accuſtomed to it from their childhood. Wherefore, unleſs the animals, that died in my engine, had been for a long time framed by degrees to live without air, it will not follow, that the want of it could not diſpatch them in a ſhort time, as ordinary men may be drowned in a few minutes. And having purpoſely let down ſome mice and ſmall birds into a deep glaſs filled with water, and kept them from emerging by a weight tied to their legs or tails, though ſome lived longer than others, yet I obſerved them to be killed faſt enough to keep my conjecture from being incredible: eſpecially the laſt we made trial of, though a large and luſty mouſe, appeared to be quite dead within ſomewhat leſs than one minute, meaſured by the vibrations of a pendulum. And we particularly took notice, that, before drowning, divers bubbles, which ſeemed to be the reſpired air, came out of their mouths, and aſcended through the water.

AND as for the explication, that Mr. *Hobbes* would recommend inſtead of mine, not to urge, that I could wiſh he had been pleaſed to ſhew us, how the tenacious air he imagines to be inſpired comes to produce thoſe ſtrange convulſions, and other ſymptoms mentioned in my epiſtle; not to urge this, I ſay, we have already diſproved the ſuppoſition his opinion is built on, namely, that there is in the exhauſted receiver ſuch a *motus vehementiſſimus*, as he pretends: beſides that he ſhews not how this motion comes to kill the included animals, which I was wont to keep, not near the center of the receiver, where he ſeems to think this motion moſt operative, but near the bottom of it, that the included animal might have ſomething firm under his feet. Nor does it at all clear the difficulty, that he would have this motion the ſame, whereby included bladders are diſtended and broken. For, beſides that it is very hard to conceive, how the tenacity of the air, or its beating from all parts upon the convex ſurface of an almoſt quite empty bladder (for in ſuch alſo the experiment he refers to will ſucceed) ſhould make it burſt outwards; beſides this (I ſay) we have already proved, that the diſtenſion, and breaking of bladders in our receiver, proceeds not from any ſuch motion of the neighbouring air as is here preſumed, but from a quite differing, if not from a contrary cauſe.

IN the ſame page our author makes a digreſſion from the engine, and diſcourſes of another experiment, which I have long ſince often made. But though his explication be liable enough to juſt exceptions (as I can make good, if it be required) yet becauſe the experiment is none of thoſe I delivered, I ſhall leave it to be examined by others. And for the ſame reaſon I forbear to meddle with that he has in the next page concerning the wind-gun. As to what he has in the ſame 16th page, in theſe words; — *Placet mihi tua magis hypotheſis quam illa de vi aëris elaſtica: nam video, quod à veritate* Page 16.

illius veritas dependet vel vacui vel pleni; ſed à veritate hujus nihil ſequitur in neutram partem quæſtionis. Aëris, inquit, ſtructura ſimilis eſt compreſſæ lanæ. Bene eſt. Lana fit ex filis. Recte. Sed cujus figuræ? Si parallelopipedi, nulla poteſt eſſe compreſſio partium: ſi non parallelopipedi, erunt inter fila illa ſpatia quædam relicta; quæ, ſi vacua ſunt, ſupponunt vacuum, ad probandum, quod vacuum eſt poſſibile; ſi plena, plenum dicunt quod vacuum putant: ‘Your hypotheſis pleaſes me better than that of the ſpring of the air: for from its truth depends the truth of a vacuum or a plenum; but from the truth of that, nothing follows on either part of the queſtion. The make of the air (ſays he) is like that of compreſſed wool. Well; wool is made of hairs or threads. Right; but of what figure? If of a parallelopipedon, there can be no compreſſion of parts; if not of a parallelopipedon, there will be betwixt the hairs certain ſpaces left, which if they be empty, they ſuppoſe ſome place empty, to prove that a vacuum is poſſible; if full, they ſay that is full, which they ſuppoſe to be empty:’ To this paſſage, I ſay, I cannot but repreſent, that the queſtion is not, whether from the hypotheſis, that aſcribes a ſpring to the air, depends the proof of a vacuum or a plenum, but whether the hypotheſis itſelf be true or no. For, ſure there are many things certain in natural philoſophy, from whoſe truth that of a plenum or a vacuum cannot be deduced. And to what he adds concerning the ſtructure of the aërial particles, the Vacuiſts may tell him, that they make no ſuch argument as he is pleaſed to make for them; and do not commonly employ the figure of the aërial particles to prove a vacuum, but other arguments, ſuch as Mr. *Hobbes* has not yet well anſwered: and having by them, as they judge, proved interſperſed vacuities, they might without inconvenience ſuppoſe in an aërial corpuſcle little empty pores, upon whoſe account it may be capable of compreſſion, in caſe they ſhould think fit (which I know not that any of them does) to aſſign it the figure of a parallelopipedon. But this controverſy the Vacuiſts may, if they pleaſe, proſecute. In the ſame page Mr. *Hobbes* begins, and in the next he continues, a long diſcourſe concerning the going out of fire in our receiver upon the exhauſtion of the air: the paſſage is too prolix, and does too little concern the ſpring of the air to be here totally tranſcribed, or examined period by period. In ſum, he endeavours to do two things: the one is to reduce what happens to kindled coals placed in our engine to what happens in certain mines, wherein when ſome thick damps aſcend, both charcoals and candles are ſoon extinguiſhed thereby: the other is to ſhew, that by the reciprocation of the ſucker, the air impelled firſt into the cylinder, and then into the receiver, is put into ſuch a motion, as gives it a certain middle conſiſtence, as he ſpeaks, betwixt the conſiſtence of pure air and that of water. But I ſhall not need to examine this ſecond part of his diſcourſe, becauſe I deny the firſt; and being able to diſprove the thing itſelf, namely, the thickneſs of the air in the exhauſted receiver, I need not ſpend time about what he teaches *de modo*.

To examine then only the firſt of the abovementioned particulars, I ſhall begin with obſerving, that his ſtory of the damp to be met with in mines is more largely ſet down by Mr. *Hobbes* in that chapter of his Elements of Philoſophy, where he treats of gravity; in which place he ſeems to mention it (to uſe his own expreſſion) as a ſtory of doubtful credit, which it is not like he would have done, if he had then ſeen it. Which I mention, not that I deny the ſtory for the main, nor that I would bring Mr. *Hobbes* into a ſuſpicion of relating things untrue as matters of fact, his enemies themſelves having not accuſed him of ſuch a meanneſs; but becauſe, if he have not ſince obſerved the thing himſelf, there may eaſily be a miſtake in ſome of the circumſtances; as for inſtance, the number of minutes, wherein the thick air choaks the fire: and it is upon that circumſtance, that the validity of what he deduces from the obſervation chiefly depends. But, however the matter fare with theſe ſubterraneal damps, we have already proved by ſeveral of the experiments of our engine, that in the exhauſted receiver there is no ſuch motion of the air as is here ſuppoſed. And it may be ſufficiently proved, that whatever remains in the receiver is not ſuch a ſubſtance as Mr. *Hobbes* would have it; for that, he here tells us, is of a conſiſtence betwixt air and water; and in the above-cited place of his Elements he ſays, that it is not much lighter than water. But by the Magdeburgick experiment (we have already had occaſion to mention two or three times) it is evident, that the receiver, by being exhauſted of common air, is ſo far from growing heavier, much leſs ſo much heavier as it muſt, if it were filled with a ſubſtance not much lighter than water, that it loſt above an ounce of its former weight. And to this agrees what we ſee happen in æolipiles, that grow lighter, when the air is expelled. Beſides, if the receiver be in our preſent caſe filled with a ſubſtance, whoſe conſiſtence is ſo much nearer that of water than is our common air, as Mr. *Hobbes* would have it; how chance a pendulum ſhould not move very ſenſibly ſlower in it, when in water the diadromes are ſo exceedingly much more ſlow? And the breaking of an hermetically-ſealed bubble in our receiver outwards, when the air was much exhauſted, and not before, together with divers other experiments that might be eaſily applied to this purpoſe, in our epiſtle, do ſufficiently evince, that it is not a thicker and far heavier air, but a more yielding and lighter, that remains after pumping in the cavity of our receiver.

AND thus much as to Mr. *Hobbes*'s diſcourſe upon our experiment. But as for the thing itſelf, it appears, that when I related it, I thought it might admit a further inquiry: and indeed there may be ſo many ways of extinguiſhing fire (as we ſee that the flame of a candle may be blown out by the wind, or quenched in water, or put out by the compreſſion of a pair of ſnuffers, or ſuffocated for want of air to receive its fuliginous ſteams, or (if that be a different way) ſtifled by the thick exhalations of deep mines or of new wine) that as it is not in all caſes ſo eaſy to aſſign the true cauſe of the extinction of fire; ſo it is unſafe to conclude with Mr. *Hobbes*, that becauſe a candle or a live-coal may, a great way under ground, be extinguiſhed by a thick damp, therefore the effect muſt proceed from the like cauſe in our receiver, where there is no ſign of any damp or unuſual thickneſs of the air, but of the contrary.

BUT let us follow Mr. *Hobbes* to the next paſſage, wherein he ſeems fond enough of playing the cenſor. For, ſpeaking of our 11th experiment, wherein the coals, that ſeemed almoſt dead in our exhauſted receiver, being taken out into the air began to ſhine again, having made his academian dialogiſt ſay, *Fuere eorum aliqui, qui remanſiſſe* Page 18. *dixerunt in carbonibus illis (quanquam extincti videbantur) particulas quaſdam igneas, quæ, admiſſo aëre, ventilatæ cæteram molem denuo accenderent:* 'There were 'ſome of them, that ſaid there remained in thoſe coals (though they ſeemed extinguiſhed) ſome fiery particles, which, being blowed up by the air, upon its admiſſion 'did rekindle the reſt of the maſs.' The other (by whom Mr. *Hobbes* is meant) anſwers him, *Næ illi quæ dicerent non videntur cogitaſſe, ſed ſortitos eſſe.* 'In good Page 18. 'faith they ſeem not ſo much to have conſidered what they ſhould ſpeak, as to have 'taken it up at all adventures.' This is very ſevere. But let us ſee what weighty reaſon he has to be ſo: *Credin' tu in carbone ignito partem aliquam non carbonem, ſed ignem eſſe; aut in caudente ferro partem ineſſe, quod ferrum non ſit, ſed ignis?* 'Do you 'believe, that in a kindled coal, there is any part, which is not a coal, but fire; or 'in a red-hot iron, there is any part, that is not iron, but fire?' But ſome will think, that by theſe words, he does rather propoſe than prove his opinion: wherefore let us

hear

Page 18. hear his ratiocination, which he annexes in the following words: *Ab unica ſcintilla magnæ urbis incendium naſci poteſt. Atqui ſi ignis corpus ab ignito diverſum ſit, non plures potuere eſſe partes igneæ in toto incendio, quam in una illa ſcintillula. Videmus corpora diverſorum generum à luce ſolis tam per refractionem quam reflectionem factam in ſpeculis comburentibus accendi poſſe: neque tamen quenquam eſſe credo, qui putet particulas igneas à ſole ejectas tranſire poſſe per ſubſtantiam globuli cryſtallini. In aëre intermedio ignis nullus eſt.* 'A great city may be ſet on fire by one ſpark: now, if the body of fire be different 'from the thing fired, there can be no more parts of fire in the whole town on fire, 'than that one ſpark. We ſee bodies of divers kinds may be ſet on fire by the light 'of the ſun, as well by the refraction as the reflection, that is made in burning-'glaſſes. And yet I do not believe, that there is any man thinks, that particles of 'fire darted from the ſun can paſs through the ſubſtance of a cryſtal globe. And in 'the air, between the ſun and the globe there is no fire.' But I doubt his adverſaries will ſay, that he is ſo far from having in this paſſage well confuted their opinions, that he ſeems not to have well underſtood them: for they will tell him, that they teach not, that the fire is materially different from every part of the fewel; but that the igneous corpuſcles, whilſt they are divided, blended and oppreſt with the others, have not the power to ſhine or burn, till being by ſome ſpark or other body actually burning, or by ſome other equivalent cauſe extricated, they flock together in ſwarms, and then are able to burn and ſhine, that is, to appear fire: which fire is yet but a part of the fewel; as appears by the phlegm, aſhes, and perhaps other incombuſtible parts of the coal or other fewel. So that the Atomiſts and divers others will not allow, what Mr. *Hobbes* infers about an *incendium.* And whereas he tells us he believes, that no body thinks *particulas igneas à ſole ejectas tranſire poſſe per ſubſtantiam globuli cryſtallini*; he ſeems to me to have very little heeded the Epicurean hypotheſis. For not only the learned *Gaſſendus*, but I know not how many other Atomiſts (beſides other Naturaliſts) ancient and modern, expreſsly teach the ſun-beams to conſiſt of fiery corpuſcles, trajected through the air, and capable of paſſing through glaſs; whereby theſe authors give an account of thoſe *ſpecula uſtoria*, that burn by reflexion. Theſe things I repreſent, not that I intend here to adopt the Atomiſts opinion of the nature of fire, of which I am not obliged to declare my thoughts here, and have done it elſewhere; but to ſhew, that Mr. *Hobbes*'s arguments are not a ſufficient ground for ſo heavy a cenſure. And if a coal be kindled at one end, though Mr. *Hobbes* would have the kindled end a coal, not fire; yet if he pleaſe to hold it in his naked hand, he may find that differs enough from the other end to deſerve another name. And I, that but related a phænomenon, did not perhaps expreſs myſelf much leſs warily, if not more ſo, than Mr. *Hobbes* himſelf. For whereas my words are theſe, Page 32. *We preſently took out the coals, in which it ſeems there had remained ſome little parcels of fire, rather covered than totally quenched; for in the open air the coals began to be rekindled in ſeveral places:* Mr. *Hobbes* even in his elements of philoſophy ſpeaks thus Chap. 30. ſect. 14. upon a ſomewhat like occaſion; *If a grate filled with coals thoroughly kindled, and burning never ſo brightly, be let down, as ſoon as ever it is below C. the fire will begin to grow pale, and ſhortly after (loſing its light) be extinguiſhed, no otherwiſe than if it were quenched in water: but if the grate be drawn up again preſently, while the coals are ſtill very hot, the fire will by little and little be kindled again, and ſhine as before.*

Page 18. As for the reaſon Mr. *Hobbes* aſſigns of our experiment in the lately mentioned paſſage of his dialogue, being grounded upon ſuch a thickneſs of the air in the receiver as we have already diſproved, it needs not to be examined. And laſtly, as to Page 18. what he ſubjoins in theſe words, *Quando autem eſt, quod de homine vere pronunciare* *poſſumus quod eſt mortuus, ſive (quod idem eſt) animam expiravit. Cognitum enim eſt homines nonnullos pro mortuis habitos poſtridie elatos revixiſſe.*

A. *De puncto temporis, quo anima à corpore ſeparatur, difficile eſt ſtatuere. Perge igitur ad experimenta alia.*

‘ When is it, that we may truly ſay of a man that he is dead, or (which is the
‘ ſame) hath expired his ſoul? For it has been known, that ſome men, who have
‘ been taken for dead, being brought out the next day, revived?

‘ *A.* It is hard to determine the point of time, in which the ſoul is ſeparated from
‘ the body. Proceed therefore to other experiments.’

I confeſs I ſee not why that needleſs queſtion might not have been well ſpared, if he deſigned to give it no better anſwer.

CHAP. VI.

Wherein other paſſages of Mr. Hobbes*'s Dialogue, that concern the author, are examined.*

WHAT our author has in the 19th page concerning a bladder, has been already examined: wherefore I proceed to the next paſſage in the ſame page, which is this:

B. Si acus magnete excitus libere pendeat intra recipiens, ſequetur tamen ille motum ferri quod circumducitur extra recipiens. Item objecta intus poſita ab iis qui extra ſunt videbuntur, & ſoni intus facti audientur, omnia hæc æque poſt atque ante exuctionem aëris, niſi quod ſoni ſunt aliquanto poſt quàm ante debiliores.

A. Manifeſtiſſima hæc ſunt ſigna recipientis ſemper pleni, nec poſſe inde exugi aërem. Quod autem ſoni inde ſentiantur debiliores, ſignum eſt conſiſtentiæ aëris. Conſiſtentia autem aëris à motu ejus eſt per lineas diametraliter oppoſitas.

‘ *B.* If a needle excited by a loadſtone hang freely within the receiver, it will ne-
‘ vertheleſs follow the motion of the iron, which is drawn about without the receiver.
‘ So objects put within will be ſeen by thoſe that are without, and ſounds made within
‘ will be heard without: all theſe as well after as before the exſuction of the air, ex-
‘ cept that the ſounds are ſomewhat more weakly heard after than before.

‘ *A.* Theſe are moſt manifeſt ſigns, that the receiver is always full, and that the
‘ air cannot thence be ſucked out. That the ſounds thence are more weak to one's
‘ hearing is a ſign of the conſiſtence of the air; for the conſiſtence of the air is dia-
‘ metrically oppoſite from its motion.’ But I meet with few of the Vacuiſts, who, even in the Torricellian experiment, think the place relinquiſhed by the quickſilver to be perfectly void, moſt of them allowing, that though it be not quite full of body, yet it may contain ſome of the earth's magnetical ſteams, or of thoſe igneous corpuſcles that flow from the ſun, or both of them. Now againſt theſe, who would from our experiments deduce but only an interſperſed vacuum, I ſee not, that the phænomena mentioned by Mr. *Hobbes* do conclude half ſo manifeſtly as he pretends: for, as to the motion of the needle within the receiver, it is known, that they are wont to aſcribe magnetical attraction to certain effluvia, that iſſuing out of the loadſtone are ſubtile enough to paſs through the pores of the cloſeſt bodies without excepting glaſs; ſo that although the receiver were quite emptied before, the needle might be wrought upon by magnetical corpuſcles, that need not be ſuppoſed to fill the tenth part of the receiver. I know indeed, that Mr. *Hobbes* has another hypotheſis of the phænomena of the loadſtone; but I know, that divers learned writers have

have abſolutely rejected it, and not one ſuch, that I have heard of, has approved it. And as for the other two phænomena here mentioned by Mr. *Hobbes*, the Atomiſts may anſwer, that the firſt (touching objects ſeen in the receiver) has been ſhewn already not to overthrow their doctrine; and that the other (concerning the debilitation of ſounds) makes againſt him, not for him; ſince we have already diſproved, that conſiſtence of the air, whereto he aſcribes it. And the ſame arguments, that overthrow that opinion, may make it ſeem ſomewhat ſtrange, that he ſhould ſubjoin our experiment of two like pendula, whoſe vibrations we found not manifeſtly to differ within and without our exhauſted receiver. For the former ſhould move far ſlower than the other, according to Mr. *Hobbes*'s conceit, that the receiver, when we ſay it is exhauſted, is filled with a ſubſtance of a middle conſiſtence betwixt pure air and water, and not much lighter than water *. But whether the receiver be in ſuch caſes adequately full or no, the Vacuiſts may further conſider. For its being granted to be full would not overthrow either of my hypotheſis, namely, the weight and ſpring of the air.

IN the ſame 19th, and ſome following pages, Mr. *Hobbes* has a long diſcourſe againſt my conjecture at the reaſon I propoſe in my 31ſt experiment, why (as I here expreſs it) if the exquiſitely poliſhed ſurfaces of two flat pieces of marble be ſo congruous to each other, that upon their mutual application there reſults an immediate contact, they will ſtick ſo faſt together, that he, that lifts up the uppermoſt, ſhall, if the undermoſt be not exceeding heavy, lift up that too, and ſuſtain it aloft in the Page 6. free air. The conjecture itſelf is in the ſame page thus ſet down; ‘ That the lower ‘ ſuperficies of that (undermoſt) ſtone being freely expoſed to the air, is preſſed upon ‘ by it; whereas the uppermoſt ſurface being contiguous to the ſuperior ſtone, is ‘ thereby defended from the preſſure of the air, which conſequently preſſing the ‘ lower ſtone againſt the upper, hinders it from falling, as we have elſewhere more ‘ fully declared.’ Which laſt words I therefore omit not, becauſe they ſhew, that I handle this matter in this place but incidentally, and may make uſe of what I have delivered, where I treat of it more expreſsly; as I have ſince done in print in the *Hiſtory of Fluidity and Firmneſs*, which Mr. *Hobbes* appears to have ſeen by thoſe cenſures of ſome paſſages of it, that I ſhall hereafter examine.

HIS whole diſcourſe concerning my conjecture, and his ſcheme, would be too prolix to be entirely inſerted. But the thing his diſcourſe drives at is to ſhew, that neither the ſpring nor weight of the air have any thing to do with this phænomenon: and therefore when he had made his academian relate, that two coherent marbles ſuſpended in our receiver did not fall aſunder upon the exhauſtion of it, he ſubjoins Page 10. that it was, *quia nihil iſtic erat, quod ageret atmoſphæræ pondus*; and annexes, *Experimento hoc excogitari contra opinionem eorum, qui vacuum aſſerunt, aliud argumentum fortius aut evidentius non potuit. Nam ſi duorum cohærentium alterutrum ſecundum eam viam in qua jacent ipſæ contiguæ ſuperficies propulſum eſſet, facile ſepararentar, aëre proximo in locum relictum ſucceſſivè ſemper influente; ſed illa ita divellere, ut ſimul totum amitterent contactum, impoſſibile eſt, mundo pleno. Oporteret enim, aut motum fieri ab uno termino ad alium in inſtante, aut duo corpora eodem tempore in eodem eſſe loco: quorum utrumvis dicere eſt abſurdum.* ‘ Becauſe there was nothing there, that the ‘ weight of the atmoſphere ſhould do; no more ſtrong or evident argument could be ‘ made againſt a vacuum than this experiment. For if of two coherent marbles ‘ either of them ſhould be thruſt forward that way, that their ſurfaces lie contiguous,

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* Compare that in the 30. Chap. 14. Sect. of his Phyſics, with that of his Dialogue, page 15, 16.

‘ they would eaſily be ſevered ; the neighbouring air ſucceſſively flowing into the deſerted place. But ſo to pull them aſunder, that at one time they ſhould loſe their whole contact, is impoſſible, the world being full. For then either motion muſt be made from one term to another in an inſtant, or two bodies at the ſame time muſt be in the ſame place: to ſay either of which, is abſurd.’ But how this ſhould be ſo cogent and manifeſt an argument againſt the Vacuiſts, I confeſs I do not well diſcern. For that, which it proves (if it prove any thing) ſeems to be, that in caſe the cohering marbles could be ſo ſevered, as to loſe at once their whole contact, the world might be concluded not to be full: but I ſee not how it thence follows, that therefore there can be no vacuum. For my part, I would demand, whether the ſo ſtrong coheſion of the marbles be neceſſary or not to the plenitude of the world. If it be, how chance a ſufficient weight hung to the lower marble can immediately draw them aſunder? And if it be not, why does not Mr. *Hobbes* aſſign ſome other cauſe of their ſo ſtrong adheſion, if it depend neither upon the ſpring nor weight of the air? As for the non-ſeparation of the two marbles in our receiver, I have ſaid, in the ſame 31ſt experiment, that the cauſe may probably be the preſſure of the air remaining in the receiver not ſufficiently exhauſted. And this conjecture I have more fully defended in what I have written about it againſt *Franciſcus Linus*; where I ſhew, that it is no way unlikely, the remaining air ſhould be able to ſuſtain a weight of four or five ounces hanging at the lower marble, ſince the free air was able to ſupport between 400 and 500 ounces hanging at the ſame.

But Mr. *Hobbes* tells us, that the cauſe I aſſign of the coheſion of our marbles is liable to huge inconveniences; of the greatneſs of which we may judge by the firſt of them; *Confitentur* (ſays he) *tum ipſi tum alii omnes, ponderationem omnem conatum* Page 20. *eſſe per lineas rectas undiquaque ad centrum terræ*; *& proinde non cylindrum vel columnam fieri, ſed per pyramidem, cujus vertex eſt centrum terræ, baſis pars ſuperficiei atmoſphæræ.* ‘ They themſelves and all others confeſs, that all ponderation is an endeavour every way by right lines into the center of the earth; and ſo that it is made not by the figure of a cylinder or column, but by a pyramid, whoſe top is the center of the earth, and whoſe baſis is part of the ſurface of the atmoſphere.’ As if it were much material, whether a body, whoſe baſis is ſcarce two inches diameter, and whoſe length amounts to ſome thouſands of miles, be conſidered as a cylinder or a pyramid. Certainly *Stevinus*, and other learned writers of the hydroſtatics, would ſcarce have made his an objection, ſince they ſcruple not to make it a poſtulatum, that all not very diſtant perpendiculars be looked upon as parallel, though they allow ſuch perpendiculars would meet in the center of the earth. What he adds, partly in theſe words, *Conatus ergo punctorum omnium ponderantium propagabitur ad ſuperficiem marmoris ſupe-* Page 20. *rioris, antequam poſſit propagari ulterius (puta) ad terram*; ‘ Therefore the endeavour of all the points, that ponderate, will be propagated to the ſurface of the upper marble, before it can be propagated further (ſuppoſe) to the earth;’ and partly, in the following lines, to prove, that the whole endeavour of the pyramid, that leans upon the upper marble, is terminated there, and that there is no endeavour of the atmoſphere againſt the under-ſuperficies of the lowermoſt marble: this ratiocination ſeems grounded, partly upon a conceit of his about the nature of gravity, according to which I ſee not, why any body placed between the ſides of that pyramid, or rather cone, whereof the upper-ſuperficies of the higher marble is the baſis, ſhould deſcend upon the account of gravity; and partly from a miſtake of my opinion. For I do nowhere, that I know of, ſpeak, as if I thought this ſuſtentation of the lower marble were performed by little globules, or other minute bodies, protruding one another directly towards the center of the earth, and rebounding from a perfectly ſmooth ſuperficies: nor need I ſay, that the lower ſtone is ſuſtained by the preſſure of the ſelf-ſame pillar of the atmoſphere, that is incumbent on the upper, ſince other parts of the atmoſphere, ſome on the one hand, and ſome on the other, preſſing obliquely upon the uneven ſurface of the earth, may have their preſſure upward terminated againſt the lower ſurface of the undermoſt marble. And in the mentioned *Hiſtory of fluidity and firmneſs*, ſpeaking (*page* 187.) of the adheſion of flat glaſſes (and the reaſon is the ſame in our flat marbles) I plainly deduce it from the preſſure of the fluid air, which, like a liquor, diffuſing itſelf upon the ſurface of the terreſtrial globe, becauſe its deſcent is there reſiſted, does, like water and other liquors, preſs almoſt equally every way, and ſtrongly endeavour to thruſt away any body againſt which it can bear; ſo that wherever the preſſure is taken off from one part of a body, and not from the oppoſite, that body will be preſt toward that part, whether it be downwards, or ſidewards, or upwards, where that formerly equal reſiſtance is removed. And this explication I do in that diſcourſe back with conſiderations and experiments, which Mr. *Hobbes* is not pleaſed to take any notice of: wherefore I hold it not amiſs to add here two or three other experiments to one of thoſe mentioned there.

First then, an æolipile being by heat freed from air as much as you can, if the little hole at the extremity of the neck, by which the air gets in and out, be preſently and carefully ſtopt with wax, and afterwards ſuffered to cool, there will not be in the cavity of the æolipile a reſiſtance any thing near equal to the preſſure of the outward air. And therefore if you perforate the wax, that air will violently be impelled in at the unſtopt orifice, whether the neck be held parallel or perpendicular to the horizon, or in any other ſituation in reſpect of the center of the earth. And the like will happen, if the æolipile be unſtopt under water.

Next, I relate in the mentioned hiſtory, that having drawn ſome of the air out of a large glaſs with a narrow mouth, and thereby deſtroyed the equality of force betwixt the weight of the outward air, and the now weakened ſpring of the inward; I found, that by immediately applying a flat body to the orifice of the veſſel, that body was readily lifted up and ſuſtained in the air as long as I pleaſed, though the weight of it exceeded twenty ounces.

Thirdly, I lately met with the relation of an experiment, which does abundantly make out the power of the ambient atmoſphere to preſs bodies againſt each other, when it cannot get between their internal ſurfaces. For the ingenious author of the Magdeburgic experiment writes to the induſtrious *Schottus*, that having cauſed two copper-plates to be made almoſt in the form of ſcales a little above half an ell in diameter, and exactly congruous, if laid upon one another; *Has* (ſays our Jeſuit) *ſi mutuo ſibi imponit, & aërem extrahit, adeo ab externi aëris gravitate compreſſæ atque unitæ tenentur, ut ſex viri robuſti eas divellere non poſſint. Quod ſi tandem adhibito omni conatu divellantur, crepitum edunt ſclopeti aut muſquetæ exploſioni non minorem; quamprimum vero per claviculum ſeu epiſtomium apertum vel minimus aditus laxatur aëri, ſponte ſeparantur.*

Schot. Mech. Hyd. Pneumat. page 461.

‘ Theſe ſcales he puts one upon another, and draws out the air, and then are they kept
‘ ſo compreſt and united by the gravity of the external air, that ſix ſtrong men cannot
‘ pull them aſunder. But if at length, by the uſe of the utmoſt endeavour, they are
‘ plucked in ſunder, they make a noiſe equal to the report of a muſquet; but as ſoon
‘ as ever, by the ſtop-cock opened, there is the leaſt entrance given to the air, they
‘ are ſevered of their own accord.’

And, fourthly, if a glaſs phial (ſuch as will be anon more fully deſcribed) have a pipe open at both ends ſo fitted into it, that no air can get in and out betwixt the neck and it; and if the phial be ſo far filled with water, that the lower end of the pipe be well immerſed therein; if then you ſuck at the upper end of the pipe, the water will aſcend to a good height; which argues its being forced upwards by the oblique preſſure of the air in the phial: for it is only in the pipe, and not in the phial, that there is any air in the ſame perpendicular with the water that is impelled up.

But let us follow Mr. *Hobbes* a little further. Having aſked this queſtion, *Sed vis illa elaſtica, quam in aëre eſſe dicunt, nihilne ad marmor ſuſtinendum conferre poteſt?* he anſwers, *Nihil omnino; non enim conatus in aëre eſt ullus ad centrum terræ magis quam* Page 20, 21. *ad aliud quodvis punctum univerſi. Quoniam enim gravia omnia tendunt à circumferentia atmoſphæræ ad centrum terræ, & inde rurſus ad circumferentiam atmoſphæræ per eaſdem lineas reflexas, conatus ſurſum conatui deorſum æqualis erit, & proinde mutuo ſe perimentes neutrâ conabuntur viâ.* ‘But can the ſpring, which they ſay is in the air, confer nothing to the holding up the marble? —— nothing at all: for there is no endeavour of the air to the center of the earth, more than to any other point in the univerſe. For ſeeing that heavy things tend from the circumference of the atmoſphere unto the center of the earth, and thence again to the circumference of the atmoſphere by the ſame reflected lines, the endeavour upwards will be equal to the endeavour downwards, and ſo deſtroying one another, they will endeavour neither way.’ But that the ſpring of the air may perform ſomewhat in the caſe propoſed, I hope the newly mentioned experiments have evinced. And the reaſon he annexes to his negative, as alſo the difficult example he ſubjoins, of a man lying in the bottom of the ſea, ſeem rather oppoſed to the weight than the ſpring of the air. But we have already, both by experiments, and by his own conceſſions, ſufficiently proved, that the air is not devoid of gravity: and that it likewiſe gravitates upon the terraqueous globe, which in this page he ſeems to deny, we have proved by divers reaſons, and particularly in our 25th experiment, by the vaſt expanſion of air under water, when the preſſure of the incumbent air was taken off from the water.

As for the ſcheme he annexes, I confeſs I do not well ſee what he drives at in it; at leaſt if it be intended for a confutation of the conjecture I have been defending: nor am I the only perſon, that complain of his writing often enough obſcurely. And as far as we can judge by the concluſion, couched in theſe words, *Non poteſt ergo pars* BC. (which in the explication of the ſcheme he calls *pars atmoſphæræ intra totam poſita ubicunque) (propter magnitudinem) quantumvis gravis ſit, deſcendere, neque ergo premere ſive gravitare*;. ‘Therefore the part BC (that is, a part of the atmoſphere placed any where within the whole) cannot (by reaſon of its greatneſs) deſcend, although it be heavy, and therefore it cannot preſs or gravitate;’ his notions about gravity are ſomewhat ſtrange, and probably either do not concern us, or will be found repugnant to thoſe experiments, on which our conjecture is grounded.

And as for what he adds, *Si poſſibile eſſe negarem, ut diligentiâ & arte humanâ duæ* Page 22. *ſuperficies corporum duorum inter ſe per omnia puncta ita accurate congruæ fiant, ut ne minimo quidem corpuſculo creabili tranſitus permittatur, non video quomodo illi aut ſuam hypotheſim tueri, aut negationem noſtram improbitatis arguere jure poſſent:* ‘If I ſhould deny it poſſible, that by the art of man, two ſurfaces of two bodies could be made ſo accurately fit, that they ſhould touch in all points, ſo that there could no creable corpuſcle paſs between them; I do not ſee, how they could defend their own hypotheſis, or diſprove our negative aſſertion:’ I confeſs I do not ſee how this argumentation proves any thing againſt the intereſt, either of the weight, or ſo much as of the ſpring of the air in the coheſion of our marbles. For, provided that the corpuſcles of the air get not in between the two ſtones, the preſſure of the air may well ſuffice to keep them together. And, laſtly, as for that modeſt paſſage (that immediately Page 21, 22. precedes the words newly recited) wherein he ſays, *Utraque illa phantaſia, tum gravitatis atmoſphæræ, tum vis elaſticæ ſive antitupiæ aëris, ſomnium erat. Siquidem autem illis concederetur eſſe aliquam in filiculis aëris antitupiam, quæretque aliquis, unde illa curvata quidem ſed quieſcentia moverentur rurſus ad rectitudinem, deberent illi, ſi phyſici haberi volunt, cauſam ejus aliquam poſſibilem aſſignare:* 'Both theſe fancies, as well that of the weight, as of the ſpring or antitupy of the air, are dreams. But if it be granted, that there is a kind of recoiling in thoſe ſmall hairs or ſlender corpuſcles, of which the air conſiſts; one may inquire, whence it is, that thoſe crooked bodies, ſettled and at quiet in that poſture, came to be moved into a ſtreightneſs. They ought, if they will be eſteemed natural philoſophers, to aſſign ſome poſſible cauſe of this:' we have already given an account, why we forbore to aſſign a cauſe of the motion of reſtitution; but methinks Mr. *Hobbes* might have, for the ſpeaking ſo, choſen a fitter place than this, where he gives me ſo fair an opportunity to tell him again, that he ſhould, if he would be thought a Naturaliſt, have aſſigned ſome cauſe of the phænomenon, about which he had all this while been diſputing; which ſince even he himſelf has declined to do, perhaps the phænomenon will be thought ſomewhat difficult, and my attempt at diſcovering the cauſe of it will be at leaſt excuſed.

BUT after having ſo long dwelt upon the conſideration of our marbles, it is high time to proceed to what remains.

IN the 24th page, Mr. *Hobbes* has that paſſage I formerly recited touching the glaſs-fountains, out of which water is thrown up by compreſſed air. But though I, as well as others, have made uſe of ſuch fountains, invented by *Vincenzo Vincenti* of *Urbin*, and was unſatisfied with the account he gives of their phænomena; yet in regard ſome learned men, and particularly Dr. *Ward*, have already examined his explication, and I am not obliged to do ſo; I ſhall only take notice of what our author objects, to prove that this phænomenon cannot be ſolved by the ſpring of the air, in theſe words:

Page 24. B. *Cur non poteſt aqua, quæ cum injiceretur, particulas aëris comprimebat, ab iiſdem particulis ſe explicantibus rurſus rejici?*

A. *Quia locum explicatæ majorem non requirunt quam compreſſæ. Quemadmodum in vaſe aqua pleno, in qua eſſet multitudo anguillarum, anguillas ſive in ſe volutas ſive explicatas idem ſemper capit locus. Propellere ergo aquam per vim elaſticum, quæ alia non eſt quam motus corporum ſe explicantium, non poſſunt.*

B. *Comparatio illa aëris cum aqua anguillis plena, noſtris, credo, non diſplicebit.*

'*B.* Why cannot the water, which when it was injected did compreſs the particles of air, be again caſt out by the ſame particles explicating themſelves?

'*A.* Becauſe when explicated they require no greater place than when compreſſed: as in a veſſel full of water, wherein are many eels, the ſame proportion of place receives them, whether they are folded round or at length. Therefore they cannot drive up the water by their ſpring, which is nothing elſe but the motion of bodies explicating themſelves.

'*B.* The compariſon of air to eels in water, I ſuppoſe, will be well received by our academians.'

But the Elateriſts will anſwer, that neither can his earthy atoms, to whom he aſcribes the rejection of the water forced in, truly fill up more parts of ſpace at one time than at another; and therefore the objection might have been ſpared: but indeed it reaches us not. For we, as Mr. *Hobbes* knows well, are not wont to compare the air to eels, but to wool: and though each hair, that makes up a compreſſed lock of wool does not really fill more ſpace with wool when extended than when crumpled; yet when there is a congeries of theſe hairs compreſſed together, the whole fleece or congeries does by its ſpring endeavour to thruſt away thoſe contiguous bodies, by which it is penned up, as I have more fully explained in my epiſtle: ſo that theſe aërial corpuſcles, being pent up by the water forced into the glaſs, ſtill endeavour to expand themſelves by throwing it out.

What our author adds in the ſame page, as if they were miſtaken, that think the experiments of the Pleniſts tended (eſpecially till of late) to prove, that the generality of them did not always mean by a vacuum a ſpace perfectly devoid of all corporeal ſubſtance, but any ſpace here below, that is not filled with a viſible body, or at leaſt with air (for theſe are my words, to which I ſuppoſe Mr. *Hobbes* alludes): to this, I ſay, it is ſcarce worth while to make anſwer, the controverſy being of ſuch ſmall moment (though I think I could eaſily enough do it) eſpecially ſince he rather excuſes thoſe, that may have negligently expreſſed themſelves, than diſproves what I ſaid. And ſince I ſpoke chiefly and by name of the Peripatetic ſchools, he may well allow, that their expreſſions concerning this matter were not always ſo accurate, whilſt in this very paſſage he concludes with theſe words; *Vides quam ineptum ſit ad explicationem effectuum talium advocare verba metaphorica, ut fugam vacui, horrorem naturæ, &c. quibus olim ad exiſtimationem ſuam tuendam uſæ ſunt ſcholæ*: 'You ſee, how fooliſh 'a thing it is to bring for the explication of ſuch effects metaphorical words, as the 'ſhunning of a vacuum, the abhorrence of nature, &c. which heretofore the ſchools 'uſed, to defend their reputation.' Nor is what he adds concerning the vacuum to be attributed to *Democritus* and *Epicurus* either clear enough, or of concernment enough to our diſpute, to be inſiſted on by us; eſpecially ſince I ſee not to what purpoſe he brings it in. Page 25.

But there are in this page two particulars, which, though they make little or nothing againſt what I ſaid of the Pleniſts, may deſerve to be taken notice of.

The ſecond (for I think it expedient to diſpatch that firſt) is couched in theſe words; *In hydriis perforatis ideo hæret aqua, quia quæ per tantillum foramen exiturit, adeo exigua eſt, ut non poſſet ita in longitudinem ſe diffundere, ut deſcendendo aditum aëri faciat, per foraminum circumferentias; neque aër ab exeunte aqua pulſus locum alium (in mundo pleno) habere poteſt præterquam quem aqua deſeret*: 'In the gardener's watering-pots 'therefore is the water ſuſpended, becauſe that which iſſues out at ſo ſmall a hole is 'ſo little, that it cannot diffuſe itſelf to ſuch a length, that by its deſcent it may give 'paſſage to the air through the circumferences of the holes. Nor can the air driven 'off by the water going out find any other place, beſides that which the water 'leaves.' Page 25.

But this experiment I have already examined as it is propoſed in his *Elements of Philoſophy*; and therefore I ſhall now only ſay to the light variation I find of it here, that the reaſon here aſſigned, why the water in gardeners pots cloſed at the top does not deſcend, is not rightly aſſigned, ſince (to omit other objections) by Monſieur *Paſchal*'s experiment it appears, that though in pipes of no great length the water will not run out, yet if the pipe be long enough, though the orifice be no wider, the water will deſcend without giving paſſage to the air at the circumference of it.

Page 25. BUT the other particular here mentioned by Mr. *Hobbes*, who thus propoſes it, *Qui per fiſtulam ore aquam ſugit, aërem medium prius ſugit, quo diſtentum aërem externum removet; qui remotus locum (in pleno) habere niſi proximum removendo non poteſt; & ſic continua pulſione aqua tandem pellitur in fiſtulam, ſucceditque aëri, qui exugitur:* He that ‘ſucks water into his mouth by a pipe, firſt ſucks up the air between, whereby he removes the diſtended external air, which being removed (the world being full) it can have no place but by removing the next, and ſo by continual pulſions the water is at length driven into the pipe, and doth ſucceed the air, which is ſucked out;’ deſerves a more particular conſideration. For this account of the aſcenſion of liquors by ſuction is not only here given by Mr. *Hobbes*, but for the main by the learned *Gaſſendus* himſelf, and other Atomiſts, and is generally acquieſced in by the modern philoſophers; perhaps the rather, becauſe it ſeems not to eſtabliſh or overthrow a vacuum. But though I ſhall not deny, but that many phænomena of nature may be probably explicated by this propagation and return of motion; yet there are ſome phænomena here below, which I ſee not how the Carteſians, or the Atomiſts, or Mr. *Hobbes* can explicate, without admitting the ſpring of the air, and which perhaps by the ſpring of the air may be explicated, without the recurring to ſuch a propagation and return of impulſe. Divers inſtances to this purpoſe I elſewhere conſider, but at preſent I ſhall propoſe only one experiment purpoſely deviſed to ſhew, that both Vacuiſts and Pleniſts ſhould admit an elaſtical power in the air. I took then a glaſs-veſſel conſiſting of two parts; the one was a phial capable of containing about a pound of water, and the other a pipe open at both ends, the lower of which reached within two inches of the bottom of the phial. This pipe was by the glaſs-man faſtened into the neck of the phial, not by any cement, leſt it ſhould be pretended, that the air might undiſcernedly get in or out, but with melted glaſs of a good thickneſs: into this veſſel, by the open pipe, I at length (for it is ſomewhat difficult) poured water enough to ſwim a pretty way above the lower extreme of the pipe, and then often inclined the veſſel, to give a free intercourſe betwixt the air within the phial and that without it, that if the internal air were compreſſed by the affuſion of the water, it might free itſelf, as it readily did by aſcending in bubbles along the inclined pipe, till the outward and inward air were reduced to an equality of preſſure. Now if all ſuction were produced by the preſſure of the air, thruſt away by the dilated cheſt of him that ſucks, and ſo thruſting the water or other liquor into the pipe at which he ſucks, it ſeems evident, in our caſe, that the water would not aſcend by ſuction; ſince by the contrivance of the veſſel, the air thruſt away by him, that ſucks, cannot at all come to bear or preſs upon the water. And yet, whether the pipe were inclined or erected, the water did according to my expectation eaſily enough aſcend, upon ſuction, to the top of the pipe, and ran over into my mouth. I ſay, eaſily enough; becauſe that though the ſpring of the air pent up in the phial were able, upon the decreaſe of the preſſure of the outward air, occaſioned by my ſucking, to impel the water ſtrongly enough into the pipe; yet, when a pretty quantity of water had been ſo impelled up, the included air gaining thereby more room to expand itſelf, its ſpring was thereby ſo far weakened, that the water aſcended far leſs eaſily than in ordinary ſuction. The other circumſtances worth noting in this experiment belong not to this place; and what has been delivered may, I hope, ſuffice for the purpoſe it is alleged for. Only one particular I ſhall here add, by way of confirmation of what I ſaid touching the weakened ſpring of the air, and it is this; that partly to ſhew ſome, who yet embrace the opinion of the ſchools, that the aſcenſion of the water in the pipe did not proceed from any ſuch tendency in the water itſelf to aſcend for prevention of a vacuum, and partly for other reaſons, that concern not this place, I did carefully take out the water by degrees, till the lower end of the pipe was but very little under the ſurface of the water; though in the cavity of the pipe, the water, as it uſually will be in pipes that are not wide, was a pretty deal higher. Then ſuffering the veſſel to reſt, and ſucking at the upper end of the pipe, the water (as I foreſaw it would be) was impelled up, yet without reaching near the top, till the ſurface of it was fallen a little below the bottom of the pipe. But then, though I continued ſucking, no more water aſcended into the pipe, but the air paſſing through it towards my mouth, did in its paſſage toſs up the water, that was already in the pipe, and turn it into bubbles (of a ſtrong bigneſs, when the cavity of the pipe would permit it) which broke (not without noiſe) one after another: and thus the aſcending air for a pretty while kept the water in the pipe from falling back to that in the phial. But when I removed my mouth, the ſpring of the air remaining in the cavity of the phial, being debilitated by the receſs of the air I had (as men are wont to ſpeak) ſucked out, it was not able to reſiſt the preſſure of the outward air, and accordingly the water in the pipe was not only depreſſed into the phial, but the outward air forced its way in many bubbles, and not without ſome noiſe, through the water contiguous to the bottom of the pipe, till the preſſure of the included air and that of the atmoſphere were reduced to an equality.

But in the ſame 25th page our author tells us, that the ſociety he writes againſt, would have the cauſe of filtration, and that of the paſſage of water through ſiphons, to be the ſame. To which he annexes, this peremptory paſſage; *Id vero impoſſibile eſt. Nam in ſiphone niſi ambo crura aquâ impleantur, aqua è pelvi non aſcendet. Aſcenſionis cauſa in pannum eſt motus ille terrearum atomorum, quæ aquæ contiguæ ſunt, motus (inquam) circularis ſimplex aëri in quo moventur communicatus; quæ atomi aquam ferientes in materiam laneam incutiunt, incuſſæ autem magis magiſque madefaciunt, donec madida tota ſit. Cum vero madida tota ſit, &c.* ‘But that is impoſſible: for in a ſiphon, unleſs both legs are filled with water, the water will not aſcend out of the baſon. The cauſe of its aſcent into that cloth is the motion of the earthy atoms, which are near the water; I ſay the ſimple circular motion communicated to the air, in which they move; which atoms ſtriking the water, beat it up into the woolly matter, which beating of them againſt the cloth, makes it more and more moiſt, till it becomes all over wet. And when it is ſo, *&c.*’ Thus far he; but the paſſage in my epiſtle, upon which he ſeems to have grounded his oppoſition, is but this, (wherewith I begin my 35th experiment) *Some learned mathematicians* (I meant the induſtrious *Schottus* and ſome Carteſians) *have of late ingeniouſly endeavoured to reduce filtres to ſiphons; but ſtill the cauſe of the aſcenſion of water and other liquor, both in ſiphons and filtration, needing (for aught we have yet found) a clearer diſcovery and explication, we were deſirous to try*, &c. So that neither did I aſcribe this reduction (of filtres to ſiphons) to a ſociety, which was not then in being, nor perhaps ſo much as deſigned; nor did I adopt it myſelf; but expreſſed a deſire to have it further examined. But as for the cauſe of filtration itſelf, I may take a fitter opportunity to diſcourſe of it: in the mean time, I doubt, whether the reaſon here aſſigned by Mr. *Hobbes* will not ſeem as well precarious, as the *motus circularis ſimplex* of earthy atoms, whereon it is grounded. Nor does his explication render a reaſon, why quickſilver will not aſcend the 14th part as high in the filtre, though in part immerſed into it, as water; nay, will not reach ſo high, where it is contiguous to the filtre, as where it is not: nor why it ſhould begin to aſcend, ſince, for aught he ſhews to the contrary, the preſſure of the air, even in the ſenſe he takes the air, ought to be the ſame on that part of the ſurface of the liquor, which is contiguous to the filtre, and on any other part of the ſame ſurface. To which I ſhall only add, that as reſolutely as Mr. *Hobbes* ſays it is impoſſible for the water to aſcend out of the veſſel into a ſiphon, unleſs both the legs be filled with that liquor, he would probably have ſpoken more warily, and diſtinguiſhed betwixt ſiphons, if he had been pleaſed to take notice of what I relate in the forementioned 35th experiment, of a ſmall glaſs-ſiphon I deviſed, whereof when the ſhorter leg was but dipped in water, the liquor did preſently, as it were of itſelf, run down the longer leg. Which experiment, beſides other conſiderations, may induce us to ſuſpect, that the nature of ſiphons, and of filtration, may not yet be ſo thoroughly underſtood, as not to deſerve a further inquiry.

BUT to draw at length towards a concluſion of our troubleſome *Examen*; it remains only, that I take ſome notice of the general corollary, that Mr. *Hobbes* is pleaſed to deduce from his whole diſcourſe, of the experiments exhibited in our engine.

Page 23. A. *Fateris ergo* (ſays he) *nihil hactenus à collegis tuis promotam eſſe ſcientiam cauſarum naturalium, niſi quod unus eorum machinam invenerit, qua motus excitari aëris poſſit talis, ut partes ſphæræ ſimul undiquaque tendant ad centrum, & ut hypotheſes* Hobbianæ, *ante quidem ſatis probabiles, hinc reddantur probabiliores.*

B. *Nec fateri pudet; nam, eſt aliquid prodire tenus, ſi non datur ultra.*

A. *Quid tenus? Quorſum autem tantus apparatus & ſumptus machinarum factu difficilium, ut eatenus tantum prodiretis, quantum ante prodierat* Hobbius? *Cur non inde potius incepiſtis, ubi ille deſiit? cur principiis ab eo poſitis non eſtis uſi? Cumque* Ariſtoteles *recte dixiſſet, Ignorato motu ignorari naturam, quomodo tantum in vos ſuſcipere onus auſi eſtis*, &c.

‘ *A.* You confeſs then, that your collegiates have as yet in nothing advanced the
‘ knowledge of natural cauſes, but that one of them hath found out an engine, in
‘ which there may be ſuch a motion of the air excited, that the parts of the ſphere
‘ may together every way tend unto the center; and that the hypotheſes of Mr.
‘ *Hobbes*, before probable enough, may be thence made more probable.

‘ *B.* Right; I am not aſhamed to confeſs it; for it is ſomewhat to arrive ſo far,
‘ if we can make no further progreſs.

‘ *A.* Why *ſo far?* To what end ſuch preparation and charge for engines difficult
‘ to be made, to make no further diſcovery than Mr. *Hobbes* had made before you?
‘ Why did you not rather begin where he ended? Why did you not uſe the prin-
‘ ciples he had laid; and when *Ariſtotle* had rightly ſaid, that *without the knowledge*
‘ *of motion there is no knowledge of nature*; how durſt you take ſuch a taſk upon your-
‘ ſelves?’

As to what he ſays to the diſparagement of the aſſembly, and in his own praiſe, the laws I preſcribed myſelf, at the beginning of this diſcourſe, forbid me to reply to what is more likely, amongſt civil and judicious readers, to prejudice his own reputation, than theirs he is ſo diſpleaſed with. And as for that, which he aſſigns to be the uſe of our engine, I ſhall very willingly leave it to the ingenious to judge, what uſe may be made of it. But as for this mentioned by Mr. *Hobbes*, if he means here, as he elſewhere teaches (pag. 13 and 19) that the motion he ſpeaks of is produced by that of the air impelled betwixt the ſides of the ſucker and the cylinder; perhaps it will be thought I have ſufficiently proved, that it is not *any* of its uſes, ſo far is it from being its chief or only uſe. But I confeſs I ſomewhat wonder Mr. *Hobbes* ſhould quarrel with me (for it is I that in my epiſtle employ the following verſe) for ſaying,

Eſt aliquid prodire tenus, ſi non datur ultra.
'Thus to have made an entrance, though we miſs
'Of further progreſs, ſome performance is.'

And this not, as ſome would perhaps ſuſpect, becauſe I do not imitate him, in ſpeaking of myſelf, as he does of himſelf, but becauſe he thinks the expreſſion too arrogant. For ſince he here confeſſes, that his hypotheſes are by this engine rendered more probable, ſome will perchance think that to be enough to entitle my experiment to ſome degree of uſefulneſs, unleſs Mr. *Hobbes*'s doctrine of the air had found more embracers than before theſe ſeaſonable, though deſpiſed experiments, it was obſerved to have. But, ſince either of us may be partial in his own caſe, I am very well content to leave others to judge, both whether my expreſſion have been guilty of arrogance, and how much he has done more than *prodire tenus* in all the paſt long diſcourſe againſt me, when they have conſidered, what new experiment or matter of fact Mr. *Hobbes* has therein added to enrich the hiſtory of nature, what new truths he has diſcovered, or what errors (except one of his own in the laſt page) he has well confuted. Theſe things, I ſay, I am very well content to leave to be judged of by all diſintereſted perſons, without being much diſcouraged by the differing ſtrain, wherein Mr. *Hobbes* thinks fit to ſpeak of his own performances and mine, or invited to imitate him in that way of writing; my endeavours (ſuch as they are) having hitherto been favoured with ſuch a reception among the Virtuoſi, that poſſibly I may have almoſt as little need as reaſon to commend them.

But it is ſomewhat troubleſome work to argue long with a man, that's angry with an expreſſion, which perhaps none but he would have found fault with for want of modeſty: and therefore as I have left unrecited ſeveral provoking and very undeſerved expreſſions he employs in the ſame page, and even that paſſage, where, to prove our naturaliſts and mathematicians profeſſed they would not receive truth coming from him, he alleges only a ſaying (whether true or no, I examine not) of Dr. *Owen*, who, beſides that his profeſſion was divinity, not philoſophy or mathematics, neither is nor ever was of our ſociety: as, I ſay, I have (for the reaſon newly intimated) declined taking notice of matters of this nature; ſo I will not now ſtay to inquire, why he urges us, whom he would have men take for Vacuiſts, with the authority of *Ariſtotle*, whom, on other occaſions, he is wont to uſe with as little reſpect, as if he were a member of our ſociety. Nor ſhall I now examine, why here, and elſewhere, he ſends us to his own writings for the doctrine of motion; as if, to omit antienter authors, ſuch great perſonages as *Galilæo*, *Merſennus*, *Verulam*, *Des Cartes*, *Gaſſendus*, *Balianus*, *Johannes Marcus Marci*, *Honoratus Fabri* (not to mention other moderns, nor thoſe of our own aſſembly, as the eminently-learned Sir *Kenelme Digby*, and the others, whom their modeſty forbids me here to name) had not moſt of them learnedly, and ſome of them copiouſly, written of local motion before Mr. *Hobbes*'s books, where he treats of it, came abroad into the world: this, I ſay, I ſhall not inſiſt on, becauſe I would haſten to a concluſion. Only one thing I muſt add, that whereas he accuſes us of deviſing elaborate and ſumptuous engines, I do not fear to find ſo many readers of his mind, that I need make them excuſes for what perhaps will obtain their thanks. And whatever Mr. *Hobbes* may think, for my part I freely confeſs, that I love truth ſo well, that I do not think, not only my pains and charges, but, even what I rate much higher, my time itſelf, too much for the diſcovery of truth; or (that Mr. *Hobbes* may not think me partial) even for the eſtabliſhment of ſuch truths, as, though diſcovered by ſome, are yet as far more generally oppoſed than embraced.

THERE remain yet ſome other pages of Mr. *Hobbes*'s dialogue, wherein he ſpeaks of fire, and cold, and ice, and light, and colours, and fluidity, and hardneſs, and thickneſs, and ethics, and politics, and the duplication of the cube, and the quadrature of the circle, and ſeveral other ſubjects. But theſe I forbear to meddle with, not that I approve them all, or the greateſt part, but partly, becauſe I am too much tired already, to be fond of engaging in controverſies, that I am not tied to meddle with (except what concerns fluidity and firmneſs, which I ſhall, God willing, examine by itſelf) partly, becauſe divers paſſages relate to perſons, not things: partly becauſe I do not much fear, that Mr. *Hobbes* will find every reader ſo eaſy and complaiſant as he makes his academian, who in many paſſages of the dialogue ſpeaks not unſuitably to what he does in the laſt page of it; where he excepts but one particular, (and that is neither the *duplication of the cube, nor the ſquaring of the circle*) when he tells Mr. *Hobbes*, *diſcedo jam multo (ut mihi videor) quam ante certior, & quæ dixiſti omnia teneo & probo:* and partly, becauſe Mr. *Hobbes* has ſome things, as about fire, and certain colours, which I am not forward to reject, though the conſiderations, that incline me to ſome ſuch opinions, be perhaps very differing from the grounds, on which he propoſes them. And indeed as well my nature as my cuſtom forbids me to ſcruple to learn, if I can, of perſons much leſs famous than Mr. *Hobbes.* It is far from my humour to write againſt all that every man ſays, that (how cauſeleſsly ſoever) writes againſt me; and I am almoſt as much indiſpoſed to reject as to embrace, without diſtinction, whatever it be, that this or that man teaches.

CHAP. VII.

(Being an Appendix to the paſt Diſcourſe:)

Wherein is examined what Mr. Hobbes *teaches concerning Fluidity and Firmneſs.*

ALTHOUGH Mr. *Hobbes* do not name me for the maintainer of the opinions which I have propoſed in the *Hiſtory of Fluidity and Firmneſs*, and which he cenſures; yet, ſince that hiſtory, after having been mentioned in the book I have hitherto been defending, was publiſhed a good while before Mr. *Hobbes*'s dialogue; and ſince ſome of the chief particulars he cenſures or takes notice of, are delivered there, and ſome of them, perchance, no where elſe; I think it may concern me to examine what he ſays either againſt my opinions, or in favour of his own, touching *Fluidity and Firmneſs.* And if it be ſaid, that he meant not his objections particularly againſt me, but rather againſt ſome other perſon, who may have liked ſuch opinions; I ſhall anſwer, that I am content to leave to others the defence of their particular opinions, and to have as much of the following diſcourſe, as is concerned in this allegation, looked upon as written only upon this ſuppoſition, that my writings are thoſe he deſigns to oppoſe. But there being other things in what he diſcourſes about *Fluidity and Firmneſs*, which, for the reaſons freſhly intimated, I think fit here to conſider, I chuſe to gather up together the paſſages touching theſe ſubjects, which I find ſcattered in his dialogue, that I may have the better opportunity to clear up the matter itſelf under debate.

BUT, before I go further, I muſt, at the very entrance of this diſcourſe, take notice, that in the fifth page, where Mr. *Hobbes* begins to diſpute againſt our notion of *Fluidity*, he very much miſtakes my opinion, as may appear by theſe words, which Page 5. he puts into the mouth of his academian; *Sed plerique noſtrum naturam fluidam à non fluida diſtinguimus magnitudine partium, ex quibus corpus aliquod conſtat, & quaſi compingitur:*

pingitur: itaque non modo aërem, aquam & liquorem omnem, ſed etiam cinerem & pulverem, tanquam fluida contemplamur. Et fluida ex non fluidis compoſita eſſe poſſe non negamus; nam diviſibilitatem illam infinitam non concoquimus. To which he anſwers, *Diviſio quidem infinita concipi non poteſt, diviſibilitas autem facile. Ego contra, diſtinctionem non capio intra fluida & non fluida, quam ſumitis à magnitudine partium; nam ſi caperem, ruina illa ſive rudera illa, quæ jacent in eccleſia Paulina, mihi dicenda eſſent fluida: ſin propter nimiam lapidum magnitudinem fluida illa eſſe negaveritis, defini mihi magnitudinem illam, quam habens pars ruentis muri propter eam ſit dicenda fluida. Tu vero, qui diviſibilitatem infinitam non capis, dic mihi, quæ tibi apparet cauſa, quare Deo omnipotenti difficilius eſſe putem creare corpus fluidum, & cujus partes actu diffluant, omni data atomo minus, quam creare oceanum. Itaque deſperare me facis omnem conventus veſtri fructum, dicendo, quod putant aërem, aquam, & cætera fluida conſtare ex non fluidis, tanquam ſi murum, cujus ruentes lapides aliquoſque diſcurrunt, dicerent eſſe fluidum. Si ſic loquendum eſt, nihil non eſt fluidum; nam etiam marmor comminui poteſt in partes omni atomo Epicureana minores.* ‘But ‘moſt of us diſtinguiſh the nature of fluid from that which is not fluid, by the great‘neſs of the parts, of which any body conſiſts and is made up with. Wherefore we ‘do not not only look upon air, water, and all liquors, but upon aſhes alſo and duſt, ‘as fluid bodies. And we deny not, that fluid things may be made of things not ‘fluid; for we do not digeſt the notion of infinite diviſibility.

‘*A.* Infinite diviſion cannot be conceived, but (infinite) diviſibility may eaſily. ‘I, on the contrary, do not underſtand the diſtinction of fluids and not fluids, which ‘you take from the greatneſs of the parts: could I digeſt this, I muſt ſay, the ruins ‘of ſhattered rubbiſh ſtones, that lie in *Paul*’s, were fluid. But if thoſe ruins cannot ‘be called fluid, becauſe the ſtones are too big, define me the bigneſs, that the parts ‘of a ruined wall muſt have, that they may be called fluid. But you, that cannot ‘underſtand infinite diviſibility, tell me what you think to be the cauſe, why I ‘ſhould think it more hard for almighty God to create a fluid body leſs than any ‘atom propoſed, that its parts might actually flow, than to create the ocean? There‘fore you make me deſpair of any fruit of your meeting, by ſaying, that they think ‘air, water, and other fluids conſiſt of non-fluids; as if a wall, that began to fall ‘and be ruinous, were called by them a fluid body. If they may ſpeak ſo, every ‘thing is fluid, for even marble itſelf may be broken into parts leſs than any atom ‘imagined by *Epicurus*.’ Thus he. But in my Hiſtory, though I make the ſmallneſs of the parts, whereof a body conſiſts, one of the requiſites to its being fluid; yet at the end of the 13th ſection I call the various agitation of thoſe particles the principal qualification of all, and in the beginning of the 14th ſection I call it the chief condition of a fluid body. And therefore he much miſtakes, if he thinks, that we always conſider aſhes and duſt as fluids abſolutely ſpeaking. But as he *ſomewhere tells us, that by fire he underſtands the combuſtible matter itſelf, not ſimply and always, but then only when, &c. ſo neither do I look upon the duſt of alabaſter (to my words about which, I ſuppoſe, he has a reſpect in the paſſage under conſideration) as a fluid body ſimply and always; for I clearly teach the contrary, but only when and whilſt its parts are not alone reduced to a competent ſmallneſs, but are alſo actually put into ſuch a various agitation as makes the body they compoſe (even according to Mr. *Hobbes*'s own definition, which is, that ‘fluid bodies are thoſe ‘whoſe parts may, by very weak endeavour, be ſeparated from one another’) emulate a fluid body, by the very eaſy ceſſion of its component corpuſcles, and by its boiling like a liquor. By this the reader may diſcern how little, that makes againſt me, which Mr. *Hobbes* talks, and ſeems to do it ſeriouſly, of the ruins of *Paul*'s; as if, according

* *Hobbes de Corp. ch. 21.*

Mr. *Hobbes de Corpore, cap.* 26. *part* 4.

according to my opinion, *Ruina illa ſive rudera, quæ jacent in eccleſia Paulina, dicenda eſſent fluida*; 'the ruins that lie in *Paul*'s church might be called fluid.' For it is moſt evident, that I require in the parts of a fluid body both minuteneſs and ſuch a motion along each other, as makes them eaſily yield to the touch: which qualifications, how well they belong to the ruins of *Paul*'s, is, I think, not very difficult to determine; though in the ſame paſſage Mr. *Hobbes* does again make uſe of the like example, to which he ſubjoins, *Si ſic loquendum eſt, nihil non eſt fluidum*; 'if they may ſo ſpeak, there is nothing but is fluid.' Which, how little it follows from my doctrine about *Fluidity*, there needs not a quick-ſighted reader to diſcern. As for the reaſon he annexes in theſe words, *Nam etiam marmor comminui poteſt in partes omni atomo Epicureana minores*; 'for even marble may be beaten into parts leſs than any Epicurean atom:' I would gladly know, by what art Mr. *Hobbes* can divide marble into leſſer particles than ſuch as are naturally indiviſible (for ſuch *Epicurus* makes his atoms to be:) nor do I ſee how, in caſe this could be done, it proves, that there is not any thing that is not fluid. For I ſay, that the blocks of marble before comminution are not fluid, either according to him or according to me; nay, the greateſt comminution imaginable would not, according to my doctrine, make a lump of marble fluid, unleſs the heap compoſed of the parts, how minute ſoever, were actually and variouſly ſet a moving amongſt themſelves. But he would perhaps have ſpoken more warily, if he had conſidered the difference there is betwixt ſaying, that all things are fluid, and ſaying, as I do, that there are many bodies, that are now ſolid, which by comminution, and other requiſite alterations, may be made parts of a fluid body: as hard ice may be turned into fluid water, and quickſilver precipitated *per ſe* into a red powder, may be reduced to running mercury. As for what he ſays of an infinite diviſibility of body, it is ſcarce in this place worth while to examine it.

For I have ſhewn in the *Hiſtory of Firmneſs*, that this diviſibility (which I had then conſidered) does no way overthrow my doctrine of fluidity; nor does Mr. *Hobbes* here anſwer what I there diſcourſe. Beſides that indeed, I do not ſo well underſtand what he means and drives at, when he ſays that *Diviſio quidem infinita concipi non poteſt, diviſibilitas autem facile*: 'Diviſion that is infinite cannot be conceived, diviſibility may eaſily.' For ſince in this very paſſage and within a very few lines he has recourſe in this matter to God's omnipotence, I ſee not, why an infinite diviſion cannot be as well conceived as an infinite diviſibility, ſince ſure an omnipotent agent is able to do what is poſſible to be done; and why elſe ſhould a body be called infinitely diviſible? Beſides, when Mr. *Hobbes* has recourſe to what God can do (whoſe omnipotence we have both great reaſon to acknowledge) it imports not to the controverſy about fluidity to determine what the almighty Creator can do, but what he actually has done. And laſtly, whereas my adverſary requires to have the magnitude defined, which a part of a falling wall ought to have to deſerve the name of fluid; firſt, he ſhould have clearly proved, that fluidity belongs to any one ſingle part of matter, how minute ſoever, and not rather to an aggregate of particles. And next I ſay thoſe corpuſcles, that compoſe a fluid body may be of ſeveral ſizes, as thoſe of water, oil, and quickſilver, provided they be little enough to be put into the agitation, requiſite to give the aggregate they make up, the qualities, that are wont to denominate bodies fluid; and it is no more requiſite for me to define preciſely the magnitudes of the parts of a fluid body, than for Mr. *Hobbes* in his definition above-recited to define (which he will not eaſily do) what preciſe degree of endeavour muſt be ſignified by that very weak endeavour, by which if the parts of a body can be ſeparated from one another, he thinks fit to call them fluid. But though I thought it not amiſs to make theſe animadverſions upon Mr. *Hobbes*'s ratiocination, yet as to the opinion itſelf, for whoſe ſake he ſpeaks ſo ſeverely and ſo deſpairingly of our ſociety, if it be conſidered as I propoſed it, he ſhews me as yet no cauſe at all to renounce it. For that which I taught is this, that if a ſolid body be reduced into parts minute enough, thoſe ſolid corpuſcles, whilſt they are put into a convenient motion, may become parts of a fluid body. And againſt this Mr. *Hobbes*'s indignation ſeems ſtronger than his argument: for that which he objects being, as we have lately ſeen, that at this rate all bodies muſt be fluid; it is evident by what I have already argued, that he infers this abſurdity not from my opinion, but his own miſtake of it: nor did I content myſelf with the proofleſs propoſal of my conjecture, but I delivered, in ſeveral parts of the often mentioned hiſtory, particular experiments to evince what I taught; as that a conſiſtent coagulum of pure ſpirits of urine and wine may, by bare digeſtion, be turned into a permanent liquor; and that the fluid body of quickſilver may, without any ſenſible addition, be turned into a permanent dry powder, and may again in a trice by bare heat be turned into a laſting fluid body. Whereto I added other experiments, which together with theſe Mr. *Hobbes* would poſſibly have thought fit to anſwer, if he had found it eaſy for him to do ſo.

After this paſſage extant in Mr. *Hobbes*'s fifth page, that I have all this while been examining, I remember nothing in his dialogue, that requires to be inſiſted on about fluidity and firmneſs, till we come to the 29th page; where, having aſked what cauſe the academians aſſign of hardneſs, it is anſwered, that ſome of them aſſign three: to which Mr. *Hobbes* ſo far agrees as to ſay, *Quin corpuſcula (qualia ſunt* Page 29. *atomi, quas ſupponit* Lucretius *atque etiam* Hobbius*) jam ante dura facile poſſint ab aliqua dictarum cauſarum compingi, ita ut totum ex illis factum durum fiat, dubitandum non eſt:* ‘But the corpuſcles (ſuch as are the atoms ſuppoſed by *Lucretius* and alſo by Mr. *Hobbes*) being hard before, might be eaſily compacted by any of the mentioned cauſes; ſo that it is not to be doubted, but that the whole to be made of theſe corpuſcles will be hard.’ But then he would have us aſſign the cauſe of that he calls *durum primum.* But after ſome diſcourſe, wherein he is pleaſed to approve an objection of mine againſt ſome learned men, that aſcribe all coheſion of bodies to a certain glue, he anſwers himſelf the objection he frames againſt my doctrine about hardneſs, and thereby allowed me to proceed to what he further preſſes in theſe words; *Si dura ex primis duris fieri dicant, quare non & fluida fieri putant ex primis fluidis?* Page 30. *An creari fluida maxima potuere, ut æther, minima non potuere? Qui corpuſculum durum aut fluidum primus fecit, potuit, ſi libuiſſet, illud feciſſe tum majus, tum minus quocunque corpore dato. Quod ſi fluidum fiat ex non fluidis, ut vos dicitis, & durum ex duris tantum, nonne ſequitur ex fluidis primis, neque fluidum fieri neque durum?* ‘If hard bodies are made out of parts originally hard, why are not fluid bodies made of parts originally fluid? Could great fluids, as the æther, be created; and could not ſmall ones? He that firſt made a body hard or fluid, could, if he would, have made it greater or leſs than any other propoſed body. Now if a fluid body be made of parts not fluid (as you ſpeak) and hard bodies only from hard parts; doth it not follow, that nothing neither fluid nor hard is made of original fluids?’ But againſt this paſſage I have divers things to repreſent. For, firſt (not now to mention, that it may be queſtioned with what propriety one part of matter more than another may be called *primum durum*) he ſhould have told us what he means by his *prima fluida*, and how he proves that there are any ſuch; which ſince he has not done, it will be at leaſt as hard for a conſidering man to acquieſce in his queſtion as to anſwer it. For my part, I know no fluid body upon whoſe account, as of an ingredient, all others are fluid. And I think, it will be hard for Mr. *Hobbes* to ſhew, that water, quickſilver, and purely-rectified chymical oils (to name now no other liquors) do conſiſt of ſuch *fluida prima* as he teaches, whereto they owe all their fluidity. And it is plain by ſeveral experiments delivered in our hiſtory, and even by the obvious changes of water and ice into one another, that it is the motion, reſt, and the texture of the corpuſcles, which compoſe a body, that make it firm or fluid. As for what Mr. *Hobbes* demands, whether the ſmalleſt fluids imaginable could not as well have been created as the æther, it proves nothing againſt me, the queſtion not being, what might have been made, but what is ſo. And he ſhould have anſwered the arguments I * allege to make it improbable that, a fluid body is, as he would perſuade us in his book † *De corpore*, always diviſible into bodies equally fluid, as quantity into quantities. It is true, he there tells us that, though many others do not, ‘ he underſtands by ‘ fluidity, that which is made ſuch by nature equally in every part of the fluid body, ‘ in ſuch manner, as water ſeems fluid, and to divide itſelf into parts perpetually ‘ fluid.’ But whether others will take this for a clear notion of fluidity, I think may well be doubted; and he ſhould not barely ſay, but prove (which I think he will find hard to do) that the corpuſcles of water divide themſelves ſo as he teaches, ſince we ſee, that not only they cannot penetrate glaſs, but are unable to be driven in at the pores of more open bodies, which other liquors eaſily pierce into. And, laſtly, as to Mr. *Hobbes*'s queſtion, *Quod ſi fluidum fiat, &c.* it is eaſy to foreſee, what, according to my doctrine, I may anſwer: for, not to mention that the argumentation is invalid, unleſs by *fluidum*, he mean *omne fluidum*, I reply, that till he have explained what he means by his *fluida prima*, and proved that there are ſuch, the queſtion needs no anſwer. Beſides that, whatever he, upon miſtakes, ſtrives to infer, my doctrine is ſo far from affirming, that there are many parts of matter, of which neither fluid nor hard bodies can be made; that I teach, as we have lately ſeen, that there are multitudes of parts, that may, by being reduced to a ſufficient ſmallneſs, and put into a convenient motion, or by being brought to a mutual contact and reſt, be made to conſtitute either a fluid body or a firm one: as may be exemplified in the formerly-mentioned inſtances of two ſubtile liquors, that immediately compoſed a conſiſtent body; and of quickſilver, which without additaments was made ſometimes a powder, *Page* 31. and ſometimes a liquor. What Mr. *Hobbes* adds in the next page about the difficulty of explaining the diaphaneity of glaſs or cryſtal, in caſe they conſiſted of corpuſcles hard and implicated, or having their pores in any way whatſoever diſjoined, I muſt not now inſiſt on; ſince beſides that ſuch a diſquiſition would require almoſt a volume, the true and general cauſe of tranſparency in bodies is, in my poor opinion, one of the abſtruſeſt things in natural philoſophy; and Mr. *Hobbes*'s explication of it (though none of his worſt conceits) has, for aught I can find, fallen ſhort of ſatisfying the curious, as well as thoſe of other men have done. But to me, that have not taken upon me to write elements of philoſophy, it is enough, that I have, by competent experiments, and other proofs, confirmed the truth of my doctrine about the cauſe of firmneſs; though I attempt not to explicate the other qualities of the ſame bodies, whoſe explication my undertaking does not exact. Wherefore I hope I may now haſten to conclude this Appendix, with ſpending a few words on the notion of fluidity and firmneſs Mr. *Hobbes* would ſubſtitute inſtead of mine. For, having now (perhaps but too prolixly) examined what he has been pleaſed to object againſt my doctrine, I ſhall not need ſpend time to vindicate the experiments and conſiderations whereon I built it; Mr. *Hobbes*, for reaſons beſt known to himſelf, not having thought fit to take notice of them.

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* Hiſtory of Firmneſs, page 227. and elſewhere.

† Chap. 16. ſect. 4.

Mr. *Hobbes*'s theory of fluidity and firmneſs is thus delivered by himſelf;

B. *Quænam duri & fluidi ſunt principia?* Page 30.

A. *Quid aliud niſi fluidi quidem quies, duri autem motus quidam ad illum effectum producendum idoneus? Per quietem intelligo duarum partium inter ſe quietem, cum ſe mutuo tangunt quidem, ſed non premunt: nam & fluida moveri tota poſſunt retenta fluiditate, & dura quieſcere, ut tamen partes eorum moveantur.*

‘ *B.* What are the principles of fluidity and firmneſs?

‘ *A.* Of fluidity, nothing but reſt; of firmneſs, motion; ſuch as is fit to produce that effect. By reſt I underſtand the reſt of two parts one with another, when they each touch, but neither preſs one another. For entire bodies of fluids may be in motion, their fluidity abiding, and hard bodies be at reſt, although their parts be in motion.’

I doubt not, but this will to moſt readers ſeem a paradox. And as for his ratiocination contained in the two laſt lines, I ſhall readily allow him, that *fluida moveri tota poſſunt retenta fluiditate*; ſince that I think agrees at leaſt as well with my hypotheſis as his: but whereas he adds, that hard bodies may reſt, and yet their parts be moved, that may in one caſe be conceived, and in another not. For indeed the implicated parts of a firm body may be made to tremble, or a little vibrate as it were to and fro, as thoſe of a ſounding bell do, or as in a hedge, the branches and twigs may be ſhaken by the wind, whilſt the trees and buſhes themſelves continue rooted in the ground. But that, in a body, the conſtituent corpuſcles ſhould all, or moſt of them, be moved quite out of their places, in reſpect of one another, as was lately ſhewn to happen in fluids, and yet the body continue hard, is more requiſite than eaſy to be proved.

But Mr. *Hobbes* contents himſelf to allege, in favour of his ſtrange notion touching fluidity and firmneſs, three particulars, which, I confeſs, afford me not the leaſt ſatisfaction.

The firſt is drawn from what he formerly taught touching the ſwift motion of the air in our cylinder; which example (as he calls it) having repeated, he adds, *Atque* Page 30. *hinc manifeſtum eſt vehementem eſſe in aëre ita moto & clauſo compreſſionem, quantam ſcilicet efficere poteſt vis illa qua incuſſus erat; atque etiam à tanta compreſſione aliquem gradum conſiſtentiæ fieri, quanquam conſiſtentia aquæ minorem. Quod ſi eſſet in iiſdem particulis aëris omnibus, præter motum illum, quo altera alteram premit, motus ille circularis ſimplex, iſque ſatis vehemens, impoſſibile fere eſſet unam earum à ſuo circello dimoveri, quin, reliquis particulis reſiſtentibus, totus ſimul premeretur, id eſt, totum durum eſſet: durum enim eſt totum illud, cujus nulla cedit pars niſi cedente toto. Vides ergo poſſe fieri duritiem in fluidiſſimo aëre per motum hunc circularem ſimplicem particularem, quibus duo motus contrarii ante dederant vertiginem.* ‘ And hence it is manifeſt, that there is a great compreſſion in the air ſo moved and ſhut up; namely, ſo great as that force, by which it was driven in, was able to make; and alſo, that from ſo great compreſſion ſome degree of conſiſtence muſt be made, though leſs than that of the conſiſtence of water. Now, if in all the ſame particles of air, beſides the motion, by which one preſſes another, there was alſo the ſimple circular motion, and that vehement enough, it would be almoſt impoſſible any one of them ſhould be moved from its little circle, but that the other particles reſiſting, the whole would be preſſed together, that is, become hard; for that is hard of which no part gives place but upon the motion of the whole. You ſee therefore that hardneſs may be made in a moſt fluid body by this

‘ ſimple circular motion of particles, which was before imparted to them by two ‘ contrary motions.’

But, I confeſs, I do not ſee, how the *motus circularis ſimplex* he talks of ſhould give ſuch a hardneſs to the fluid air: nor is it manifeſt to me, how the air, that perfectly fills the cylinder, can be by motion compreſt, eſpecially ſo far as to obtain thereby a degree of conſiſtence fit to be mentioned, as he ſpeaks of it, when (without adding the word *much*, or any other equivalent term) he ſays, that yet it is *leſs than the conſiſtence of water*. For the cylinder being, according to him, perfectly full of air, I ſee not how the pumping can make the cavity (to uſe his own expreſſion elſewhere) *fuller than full*; nor conſequently can compreſs air to a conſiſtence any thing near that of water, without penetration of dimenſions. But theſe things were mentioned only *ex abundanti*, for the violent motion itſelf of the air in the cylinder (which motion the argument ſuppoſes) has been already, in the examen of one of the former paſſages of his Dialogue, ſufficiently manifeſted to be contrary to experience.

The ſecond thing Mr. *Hobbes* alleges is his conceit of the generation of fleſh within the muſcles of the human body. But beſides that, he takes for granted two or three things, which many learned anatomiſts and phyſicians, even among the moderns, will ſcarce allow him, and which he does not prove: beſides this, I ſay, (which I may elſewhere have occaſion to conſider further) the account he gives of the generation of fleſh from theſe ſuppoſitions, is far from being evidently enough deduced to vie for clearneſs with many of thoſe experiments, which I have alleged in favour of the opinion he oppoſes. And whereas he adds, Page 31. *Atque talis quidem eſſe poteſt cauſa efficiens duri primi*; *duri autem ſecundi, id eſt, duri à cohæſione durorum primorum, cauſa poteſt eſſe motus ille idem circularis ſimplex conjunctus cum contactu eorundem ſuperficiali, vel etiam intricatione:* ‘ And ſuch indeed may be the cauſe of the *durum* ‘ *primum*, or firſt hard body. But of the ſecond, that is, of the coheſion of the two ‘ of theſe firſt hard bodies, the cauſe may be the very ſame circular motion, conjoined with their ſuperficial contact, or perhaps their being one with another intricated:’ Not again to ſay any thing to his *durum primum*, I confeſs I do not ſee how the *motus circularis ſimplex* ſhould need to be ſuperadded to the contact or intrication of the cohering firm corpuſcles, to procure a coheſion, to which it is needleſs, and which, in divers caſes, may be rather hindered than furthered by it.

The third thing, that Mr. *Hobbes* alleges, is not ſo much a confirmation of his own doctrine, as an objection, as he ſeems to think, againſt that of his adverſaries. Page 31. For, *Si ſupponamus* (ſays he preſently after his laſt-recited words) *cum illis, duritiei cauſam eſſe magnitudinem aut craſſitiem partium, quam rationem reddere poterimus, cur durior vel firmior ſit aqua congelata, quam eſt eadem aqua ante congelationem?* ‘ If we ‘ ſuppoſe with them, that the cauſe of hardneſs is the greatneſs or thickneſs of the ‘ parts, what reaſon can we give, why congealed water ſhould be harder or firmer, ‘ than the ſame water is before ſuch congelation?’ But it may eaſily be replied, that we make not the bigneſs or groſſneſs of the parts of a body the only or chief cauſe of its hardneſs, but their reſt by one another, which the parts of frozen water have; whereas thoſe of unfrozen water have it not, but are in a ſtate of fluidity, and conſequently not of firmneſs. Which may be illuſtrated by what I † elſewhere relate of pure oil of aniſeeds, and a ſubſtance I diſtilled out of benzoin, both which bodies were ſometimes fluid and ſometimes conſiſtent, as the greater or leſſer warmth of the air kept their parts in a due motion, or ſuffered them to reſt.

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† In the Hiſtory of Fluidity and Firmneſs.

But in exchange of theſe few and unconcluding arguments, which are all that Mr. *Hobbes* alleges to countenance his paradox, how many experiments and reaſons might we tranſcribe out of our Hiſtory of Fluidity and Firmneſs, in favour of the contrary truth?

And as critical as Mr. *Hobbes* appears in laying down the requiſites of a good hypotheſis, I muſt make bold to the two conditions he mentions (page 11.) *ut ſit conceptibilis (id eſt, non abſurda)* which, whether it be enough, I now diſpute not; *& ut ab ea conceſſa inferri poſſit phænomeni neceſſitas*; ‘ that it be conceivable, that is, ‘ not abſurd; and that from its being granted, the neceſſity of the phænomenon ‘ may be inferred;’ to add a third, namely, that it be not inconſiſtent with any other truth or phænomenon of nature. Which third condition whether divers of Mr. *Hobbes*'s hypotheſis (which himſelf in this place calls *mirandæ)* do not want, we have in part already conſidered in the treatiſe, to which this is an Appendix; and (as I newly intimated) I might further ſhew, as to his notion of fluidity and firmneſs, if I would here repeat all the experiments mentioned in my hiſtory of them, though they be not all that I have made ready to the ſame purpoſe againſt another opportunity: but partly wearineſs, and partly a natural unwillingneſs to repeat, induces me rather to refer my reader thither. Which when I do, I do not forget, that Mr. *Hobbes* appears offended at me and others for troubling ourſelves to make unobvious experiments. But that I may not repeat what, in divers treatiſes, I repreſent concerning the uſefulneſs of ſuch experiments, I ſhall now only oppoſe to the authority of Mr. *Hobbes* in this Dialogue (wherein he has been pleaſed to chuſe thoſe he calls the *Experimentarian Philoſophers* for his adverſaries) the authority and reaſon of the ſame Mr. *Hobbes* in another Dialogue (publiſhed but the year before) where one of his two diſcourſers having ſaid, *Qui corpora corporibus admovendo, nova & mirabilia oſtendunt naturæ opera, mirum in modum incendunt animos hominum amore philoſophiæ, & ad cauſas inveſtigandas non parum inſtigant, eoque nomine laude digni ſunt:* ‘ They who, putting bodies to bodies, ſhew the new and admirable works of nature, ‘ do wonderfully inflame the minds of men with the love of philoſophy, and do ‘ not a little inſtigate them unto the ſearch of cauſes, and on that account are worthy ‘ of commendation:’ The other confirms it, by adding, *Ita eſt; nam hiſtoriam naturalem (ſine qua ſcientia naturalis fruſtra quæritur) locupletant:* ‘ True; for they enrich natural hiſtory, without which natural ſcience is in vain ſought for.’ And howſoever, Mr. *Hobbes* needed not have recourſe to ſuch experiments, as he would be thought to diſapprove (I mean elaborate ones) to diſcern, that his notions do not over-well agree with the phenomena. For, if there be not a various motion in the ſmall parts of water and ſuch liquors, whence it is, that a lump of common ſalt being thrown into a pot of water, is there diſſolved into minute bodies, whereof many are carried to the very top of the water, and are ſo exquiſitely diffuſed and mingled with the liquor, that each leaſt drop of it contains numbers of ſaline corpuſcles? And if motion be the cauſe rather of hardneſs than fluidity, how comes it to paſs, that in froſty weather ice is by heat (which Mr. *Hobbes* will not deny to be motion, or an effect of it) turned from a hard to a fluid body? and that metals, as gold and ſilver, *&c.* whilſt they are cold, or expoſed but to a moderate heat, are firm and conſiſtent bodies; and by a violent heat, which does manifeſtly give their parts a various and vehement agitation (as appears by their ſudden diſſipating of ſpittle, greaſe, and far more ſtable bodies, caſt upon them, into ſmoak) are put into a fluidity; which, upon their removal from the fire, they quickly exchange for firmneſs?

Mr. *Hobbes in examinat. & emendatione, &c. Dialog.* 6.

BUT ſince the want of more to ſay would not, in haſte, put a perlod to this diſcourſe, I am content to let my haſte break it off; eſpecially ſince after I have thus examined what Mr. *Hobbes* teaches concerning fluidity and firmneſs, either here, or in that ſection of his Elements, where he pretends to define them, I think I need not fear, that a doctrine, which I have perhaps with ſome care endeavoured to eſtabliſh, for the main, upon experiments, ſhould be overthrown by opinions, whoſe grounds are but ſuch as we have already ſeen; and in pleading for which, the author is pleaſed not only to leave almoſt all my arguments untouched, but not ſo much as to offer at explicating, by his principles, any of thoſe numerous and important phænomena of fluidity and firmneſs, delivered in the treatiſe he oppoſes.

AND now leaving Mr. *Hobbes*, to apply myſelf to the reader, I have, to the things hitherto diſcourſed, but this one thing to add concerning them; that as little cauſe as Mr. *Hobbes* has hitherto given me, to diſtruſt what I have written of fluidity and firmneſs; yet I am not now more confident of my conjectures than I was, when, toward the end of the preface to the hiſtory of theſe two qualities, I ſpoke diffidently enough of the theorical part of that treatiſe. And I freely confeſs, that the great difficulty of things, and the little abilities I find myſelf furniſhed with to ſurmount it, do often, in general, beget in me a *great* diſtruſt even of things, whereof my adverſary's objections give me not *any*.

SOME
MOTIVES and INCENTIVES
TO THE
LOVE of GOD,
Pathetically diſcourſed of, in a LETTER to a FRIEND.

To the COUNTESS of *WARWICK*.

MY DEAR SISTER,

I EXPECT you ſhould ſomewhat wonder, that after having for above eleven years been careful to keep this following letter from the public view, and that too notwithſtanding the ſolicitation (not to ſay importunity) of divers illuſtrious perſons, and even your commands to releaſe it from its confinement; I ſhould now at length give way to its paſſing abroad into the world, and its making you a public and ſolemn addreſs. Wherefore judging myſelf obliged to give you an account of a paper, for which you have been long pleaſed ſo highly and ſo obligingly to concern yourſelf, I muſt, to remove your wonder, inform you, that I am reduced to this publication in my own defence. For, whilſt I was far from dreaming of permitting this epiſtle to paſs out of my cloſet, it happened, that a broken copy of it did (by I know not what misfortune for me) fall into the hands of a neceſſitous perſon, who would needs perſuade himſelf, that by printing it, he might relieve ſome of his preſent wants; and thereupon proffered to ſell the copy for a ſum of money. But my good fortune leading him to a ſtationer, to whom my name was not unknown, he very civilly ſent me forthwith notice of the propoſition that was made him, and after came himſelf to acquaint me, that the copy, about which he had been treated with, being but one of two or three, that were then abroad, ſome or other of them would, queſtionleſs, ſoon find the way to the preſs. This unwelcome accident did little leſs trouble than ſurprize me; for, beſides that it impoſed on me the neceſſity of a publication I had ſo long declined; and beſides that I knew, that * compoſures of a very differing nature being expected from me, the appearing of this, inſtead of them, would make this unwelcome to many, though it had fewer imperfections than it has: beſides theſe things (I ſay) my ſight was then, and is ſtill, ſo impaired by a diſtemper in my eyes; and the hours I could diſpoſe of were ſo pre-ingaged to philoſophical themes that I could not promiſe myſelf ſo much as to read it it over before its going to the printer. But conſidering after all this, that the copy I had by me was like to prove more full, and leſs faulty, than any of thoſe, that ſome endeavoured to obtrude upon the world, I thought it leſs inconvenient to venture mine own abroad, than to run the hazard of a ſurreptitious edition of a diſcourſe, that could ſo ill bear the appearing with any other blemiſhes, than thoſe it brought with it into the world from my pen. And therefore having put it into the hands of perſons, whoſe eminent abilities the more knowing part of the nation has long and juſtly admired, and begged their impartial opinion and caſtigations of it (giving them entire liberty to alter or expunge whatever they diſliked;) and finding (by haſtily turning over the leaves) that their caſtigations were only enough to let me ſee, that they had heedfully read it over; and were accompanied with encouragements, which care was taken to keep me from looking upon as meer compliments: I let it go to the preſs, without ſo much as having once peruſed it, or heard it read over, ſince the ſtationer firſt gave me the advertiſement I told you of. Which I made the leſs ſcruple to do, becauſe a critic, whoſe judgment and piety I much reverence, ſeriouſly endeavoured to perſuade me, that I ought not in conſcience to decline publiſhing what he was pleaſed to think proper to kindle or cheriſh the flames of divine love in the breaſts of the readers. And my haſte itſelf did afterwards promiſe me theſe advantages, that notwithſtanding my book's not coming forth ſooner, I ſhould not loſe the excuſe of youth I had when it was written; nay, and that the faulty paſſages, which may be met with in it, will perhaps be charged upon thoſe, that ſuffered them to paſs uncorrected, when they had ſo abſolute a power to expunge or reform them. Theſe, and the like motives, having induced me to conſent to the publication of the following letter, I needed not deliberate long, to whom I ſhould addreſs it. For, ſince that accompliſhed *Lindamor*, whom it ſo much concerned, has left the world, there is no perſon in it, to whom this addreſs is any thing near ſo due, as unto you, dear ſiſter. It was at that delicious *Leeze*, where you are now the miſtreſs, that this letter was written; and it was of you that I borrowed thoſe hours I ſpent in writing it. It was to you, that I ſhewed it almoſt ſheet by ſheet, before I reſolved to ſend it away. It is you, that can beſt excuſe the imperfections of it, as knowing not only the more obvious, but the more private avocations and other diſadvantages, among which it was penned. It is you, that have ever ſince ſollicited me to divulge it, and have given me the greateſt encouragements to do ſo, not only by thoſe ſollicitations, which implied your own favourable opinion of it, but by procuring me (by concealing or diſguiſing my name) the unſuſpected opinions of divers competent judges. In a word, this addreſs belongs upon ſo many ſcores to you, that I could not make it any where elſe, without manifeſtly wronging you. I know, dear Siſter, that it is not uſual to dedicate books to ſo near a relation; and that it is uſual, in dedicatory epiſtles, both to depreciate what one has written, and to extol the perſon that one writes to, eſpecially if it be one of your ſex and quality. But you know too, that I never ſwore allegiance to cuſtom, and therefore will not, I ſuppoſe, wonder to ſee me as little ſolicitous to conform to it on this occaſion as on others. In an age, when ſo few perſons have merit enough to keep that from being flattery, which ſhould be but praiſe, I am not at all ambitious of caſting myſelf upon the unhappy neceſſity, of either flattering, or appearing rude; when by better chuſing the objects of my addreſſes, I can, as occaſion requires, give praiſes without untruth, or forbear them without incivility. Nor dare I preſume, that a lye ceaſes to be a fault, by being put into a dedicatory epiſtle; as antiently the hurtful beaſts forgot their pernicious nature, when brought into the ark. Not to mention, that books of devotion have generally the ſtrange and unhappy fate of being leſs welcome to them, that moſt, than to them, that leſs need them. As for the diſcourſe I preſent you, though my opinion of it may be gueſſed at, by the privacy, to which I have ſo long confined it; yet becauſe, as in phyſic, to have a good opinion of the medicine, and the doctor, ſo in books of devotion (which are a kind of phyſic for the mind) to have one of the compoſure, or of the writer, both inclines us to look after the preſcriptions, and advantages their operations: for this reaſon I ſhall ſay nothing to diſparage a diſcourſe, which is of a nature to aim more at the being practiſed by the readers, than the being pardoned; and ſhall not at all repine to ſee it find, from the public, as favourable a reception, as it has hitherto met with in cloſets. And now, dear Siſter, though I know you will not, yet I doubt, my other readers will expect, that this letter ſhould, according to the mode of epiſtles dedicatory, be concluded with commendations of you, and compliments to you; it being almoſt as much out of faſhion in ſuch addreſſes to omit giving praiſes, as it is to believe the praiſes given on ſuch occaſion. But, though I can praiſe you without either diſbelieving myſelf, or fearing to be diſbelieved by any that knows you; yet, beſides that our relation, and our friendſhip, would make ſuch a way of writing miſbecoming me on all occaſions; I know your piety and your modeſty would peculiarly diſallow it upon this; where the ſubject I am to entertain you with is of ſuch a nature, as would make a flaunting addreſs but a very unſuitable introduction to it. The nature of my theme, as well as the ſtrictneſs of your virtue, and our friendſhip, forbidding me here to celebrate you, otherwiſe than by letting the world ſee, that I dare, even in a dedicatory addreſs, without fear of diſpleaſing you, forbear to celebrate you. And if it be demanded, why then I refrain not likewiſe from writing to you a dedication? I ſhall anſwer, that I am unwilling to loſe this opportunity of making a public acknowledgment, that as I have the honour to be your brother, and your friend, ſo I have the juſtice to think it very much my happineſs to be ſo: eſpecially, ſince you are pleaſed to aſſure me, that my relation to you has rather been the occaſion than the ground of a friendſhip, that does, whilſt it enjoins me to profeſs myſelf without compliment, oblige me to be, with all the ſincerity that is poſſible,

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* Some Treatiſes relating to Experimental Philoſophy.

My dear Siſter,

Your moſt affectionate Brother, and

Moſt faithful, humble Servant,

ROBERT BOYLE.

Advertiſements to the Reader;

(As they were written ſome years ſince, when ſome intelligent perſons were to have a ſight of the following Diſcourſe.)

TO prevent thoſe ſcruples, which elſe might ariſe in the peruſal of the enſuing diſcourſe, the reader of it is requeſted to take notice.

I. That though ignorance hath been of late ſo much in faſhion with too many of our young nobility, that ſome paſſages of the enſuing papers may ſeem unfit to have been written to a young gentlemen; yet the perſon they were addreſſed to, being both a traveller, a linguiſt, and a ſcholar, it was not improper to diſcourſe with him at the rate of ſuppoſing him ſo qualified.

II. That the enſuing letter was not written ſingle, being but the laſt of divers, wherein love in general was confeſſed, juſtified, and celebrated; wherein the received way of making love was explicated, defended, and expoſed; wherein conſtancy and inconſtancy in love were argued for and againſt; wherein Platonic love was explicated, celebrated, and wherein the cure of love was propoſed and proſecuted; wherein (to diſpatch) the controverted points concerning love were diſcourſed of, in a way ſuited to the ſeveral themes, and to the humours and principles of the ſuppoſed writers. But all the former papers (written in compliment to a fair lady) though very free from the guilt of either licentiouſneſs or profaneneſs, have been by the author judged too little ſerious, either to appear alone, or to accompany the enſuing diſcourſe, wherein he has expreſſed his own opinions, as in the former he but deduced thoſe of imaginary perſons.

III. That the following paper was to have been attended by another, wherein the properties and duties of ſeraphic love were to have been explicated and inſiſted on; but the author, before he had written that other, accidentally meeting with a French book, wherein though what he had already diſcourſed of were not handled, he found, that much of what he had deſigned to ſet down was anticipated, and the moſt pertinent conſiderations his intended themes ſuggeſted, were ſkilfully diſplayed already; he feared it might be injurious to his reader, and himſelf, if he ſhould either turn a tranſcriber of others notions, or decline the beſt thoughts belonging to his theme, that the leſs good ones might paſs without ſuſpicion for his own.

L'Amour Divin, de Mr. du Moulin.

IV. That the enſuing trifle having been written in haſte, and by ſnatches, amongſt diſtracting avocations, and far from literary accommodations, is freely ſubmitted to the cenſure of learned and impartial peruſers, eſpecially divines; who are by the author requeſted to believe its miſtaken paſſages, and unwary expreſſions (if it contain any ſuch) to have been altogether unintentional, and ſuch as they ſhall never find it to be more than one labour to make him diſcern and retract; he being reſolved, by God's aſſiſtance, to continue ſtill obſequious enough to all clearer diſcoveries of truth, to be able to ſay, *Errare poſſum, hæreticus eſſe nolo.* And as he is on his part ſo reſigned, and ſo willing to be rectified, ſo he expects this equity from thoſe, to whoſe judgments he ſubmits, that, conſidering he intended not to write a ſolemn and regular treatiſe, but a private and familiar epiſtle, they will paſs by ſuch unaccurateneſſes, as are wont to be incident to compoſers of this later nature. He preſumed he might ſometimes make bold with the vulgar laws of tenſes, treating of an eternity, that

that admits them not. He hoped, that the freedom taken in ſome of his expreſſions concerning love, and the wonted objects of it, would be excuſed, by its being exacted by the deſign he had upon *Lindamor*, though he had not (which yet he has) otherwiſe accounted for it*. He intends not to adopt thoſe (too often erroneous) traditions of the vulgar, whence he ſometimes draws compariſons, that he may not deny himſelf the liberty taken by the generality of writers (without excepting the learneder ſort of them) who ſcruple not in popular compoſures to make ſimilies and alluſions grounded on popular traditions and perſuaſions. As for thoſe common places, which the method of our author's diſcourſe allowed him not to decline, he hopes the reader will diſcern by his way of writing, that he loves to uſe them, not as travellers, but as hunters uſe the highways: for the traveller, when he once gets into the road, makes it his buſineſs to keep in it; whereas the hunter, if he meets one lying in his way, is not fond of ſtaying in it, but nimbly croſſes it, or traverſes ſo much of it, as is requiſite to be paſt through in the purſuit of his game. And laſtly, if in ſome few paſſages of the following letter (eſpecially about the beginning of it) the expreſſions are leſs grave and ſolemn than the deſign; the author hopes, that even they will yet appear tolerably ſerious, for the airieſt parts of a letter, written by one young gentleman to another.

SOME

MOTIVES

TO THE

LOVE of GOD.

MY DEAREST LINDAMOR,

I AM very much delighted to learn, both by the voice of fame and the information of much more credible relators, that *Hermione*'s cold uſage has cured you of the fever her ſcorching eyes had given you: and that when once you found yourſelf reſerved to ſhew, what wonder her eyes were able to perform, you ſeaſonably reſolved to become an inſtance of the power, rather of reaſon, than of love; and accordingly, did yourſelf the right to fruſtrate the vain hopes, your inſulting miſtreſs cheriſhed to manifeſt in you, that her charms were capable to make your flame perſevere, when her change had made it as well cauſeleſs as hopeleſs. I could wiſh indeed for your ſake, that you owed your cure more entirely to your reaſon, and leſs to your reſentment; that the extraction of your freedom may no ways blemiſh it. But ſince unallayed ſatisfactions are joys too heavenly, to fall to many men's ſhares on earth, I

* In a dedication of all the forementioned letters to my Lady *T. J.*

cannot but conclude, that your recovery, even on theſe terms, deſerves I ſhould congratulate it; for the French ſay truly, that *les plus courtes folies ſont les meilleures.* And liberty being too high a bleſſing to be diveſtible of that nature by circumſtances; I (that ſeldom deplore him, who by loſing his miſtreſs recovers himſelf) think, that *Hermione* has but intentionally, not eventually diſobliged you; and hath made your flames a better return, by reſtoring you your own heart, than ſhe could have done by exchanging hers for it. But that which not leaſt endears to me your recovery, is, that I am aſſured by perſons, from whom I dare credit even ſo welcome news, that my endeavours proved ſo happy as to be conducive to it, and that the conſiderations I ventured to preſent you, did at leaſt ſo far contribute to your freedom, as to give you the deſire and the deſign of regaining it. For, I hope, I need not tell you, that I ſeldom uſe endeavours, whoſe proſperouſneſs is more welcome to me, than thoſe that aſpire to ſerve *Lindamor*. And though I cannot ever pay you any great ſervices, in relation to my vaſtly greater deſires, yet I can ſcarce do you little ones, in relation to the delight reſulting from the having done you any. Nor has the joy, which this ſucceſs of my diſcourſes brings me, been ſparingly increaſed by my having ventured them with much more deſire, than expectation, of their proſpering; and leſs out of any ſtrong hope they would ſucceed, than out of an unwillingneſs to leave the means, I thought leaſt improbable, uneſſayed; being invited to excite you to greater hopes than I durſt allow myſelf for you, by the example of generals, who, whatſoever diſtruſtful thoughts they harbour in their breaſts, ſuppoſe, that (before the battle) to make their ſoldiers fight ſucceſsfully, it is as well conducive as requiſite, by encouraging orations, to make them think they ſhall do ſo. For, although I endeavoured indeed to perſuade you, that reaſon being born ſovereign of the paſſions; though her lenity or ſupineneſs do ſometimes both occaſion and permit their uſurpations, ſhe is ſeldom ſo diveſted of her native power, but that whenſoever ſhe pleaſeth to employ what ſhe hath left, ſhe is able to reſume what ſhe hath loſt. And though I was willing you ſhould believe, that to perfect what your reſentment had begun, was a taſk ſo eaſy, that the victory was as much in your power, as the reſolution of attempting it; yet, notwithſtanding all this, I ſay, I was once half perſuaded, that to undertake the curing of a lover was the next weakneſs to the being one. And, *Lindamor*, to deal ingenuouſly with you, your recovery hath circumſtances in it, that make me very apprehenſive, that you are not yet out of the danger of a relapſe, and that you have not half ſo abſolutely abandoned your former amorous conſtitution of mind, as the former idol of it. I know, that from a perſon, who, for one that hath never yet been hurt by *Cupid* is accuſed of uſing him ſlightly and ſeverely enough, you will expect endeavours to preſerve you from relapſes, by ſuch diſſuaſions from love, as its votaries will ſcarce vouchſafe ſo mild a title to, as that of invectives againſt it. And I ſhall ingenuouſly acknowledge, *Lindamor*, that I have been ſometimes no very unready ſatiriſt on that theme; and with a pen, reliſhing of the liberty I cheriſhed in my heart, endeavoured to diſabuſe thoſe ſervile ſouls, that being born to reaſon, ſo far degraded themſelves, as to boaſt ſolely an exceſs of paſſion; and had ſuch low and narrow thoughts of felicity and miſery, as to expect either from a woman's uſage. All which I thought I might the freelier do, becauſe having never known the infelicities of love, but in the ſufferings of others, I might probably ſuppoſe, that my declamations againſt it would paſs for the productions of my reaſon, not my revenge. But, *Lindamor*, though the extravagances of ſome men's folly have been ſometimes too great, to let me avoid laughing ſomewhat ſatirically at it; yet I am really too little an enemy to love, unleſs exceſſive or miſplaced, by indiſtinct and diſfiguring conſiderations, to repreſent to you the nobleſt paſſion of the mind, as its moſt hideous and formidable diſeaſe.

Sect. 1. To love even with ſome paſſionateneſs the perſon you would marry, is not only allowable but expedient, being almoſt neceſſary to the duty of fixing your affection, where you have once engaged your faith; for, it hath been obſerved, that love doth ſeldom ſuffer itſelf to be confined by other matches than thoſe of its own making. And few, but they, that are ſo wiſe as they ſee cauſe to be almoſt in love before-hand with thoſe they marry, prove ſo honeſt, as afterwards to be in love with none elſe. Since therefore the marriage of a wiſe man ſuppoſes at leaſt as high a degree of love, as he is capable to cheriſh without forfeiting that title; I can ſcarce diſallow the being moderately in love, without being injurious to marriage, which is a relation, that though I can with much leſs reluctancy permit others, than contract myſelf; yet dare I not abſolutely condemn a condition of life, as expedient to no man, without which, even Paradiſe and innocence were not ſufficient to complete the happineſs of the firſt man. Thus you ſee, *Lindamor*, that I do not promiſcuouſly quarrel with all ſorts of love, but endeavour only to poſſeſs you with this truth, that as anciently among the Jews there were odoriferous unguents, which it was neither unuſual nor unlawful to anoint themſelves with, or beſtow upon their friends; but there was a certain peculiar compoſition too of a precious ointment, which God having reſerved to be employed in his own ſervice, with that the perfuming of their friends was criminal and ſacrilegious: ſo there are regulated degrees of love, which it is not forbidden to harbour for a friend, a miſtreſs, or a wife; but there is too, a certain peculiar ſtrain, or (if I may ſo call it) heroic temperament of love, which, wherever it is found, makes it belong, as unalienable, as juſtly unto God. A virtuous wife may love both her huſband's relations and her own, and yet be truly ſaid to love him with her whole heart; becauſe there is a certain unrivalled degree of kindneſs, and a peculiar ſort of love, which conſtitutes true conjugal affection, that ſhe confines to him, and reſerves entirely for him, and would think it criminal to harbour for any other perſon: ſo a religious ſoul may obey the command, of loving God entirely, though ſhe allow her affections other objects; provided they be kept in a due ſubordination to, and kept from entering into competition with, that love, which ought to be appropriated to him; and which reſults chiefly from an either altogether, or almoſt unincreaſable elevation, and vaſtneſs of affection; from an entire reſignment to, and an abſolute dependence on, the loved party; from a reſtleſs diſquiet upon the leaſt ſenſe or doubt of her diſpleaſure; from a greater concern in her intereſts, than one's own; from an expectation of no leſs than felicity or wretchedneſs from her friendſhip or indignation; or at leaſt, a perſuadedneſs, that nothing can be a greater happineſs, than her favour, or deſerve the name of happineſs without it. For, wherever a paſſion has theſe properties, or any of them, conſpicuous in it; it cannot, but by being conſecrated to God, avoid becoming injurious both to him and to itſelf. The very nobleneſs of it entitles him to it; as in ſome kingdoms (and particularly here in *England*) though veins of coarſer metals may belong to the owner of the ſoil they grow in, yet all the mines of the more perfect metals (as gold and ſilver) are by the law made mines royal, and belong to the king, to whom their value appropriates them. By reflecting upon this peculiar notion of love, you may be pleaſed, *Lindamor*, to interpret ſuch indefinite expreſſions as you may meet with in the following diſcourſe. 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At leaſt if you be of the mind of that generous youth (to whoſe ſucceſsful valour the conquered world was both theatre and trophy) who refuſed to run at the Olympic games, becauſe there were no monarchs to run with him. But I fear, *Lindamor*, I have a little digreſſed; ſince I might have told you in fewer words, that it is not my deſign in this paper to declaim againſt love in general, or make a ſolemn harangue of the fickleneſs of women; and that therefore, as when young gallants (ſuch as you, *Lindamor*) are ſubject to caſt away their love upon unfit objects, their diſcreet friends (ſenſible of the truth of the Italian comic's obſervation, that

Oneſtà contra' amore
E troppo frale ſchermo
In giovinetto cuore;

In youthful hearts bare vertue's wont to prove
But a weak ſhield againſt the darts of love:)

without taking any more than neceſſary notice of their former fond and ſtraggling paſſions, reclaim them by either matching them, or at leaſt (in order thereunto) engaging their addreſſes to perſons, whoſe beauty or prerogatives may both legitimate or confine their affections: ſo I ſhall now endeavour to prevent the future gaddings of your love to objects, that cannot deſerve ſo tranſcendent and diſintereſted a one, as I have obſerved yours to have been; by preferring and engaging it to the true object, that paſſion was born to, the nobleſt it can aſpire to, and the moſt ſatisfying it can enjoy. Yes, *Lindamor*, as it has hitherto been my not-unproſperous taſk to unhood your ſoul, I ſhall now make it my buſineſs to ſhew her game to fly at. I ſee, that love in *Lindamor* is too noble and predominant an affection, to be either eaſy or fit to be deſtroyed. It will therefore be my deſign, not to ſuppreſs your flame, but to addreſs it. I wiſhed it withdrawn from *Hermione*, not to annihilate it, but to transfigure it. I would not have a paſſion, which wanted nothing, but a due object, to be ſeraphic love, like vulgar men, be ſwallowed up by death, the common fate; but be ennobled, by a deſtiny like that of *Enoch* and *Elias*, who, having ceaſed to converſe with mortals, died not, but were tranſlated into heaven.

Sect. 2. WHAT has been ſaid already, *Lindamor*, hath, it ſeems, ſufficed to rectify your love; by diſabuſing it, and ſhewing you, how unfitly it was placed on its former objects. Your proficiency in that invites me to proceed with you to a new leſſon, and (mindful of that true ſaying of an eminent father, *Nemo aliquem amat, quem non vult eſſe meliorem*) to endeavour to exalt your paſſion by directing and ſettling it upon an object, the due contemplation of whoſe lovelineſe may cure as perfectly all hurts received from any mortal beauty, as anciently the ſight of the myſterious ſerpent on the pole did cure the hurts the fiery ſerpents gave. For, ſince to gaze ſtedfaſtly on Numb. xxi. 8, 9. an outward beauty, where all your looking will but diſcover the ſame face, is found ſo effectual to kindle or to blow the fire of love (which the Greeks prettily enough expreſs by their ἐκ τοῦ ὁρᾶν γίνεται τὸ ἐρᾶν) how much muſt a due contemplation enamour us of that divine, and, though refulgent, yet ever more and more diſcoverable object, where attention and wonder ſtill mutually excite and cheriſh each other? whence the zealouſeſt and perfecteſt lovers of God are the glorious angels, of whom our Saviour ſays, that in heaven, *they always behold the face of his father which is in heaven*; Mat. xviii. 20. and thoſe bleſſed ſaints, whoſe employment, and whoſe happineſs is, in the *Revelation*, expreſſed to be, *to follow the lamb whitherſoever he goes*. And thoſe unblemiſhed Rev. xiv. 4. perſons are in that place recorded to have kept themſelves undefiled by mortal beauties, to teach us, that the nearer and clearer intuition, that heaven affords them of the glorious object of their paſſion, was not the cauſe of it, but the recompence; and that whilſt they lived exiles here on earth, it was ſuch a ſpeculation, as I am recommending (*Lindamor*) to you, wherewith they lived (to borrow the expreſſion uſed of *Moſes*) *as ſeeing him who is inviſible*; a ſight, whoſe glory made them look on Heb. xi. 27. fading beauties with as undazzled and untroubled eyes, as eagles can be ſuppoſed to caſt on glow-worms, when they have been newly gazing on the ſun.

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* The ſame *Seraphim*, in Hebrew, ſprings from the root *Saraph*, which ſignifies to burn or flame: whence, *Numb*. xxi. 6. thoſe pernicious creatures, that our tranſlators engliſh, *fiery ſerpents*, are ſtyled in the original *Hannechaſim Has-ſeraphim*.

To engage your love, *Lindamor*, to this ſublimeſt of objects, I ſhall deſire, that *Sect.* 3. all that I have ſaid to divert your former flames, and all that may ſay to depreciate and diſcredit thoſe degrading and diſquieting amours, that are wont to inveigle mortals, may be looked upon as meant, rather by way of preparative, than of motive, aiming but to diſabuſe and rectify your mind, that with compoſed and unprepoſſeſſed thoughts you may judge of the object I propoſe to you. Which to qualify you to be but impartial to, this method ſeemed expedient; being but imployed upon ſuch a ſcore, as that on which the Platoniſts uſed to premiſe certain virtues, which they (ſignificantly enough) called purgative, to diſpoſe and fit the mind for the reception of ſublimer truths. As phyſicians to clear the ſight are wont to purge the head, that the eye, freed from all darkening and diſguiſing tinctures, may the more perfectly diſcern the objects preſented to it. And certainly, the love I would perſuade, is of a nature, that makes nothing more conducive to it than the greateſt uncloudedneſs of the eye, and the perfecteſt illuſtration of the object; which is ſuch, that the cleareſt reaſon is the moſt advantageous light it can deſire to be ſeen by. You know the ſtory of that witty wench, who uſed merrily to wiſh her lovers all good qualities, but a good underſtanding; for that, ſaid ſhe, would make them out of love with me. The divine object, to which I would prefer you, *Lindamor*, is ſo far from being obnoxious to a danger of that nature, that I need not diſparage any other to enamour you of this. For, the Creator is too full of excellences to need to be recommended or ſet off by the creature's defects. He was ſovereignly lovely in himſelf, before they had a being; nor has his giving us that paſſion, called love, made him the unfitter object for it. To love God, but becauſe your miſtreſs has perſonal faults, is a greater, than any of thoſe, for which you have declined her, and is ſuch a ſolœciſm in ſeraphic love, as it were in the Perſian religion (for that has yet embracers in the Eaſt, as thoſe, that have there converſed with them, aſſure me) to make the ſpots, that blemiſh the moon, the motives of their worſhipping the ſun. *Hermione* is a lady, to whoſe noble relations I am too much a ſervant, not to have a reſpect for her; and I cannot without concluding her an extraordinary perſon, remember, how lately ſhe enjoyed the honour of captivating *Lindamor*; and though ſhe has ſince, by injuriouſly

declining, juſtly forfeited the glorious title of his miſtreſs; yet the qualities, that
gave it her, made me, among divers others. like her ſo well, that it is now my quar-
rel to her, that ſhe is not what ſhe was. And therefore whatever may fall from my
pen to her diſadvantage, relates to her but as ſhe was, or may again be, an obſtacle to
your devotedneſs to ſeraphic love. To engage you to which (all this notwithſtanding)
Lindamor, I muſt do you the right to tell you, that God deſerves the higheſt eleva-
tion of your love, though *Hermione* had not rejected it. Were ſhe not only as hand-
ſome as you fancied her, but as conſtant as you wiſhed her, her beauty and her friend-
ſhip too concurring could not have made her other than as diſadvantageous as inju-
rious a rival unto God; and, in the very fruition of as obliging returns, as her kind-
neſs could have made your paſſion, ſhe would have deſerved but a more moderate
degree of it. Angels, to whom women were never falſe or cruel, love their Maker
Rev. xxii. above all things: and he to whom St. *John* preſents his adoration, refuſed it for him-
4. and ſelf to direct it unto God. Would not yourſelf, *Lindamor*, have thought your *Her-*
xix. 1. *mione* abſolutely handſome, though Moors had not worn the devil's livery; and the
thing called deformity had been as much a ſtranger, as it is unwelcome to her ſex?
It is true, ſuch is our frailty, that, as the Iſraelites needed a cruel and oppreſſive
bondage in *Egypt*, to make them reſolve on returning to the bleſſed land of *Canaan*;
ſo oftentimes, the amorous ſoul needs the harſh uſage of a diſdainful miſtreſs, to diſ-
guſt it with its thraldom, and make it aſpire to its more genuine and ſatisfying ob-
ject. I may therefore allow *Hermione*'s defects to have given you freedom and occa-
ſion to conſider the prerogatives of *Seraphic Love*; but I muſt not allow them to be
the chief motives of your embracing it. For, it were too injurious to its glorious
object, to make that but the refuge of a defeated paſſion, whoſe tranſcendency entitles
it to more than the moſt elevated and entire one. Alas, God needs not theſe beg-
garly parallels with any fading objects, to become that of our diſabuſed devotion;
and he were lovely without compariſon, as well as he is ſo above it. And though I
be not ſo unjuſt to deny, that there are ladies (ſome of which I have the happineſs
not to be unknown to myſelf) whoſe excellencies are capable of ennobling their whole
ſex: yet their greateſt accompliſhments compared to his perfections, whoſe gifts they
are, are, in that eclipſing company, as inconſpicuous as the faint qualities of more
ordinary perſons. As when in a clear morning, the riſing-ſun vouchſafes to viſit us,
as well thoſe bright ſtars that did adorn our hemiſphere, as thoſe dark ſhades that did
benight it, vaniſh: conſonantly whereunto, give me leave to obſerve to you, *Linda-*
mor, that though divers of God's attributes are through his goodneſs participated by
his creatures, yet the ſcripture makes ſo vaſt a diſparity betwixt the excellencies, that
it aſcribes to men, and the ſame perfections conſidered as they exiſt in God, that it
ſeems abſolutely to exclude created beings from any title to thoſe attributes; becauſe
they poſſeſs them but in a way ſo inferior to that tranſcendent, peculiar, and divine
manner, in which they belong to God. Thus our Saviour ſays to him, that (taking
Mat. xix. him but for a man) called him good, *Why calleſt thou me good? there is none good but*
16, 19. *one*, that is, *God*. Thus St *Paul* calls God (or Chriſt) the only potentate, μόνος δυνάστης,
2 Tim. vi. 15. though the earth be ſhared by ſeveral potentates; and even the devout eunuch in the
Acts viii. 27. *Acts*, and the depoſed grandees mentioned by the bleſſed virgin in her canticle, are,
Luke i. 52. in the original, ſtiled potentates. Thus, there be wiſe virgins as well as fooliſh, and
Mat. xxv. though our Saviour tells us, that *the children of this world are in their generations*
Luke xvi. 8. *wiſer than the children of light*; yet St. *Paul* ſcruples not to term his Maker, *The only*
1 Tim. i. 17. *wiſe God:* and thus he elſewhere paraphraſes him, *He that hath only immortality*, though
1 Tim. vi. 16. angels and human ſouls be deathleſs. In ſo incommunicable a manner does the ſupe-
riority

riority of God's nature make him poſſeſs thoſe very excellencies, which the diffuſiveneſs of his goodneſs makes him pleaſed to communicate. I am the more zealous, *Lindamor*, to transfigure your love into devotion (which I muſt deſire you to look upon but but as a varied name for *Seraphic Love*) becauſe I have obſerved your paſſion to have been extremely impatient of confinement, and to have eſteemed whatever may be termed limits, to be priſons. Few therefore can need more, or deſerve better, an object for their love, for which too immenſe a vaſtneſs were impoſſible. And ſuch a one is God, whoſe ſovereign perfections render him ſo uncapable of being loved too much, that the moſt aſpiring paſſion can ſcarce arrive (ſo much as) to leſſen its diſproportion to the object. Other paſſions, like other rivers, are moſt liked, when they calmly flow within their wonted banks; but of *Seraphic Love*, as of *Nilus*, the very inundations might be deſireable, and his overflowings make him the more welcome. For mortal beauties, our paſſions are like ourſelves: if our ſtatute chance to exceed a certain ſize or ſtandard, it makes us monſtrous; but devotion is like a flawleſs diamond, where the bigneſs takes the value, and the unuſual bulk both rates and inhances the luſtre and the price. To give God all our love is the greateſt command both of the law and goſpel (in its capacious and teeming womb, both comprizing and cheriſhing all the other ſervices God requires) and that there is not more exacted of us, is not, that an addition were culpable, but becauſe it is impoſſible. So noble is the nature of devotion, that it admits of failings but by one of the extremes, which is that of defect. For, mediocrity (whoſe office it is to reſtrain us from approaching the utmoſt limits) which in other paſſions is an excellence, is here an imperfection. Or, at leaſt, if mediocrity be that, which creates paſſions virtues, the mediocrity of this love muſt conſiſt in the boundleſſneſs of it, ſince that is it, which makes it moſt a virtue. The man after God's own heart, is not afraid to own, even to his Maker, an ardency of love for him, which muſt be expreſſed by the ſignificant metaphor of thirſt; and that ſuch a thirſt too, as makes the panting hart (by naturaliſts obſerved to be a very dry creature) *bray* (as I remember the Hebrew hath it) for thoſe refreſh-ſtreams, whoſe want diſtreſſes and reduces her to an almoſt gaſping condition: *My very ſoul* (ſaith he) *thirſteth for God*. And we know, that thirſt is not only ſo violent an appetite, that it leſſons the wonder of that monarch's bargain, whom hiſtory records to have parted with his kingdom for a cup of water; but thirſt doth ſo confine our longings to what it craves, that nothing elſe can ſatisfy them. The wealth of both the *Indies* would not excuſe the want of a needed cup, ſuppoſing their poſſeſſor tormented with an appetite, which cannot be quenched but by drink. To which I muſt add, that the uneaſineſs of unrelieved thirſt is not, like that of other inconveniences, leſſened by continuance, but grows by laſting the more unſupportable. The ſame inſpired poet ſcruples not alſo to profeſs ſo ſenſible and ſo active a concern for God's intereſts, that the *zeal of God's houſe had eaten him up*; and hugely troubled he is, that others are not affected with the ſame zeal. *I beheld* (ſays he) *the tranſgreſſors, and was grieved, becauſe they kept not thy word.* Nay, *rivers of waters*, ſays he, *run down mine eyes, becouſe they kept not thy law.* And to manifeſt how much the tenderneſs and unreſervedneſs of his love made him think thoſe his friends or enemies, that were ſo to God; *Mine eyes,* (ſays he) *ſhall be upon the faithful of the land, that they may dwell with me: he that walketh perfect in the way, he ſhall ſerve me. Do not I hate them, O Lord, that hate thee, and am not I grieved with thoſe, that riſe up againſt thee? I hate them with a perfect hatred, I count them mine enemies.* At this rate did pious *David* love his Maker; but he was ſo far from thinking this rate exceſſive, that, tranſported by the ſenſe of his perſonal diſability to pay that divine object all the love, that

Pſa. xlii. 1.

Cervina caro ſicca eſt, &c. *Sennert.* Inſti. De Alimentor. facultatbus, *lib.* 4. *part* 1. *cap.* 3. *Verſ.* 2.

Pſal. cxix. 15.

Ibidem ver. 136.

Pſal. ci. 6.

Pſal. cxxxix. 21, 22.

that his perfections merited, he is not content to rouſe up all his own faculties to praiſe Pſal. ciii. 1. God (*Bleſs the Lord, O my ſoul, and all that is within me bleſs his Holy Name)* but he Pſal. cxvii. 1. invites all the godly to aſſiſt him in the payment of ſo vaſt a debt; *Love the Lord, all ye ſaints, for,* &c. And again, Pſalm cxxxvii. 2. *Praiſe the Lord, all ye nations, praiſe him all ye people.* And not content neither frequently to do this, as may appear by very many paſſages of his ſacred poems, he extends his invitation to the angels, and all the other hoſts of God, and concludes the book of *Pſalms* with a, *Let every thing, that hath breath, praiſe the Lord.* Hallelujah.

Sect. 4. Nor does it invalidate what has now been delivered, that ſome men have, even by devout perſons, been blamed for too much devotion; for, it was not an exceſs of love, but a want of diſcretion, that was guilty of their faults: the expreſſions of our love to God ought to be regulated, not by our blind and wild fancies, but by his revealed will (as Chriſt ſays, *if you love me, keep my commandments.)* And therefore it is very poſſible to be too devout, not becauſe any expreſſion of *Seraphic Love* can be made with too much ardency, whilſt it is conſidered abſtractedly in itſelf, and irrelatively to the reſt; but becauſe that there being ſeveral duties of love, which require an ardency of it, it is injurious to exerciſe all that in one alone, or a few, that belongs equally to the neglected others. We muſt not (as too many profeſſors are now wont to do, of whoſe error you may receive a fuller account in ſome other papers) daſh in pieces the two tables of the law againſt one another; but muſt ſo *love God with all our hearts, as to love our neighbour as ourſelves.* You know what our Saviour ſaith to the Phariſees, that tithed mint and cummin, with a neglect of judgment, Mat. xxiii. 23. mercy, and faith, thoſe weightier matters of the law; *Theſe ought you* (ἔδει) *to have done, and not to leave the other undone.* And indeed this partiality, *Lindamor,* which makes us diſplay ſo much of the ſtrength and vigour of our ſpirits in ſome favourite duties, that we can but languidly and perfunctorily perform thoſe others we are leſs fond of, begets in devotion a diſeaſe, not unlike that new one in children we call the rickets, which ſome learned phyſicians do not improbably conceive to raiſe from the unequal nutrition of the parts. For though none of them receive exceſſive nouriſhment, yet ſome of them receiving as much as is convenient for them, and thereby growing up to their natural bigneſs, whilſt others are leſs nouriſhed than (were the body healthful) they would be, do grow ſo little, that the ſounder parts ſeem overgrown, and ſo the diſproportion betwixt them and the ricketing ones makes the whole body they compoſe miſhapen and unwieldy. But, *Lindamor,* this proves not, that we can love God too much, but only that we may employ too much of that Job iv. 19. love, in this or that way of expreſſing it. Whilſt we are (as *Job* ſpeaks) *Inhabitants of theſe houſes of clay,* there are many duties, which do as well challenge an intenſity 1 Cor. vii. 32, 33, 34. of our affections, as thoſe which relate more immediately to God. As St. *Paul* tells us, that there is difference betwixt married and ſingle perſons, the affections of the one being at liberty to devote themſelves more undiſtractedly to God, whereas thoſe of the other are diſtracted; (as *Adam*'s were betwixt his Maker and his rib). But where a direct and immediate expreſſion of love to God defrauds not any other duty, there it is free from the danger of exceſs. Though prayers may eaſily be too long, and faſts grow exorbitant; yet Chriſt could ſpend the whole night in prayer, and faſt forty days without immoderateneſs, when the other expreſſions of his love to 1 Kings xix. 2. his Father, and the other exerciſes of his mediatory function, were not thereby diſturbed, but furthered and promoted. And ſo *Elijah* might inculpably faſt long, when that faſting did not diſable him to proſecute his journey to the mount of God; and though juſt men here on earth muſt expreſs their love to their maſter by (that buſy diſtracting, and remoter way of ſervice) trading with his talent truſted to them; yet when their diveſture of mortality diſpenſes them for thoſe laborious and avocating duties to diſtreſſed Chriſtians, and their own ſecular relations, which are here requiſite to be performed; their glorified ſpirits may now, without any immoderate devotion, employ, I ſay not, their time, but their eternity itſelf, in converſing with God, and following the Lamb whitherſoever he goes. And congruouſly I obſerve, that the Rev. iv. 6, 7. four myſterious beaſts, allowed to approach neareſt to the throne of God, though their many wings, and more numerous eyes, intimate them of a very active nature, are repreſented to us in the apocalypſe, as addicted but to one employment, ceaſing *neither day nor night from ſaying, Holy, Holy, Holy, Lord God Almighty*; and from ver. 8. *giving glory, and honour, and thanks unto him.* And of *thoſe that have whited their robes in the blood of the Lamb*, this account is in the ſame book given us, *that they are be-* Rev. vii. 14, 15. *fore the throne of God, and ſerve him by day and night in his Temple.* So true is it, that no degree of *Seraphic Love* can be exceſſive; nay, not any expreſſion of it immoderate, unleſs it be made ſo, not by its greatneſs, but by its uſurpation, whereby it either ingroſſes or invades what belongs to its injured and languiſhing aſſociates. Our love unto the creatures is a preſent, but unto God it is a tribute; and though we may eaſily play the prodigals in parting (over-freely) with our gifts, we can ſcarce be ſo in the payment of our debts; for, be the ſums never ſo vaſt we pay away, their being due, in ſpight of their being great, makes the diſburſement too much an act of juſtice, to be one of profuſeneſs. *Seraphic Love* (whoſe paſſionateneſs is its beſt complexion) has then moſt approached its nobleſt meaſure, when it can leaſt be meaſured; nor aught its extent to admit any other limits, than an utter diſability to exceed thoſe that terminate it. For he alone loves God as much as he ought, that, loving him as much as he can, ſtrives to repair the deplored imperfection of that love, with an extreme regret to find his love no greater. Such a ſublimity of love will beſt entitle you to the conſolation accruing from that memorable paſſage of St. *John*, where he ſays, that *God is love, and he that dwelleth in love dwelleth in God, and God in him*; 1 Joh. iv. 16. which ſupplies me with a forcible inducement, to invite you to an eager aſpiring to a tranſcendency in devotion, ſince it may render ſelf-denial ſo eaſy, that it will (at laſt) almoſt diveſt that name. For this ſublimer love being, by an intimate conjunction with its object, wholly devoted to it, and throughly refined from all baſe droſs of ſelfiſhneſs and intereſt, nobly begets a moſt ſtrict union of our wills with God's (or rather) a perfect ſubmiſſion of the one to the other. And thus, when it is become your will to obey his, no diſpenſations of providence will immoderately diſquiet you: for, you poſſeſs your wiſhes in general, and in bulk, though poſſibly not always in retail. For, your chiefeſt deſire being to ſee your Maker's will fulfilled, your knowledge of his being the ſovereign and uncontrouled diſpoſer of the events aſſures you, that all accidents, that can befal you, are but exact accompliſhments of his will; and conſequently of yours, ſo far forth as that is included and comprized in his. When you have reſigned, or rather conſigned, your expropriated will (if I may ſo call it) to God, and thereby (as it were) entruſted him to will for you; all his diſpoſals of, and his diſpenſations towards you, are in effect the acts of your own will, with the advantage of their being directed and ſpecified by him: an advantage, that does at once aſſure you both of their rectitude and ſucceſs. God's wiſdom, power, and love to you conſidered, how much more happy muſt you be in your opinions of his chuſing for you, than your immediate own? The patient thinks himſelf obliged to gratify his phyſician for chuſing for him what ſorts of meat he is to feed on; though the doctor be wont to make ſuch a choice for him, as deprives him of the diſhes he beſt likes, and oftentimes confines him to thoſe he loaths. Alas, how often might God ſay of our requeſts, as Chriſt did of thoſe of the two aſpiring diſciples, *Ye know not what to aſk?* I admire and bluſh to read in an heathen ſatyriſt ſo heavenly a leſſon, as,

Permittes ipſis expendere numinibus, quid
Conveniat nobis, rebuſque ſit utile noſtris:
Nam pro jucundis utilia quæque dabunt di.
Charior eſt illis homo, quam ſibi: nos animorum
Impulſu, & cæca pravaque cupidine ducti,
Conjugium petimus partumque uxoris; at illis
Notum, qui pueri, qualiſque futura ſit uxor.

'Unto the wiſer gods the care permit,
'Of what's for us and our affairs moſt fit.
'They will for pleaſant things the beſt confer;
'To whom man is than to himſelf more dear,
'We by our blinder paſſions led aſtray,
'Do for a wife perhaps or children pray;
'Which they may chance refuſe us out of love,
'Knowing what both the wife and boys would prove.'

THE conſideration of which made a heathen philoſopher ſay, that he was wont only in general terms to beg good things of the gods, leaving it to them to determine what things were good for him. And indeed our own wiſhes are but too commonly as blind as *Rachel's*, who having ſo eagerly longed for children, that ſhe impatiently cries, *give me children or elſe I die*, died in child-bearing; and as deſtructive to the wiſhers, as their longings proved to the murmuring *Iſraelites*, who loathing the wholeſome manna (that bread of angels) God had provided for them, are their own bane in the fleſh they had ſo greedily luſted for. Thus, *Lindamor*, that ſo affrighting virtue of ſelf-denial proves to be little more than a ſon's letter of attorney to his father, of whoſe paternal kindneſs and conſummate abilities in the management of affairs his confidence amounts unto a certainty. Nay, till my ſecond thoughts checked the over-forward impetuoſity of my firſt, I was about to add, ſince God reſents an infinite ſatisfaction in the accompliſhment of his own will, your making over your whole will to God will impart to you that felicity, proportioned to the degree of the reſignment. And as the eye, whilſt by the optic nerve tied unto the head, ſo changed, can taſte delights, which it is dead to, being once ſevered from it, though (otherwiſe) it enjoy the beſt condition of which its inanimate nature can be ſuppoſed to be capable; ſo may your will, by an identity or ſameneſs (in tendency, though not in nature) with your Maker's, as it were engrafted into God's, receive a new and enlarged capacity, which will enable you to contain and reliſh joys, highly tranſcending thoſe, which the fulleſt fruition of your private wiſhes were able to create. Thus ſelf-denial is a kind of holy aſſociation with God, and (by making you his partner) intereſts you in all his happineſs and acquiſitions. And conſonantly we ſee, that glorified ſaints and bleſſed angels, whoſe will have the moſt exquiſite and exact conformity to God's, enjoy a happineſs moſt approaching his; whereas the apoſtate ſpirits, in a confirmed repugnancy to his will, find the extremity of wretchedneſs.

Gen. xxxv. 8.

Num. xi. 33.

But though I dare not own, *Lindamor*, ſo bold a ſally, yet I dare without ſcruple Sect. 5. improve the diſcourſe that preceded it, to make out to you an advantageous difference of *Seraphick Love* from ordinary flames. For, he that makes a preſent of his heart to any mortal beauty, even by her welcoming it, and lodging it with her own, grows ſubject to have it wounded in her breaſt. Thoſe misfortunes reach him, that would otherwiſe terminate in her: her afflictions torment him, whilſt his own reprieve him; and the felicity of two perſons grows requiſite to make one happy. The letting out our love to mutable objects doth but enlarge our hearts, and make them the wider marks for fortune, and capable of being wounded in more places: for, although love may as well make us participate the joys, as reſent the infelicities of the parties loved; yet even the leaſt unhappy perſons do in ſo fickle and ſo tempeſtuous a ſea, as we all find this world, meet with ſo many more either croſs-winds, or ſtormy guſts, than proſperous gales; and we are ſo much more ſenſible of pain than pleaſure (an aching corn, though leſs than a ſickneſs, unfitting us to reliſh the otherwiſe perfect health of the whole body) that even friendſhip itſelf, though a much calmer affection than love, ought to be declined as injurious to our quiet, did we conſider it but as a partnerſhip of fortunes, not an exerciſe of virtues. But he, whoſe wiſer love ſettles itſelf on God, is not only, by the immutable, and even eſſential happineſs of that adorable object, ſecured from participated infelicities; but finds his perſonal croſſes and diſtreſſes ſweetened by conſidering, that what he moſt loves is moſt happy, and as able as willing in due time to make him ſo. And though *Seraphick Love* make us partake but God's felicities, yet his acceptance of it makes him reſent our ſorrows. *In all their* Iſai. lxiii. 3. *afflictions he was afflicted*, ſays the prophet of God, and of the Iſraelites. And ſo the Son of God (who is ſo much one with thoſe that love him, that both he as the head, and they as the members, are ſometimes, as making up one body, called by one name, Chriſt) though as high as heaven, above the reach of perſonal or immediate 1 Cor. xii. 12. perſecutions, called out to *Saul* for an intention of harming thoſe that loved him, *Saul, Saul, why perſecuteſt thou me?* and to demonſtrate the tenderneſs of this compaſſion, the prophet ſays to the returned Iſraelites, concerning God, *He that* Zech. ii. 8. *toucheth you, toucheth the apple of his eye.* Nor is God's compaſſion like a miſtreſs's, a grieving only, and an uſeleſs pity, whereby the ſuffering lover is oftentimes leſs comforted, as it proceeds from her kindneſs, than afflicted, becauſe it breeds her diſquiet; but God's is a compaſſion, though active, yet ſerene, and worthy of himſelf, which without producing the diſcompoſure, produces the effects of the moſt ſenſible pity, by engaging him to a timely relief and reſcue: as that freſhly mentioned expreſſion, *In all their afflictions he was afflicted*, is immediately followed by, *and the angel of his preſence ſaved them; in his love, and in his pity he redeemed them, and he bare them and carried them all the days of old.* Yes, this pity, for its not diſturbing God's happineſs, inclines him not the leſs to expreſs a ſenſe of our miſeries, and makes us find (to uſe a ſcripture phraſe, as I would render it) χάριν εἰς ἐυκαιρον βοήθειαν Heb. iv. 16. (*grace for an opportune relief,*) I ſay, with the divine writer, *an opportune* or *ſeaſonable relief*; becauſe it comes not always when it is moſt deſired, but when it is moſt fit; and when that is, he that hath at once all preſent, paſt, and future things in his proſpect, is fiteſt to determine. Chriſt's words to his diſciples, *It is not for you to know* Acts i. 7. *the times or the ſeaſons, which the Father hath put in his own power*, are applicable to more caſes than that which occaſioned them. The Canaanitiſh woman muſt put up a M t. vii. 27. refuſal, and the reproachful name of a dog (which yet, by the way, was a paraphraſe commonly enough uſed by the Jews of the heathen, and as ſuch was underſtood by this 'Ελλενὲς Gentile not Greek *.) The Iſraelites (compriſing the patriarchs their progenitors) were reduced to wait four hundred and thirty years, ere they were Gal. iii. 17. introduced into the promiſed land; and during a great part of that long ſpace of time, languiſhed and groaned under the heavy burdens, and other as heavy preſſures Ezekiel xxviii. 24. of the as cruelly as unſucceſsfully politick Egyptians. St. *Paul* himſelf prayed the 2 Cor. xxxii. 7. Lord thrice for the removal of that rude σκόλοψ τῆ σαρκὶ, *thorn to the fleſh* (whatever that may mean). Nay, of the bleſſed virgin-mother herſelf, her divine Son would Luke ii. 48. not be found till the third day, though ſhe ſought him ſorrowing. And *Lazarus*, to whom, even during his ſickneſs, he vouchſafed (a title, to which all *Cæſar*'s were Joh. i. ver. 3. but trifles) the ſtyle of friend, which emboldened the pious *Mary* to paraphraſe him by a *he whom thou loveſt*, was permitted not only to lie a dying, but to die; his reſcue being deferred, till it was thought impoſſible, and was ſo indeed to any leſs power than omnipotence. Which manifeſts, that as no degree of diſtreſs is unrelieveable by his power, ſo no extremity of it is inconſiſtent with his compaſſion, no not with Mal. iii. 3. his friendſhip. He, whoſe ſpirit inſpired the prophets, is in the laſt of them repreſented under the notion of a refiner: and it is not the cuſtom of refiners to ſnatch the beloved metal out of the fire, as ſoon as it feels the violence of that purifying element; nay, nor as ſoon as it is melted by it, but they let it long endure the brunt of the active flames, actuated by exciting blaſts, till it have ſtood its due time in the fire, and there obtained its full purity and ſplendor. And I hope, you will give one, that converſes with furnaces, though he be no pretender to the philoſopher's ſtone, leave to implore a chymical metaphor, and obſerve, that though in afflictions, eſpecially national or publick calamities, God oftentimes ſeems to make no diſtinction betwixt the objects of his compaſſion, and thoſe of his fury, indiſcriminately involving them in the ſame deſtiny; yet his preſcience and intentions make a vaſt difference, where his inflictions do not ſeem to make any: as when on the ſame teſt, and with the ſelf-ſame fire, we urge as well the gold, as the blended lead or antimony; but with foreknowing and deſigning ſuch a diſparity in the events, as to conſume the ignobler minerals, or blow them off into droſs or fumes, and make the gold more pure and full of luſtre.

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* The text refers her extraction to *Syrophœnicia*, the ſame region with *Canaan.*

Sect. 6. It is true, *Lindamor*, and (not to be ſuſpected of partiality towards a love, which ſo little needs it to be thought fit to be preferred before all other paſſions) I ſhall acknowledge it, that the happineſs reſulting from thoſe many prerogatives I have endeavoured to diſcover to you in a tranſcendent degree of *Seraphick Love*, is moderated by the effects of that ſublimity, the eager deſires it creates of a more complete fruition, of its perfect and divine object. Such aſpiring ſallies of the longing ſoul Cant. ii. 5. made the languiſhing ſpouſe in the *Canticles* cry out, *Stay me with flagons, comfort me* Phil. i. 23. *with apples, for I am ſick of love.* Such made the raviſhed apoſtle deſire to return * Luk. xii. 36. (for ſo I ſhould rather tranſlate the word, ἀναλῦσαι, there, and ſo I find it * elſewhere to ſignify) *and to be with Chriſt:* and the inſpired poet thus expreſſes his longings to Pſal. xli. 1, 2. the bleſſed object of them, *As the hart panteth after the water-brooks, ſo panteth my ſoul after thee, O God. My ſoul thirſteth for God, for the living God: when ſhall I come and appear before God?* But, *Lindamor*, it was fit, that to elevate our thoughts and wiſhes to heaven, ſome peculiar, and elſewhere incommunicable degree of joy ſhould be reſerved for us there. And it is a good ſign, and ſuch as worldly objects cannot boaſt, when the incompleatneſs of our *Seraphick Lover*'s happineſs, in his fruitions, proceeds not from their want of ſatisfactorineſs, but his want of an entire poſſeſſion of them. And let me tell you, *Lindamor*, that even this uneaſy ſtate of ſeparation is ſweetened with as much allay as is conſiſtent with its being a grief. For, the divine evidence and teacher of God's love pronouncing a thirſt after perfection to be a title Mat. v. 6. to it (according to thoſe Scriptures, *Bleſſed are they that hunger and thirſt after righteouſneſs*;

ouſneſs; *for they ſhall be ſatisfied*: and, *Let him that is athirſt come, and whoſoever will,* Rev. xxii. 17. *let him take of the water of life freely)* and the joys of heaven being ſo vaſt, that they diffuſe their nature to all the grounded hopes men have to obtain them; each new aſſurance is a new degree of them, and is acceptable to our hope, though uneaſy to our deſires. And theſe baitings at compleat felicity ſhould not be more unwelcome for the preſent diſquiet they ſuppoſe, than the contrary for the zeal they argue, and the felicity they promiſe. For this production of the ſpirit in our hearts may be juſtly termed as the Spirit himſelf in ſcripture is, an earneſt; which though, by being ſuch, 2 Cor. v. 5. it confeſſes itſelf not to be the entire ſum, yet it is not only a part of it, but a pledge: and, *Lindamor*, how ſupportable is this thus qualified allay of the joys of *Seraphick Love*, in compariſon of the diſquiets, and the torments, that are wont to attend ſenſual love? I ſhall not loſe [time] to enumerate how many it is ſuppoſed to have ſent to their graves; becauſe, though I find thoſe tragical ſtories riſe enough in romances, yet I find them rarities every where but in thoſe fabulous compoſures: and though I have had the curioſity to viſit ſome of thoſe warmer regions, where the flames of love are thought to burn with more violence, yet (bating the duels and the pox) I remember not to have obſerved love to have ever been the death of any man; unleſs, ſpeaking like philoſophers, who make reaſon the eſſential conſtituent form of a man, we will affirm, that love, by dethroning reaſon, though it leave the lover alive, doth kill the man. But, though I am loth to put ſo bad a compliment upon mankind, as to ſay, that love is wont to deſtroy men's lives; yet I think it would be no calumny to ſay, it much diſquiets them. I could aſk you, how long many a lover muſt continue a ſervant, to purchaſe the honour of being taken notice of to be ſo? And I could recruit that queſtion with pretty ſtore of others of the like nature; but that I ſuppoſe your memory will ſave my pen the labour of repreſenting to you the torments of love, which they that feel them, would little leſs juſtly than they do frequently ſtyle martyrdoms, if the greatneſs only, without the cauſe and object of men's ſufferings, ſufficed to make them martyrs. And though the condition of lovers be in romances ſo dextrouſly and delightfully deſcribed, that not only ſanguine readers are tranſported, but even I myſelf have been ſurprized into inclinations, to admire and envy their felicity; yet when ſome, I was concerned for, have been really concerned and engaged in ſome adventures, my envy quickly turned into pity. For, the repulſes, the regrets, the jealouſies, the fears, the abſences, the deſpairs, and the reſt of the afflicting diſquiets of lovers; though in well-writ romances they are ſoon read over by the diverted purſuer, yet they are not ſo ſoon weathered out, nor ſo eaſily ſupported by the diſconſolate lover; whoſe infelicities, though they may be perhaps ſo handſomely deplored, as to delight the reader, yet truſt me, *Lindamor*, it is a much happier condition to be free from misfortunes, than to be able to complain eloquently of them. And as I have with delight beheld a ſtorm excellently drawn by ſome rare artiſt's pencil; but when I was this ſpring toſt by the rude winds, that blew me out of *Holland*, I found a real ſtorm a very troubleſome and uneaſy thing: ſo the condition of a lover, though drawn by a ſmooth pen, is wont ſtrangely to affect and pleaſe us; yet when men are really engaged in it, they find it full of hardſhips and diſquiet. It is a much better condition to be looked on than embraced; and experience gives men of it (ſadder and) more unwelcome notions than deſcription did. Nor fancy, *Lindamor*, that the troubleſomeneſs of your ſufferings in love proceeded but from their not being acceptable to her, for whom you endured them; for had your miſtreſs crowned them with myrtle, and proved as kind to you as *Hymen* could have made her, yet, I fear, ſhe could have recompenſed you but by diſabuſing you, and could not have freed you from the need of happineſs, but only from a miſtake concerning it. For, methinks, *Lindamor*, moſt of theſe tranſitory goods, that we are ſo fond of, may not unfitly be reſembled to the ſenſitive plant, which you have admired at *Sion-garden*; for as, though we gaze on it with attention and wonder, yet, when we come to touch it, the coy deluſive plant immediately ſhrinks in its diſplayed leaves, and contracts itſelf into a form and dimenſions, diſadvantageouſly differing from the former; which it again recovers by degrees, when touched no more: ſo theſe objects, that charm us at a diſtance, and whilſt gazed on with the eyes of expectation and deſire; when a more immediate poſſeſſion hath put them into our hands, their former luſtre vaniſhes, and they appear quite differing things from what before they ſeemed; though after deprivation or abſence hath made us forget their emptineſs, and we be reduced to look upon them again at a diſtance, they recover in moſt men's eyes their former beauty, and are as capable as before to inveigle and delude us. I muſt add, *Lindamor*, that when I compare to the ſenſitive plant moſt of theſe tranſitory things, that are flattered with the title of goods, I do not out of that number except moſt miſtreſſes. For, though I am no ſuch an enemy to matrimony, as ſome (for want of underſtanding the raillery, I have ſometimes uſed in ordinary diſcourſe) are pleaſed to think me, and would not refuſe you my advice (though I would not ſo readily give you my example) to turn votary to *Hymen*; yet I have obſerved ſo few happy matches, and ſo many unfortunate ones; and have ſo rarely ſeen men love their wives at the rate they did, whilſt they were their miſtreſſes; that I wonder not, that legiſlators thought it neceſſary to make marriages indiſſoluble, to make them laſting. And I cannot fitlier compare marriage than to a lottery; for in both, he that ventures may ſucceed, and may miſs; and if he draw a prize, he hath a rich return of his venture: but in both lotteries there is a pretty ſtore of blanks for every prize. And for your particular, *Lindamor*, the world is much miſtaken in both your humours, if *Hermione*'s and yours be not ſo unſuitable, that (to make haſte from ſo nice a ſubject) had ſhe juſtified your expectation of her kindneſs, you would have poſſeſſed the perſon without poſſeſſing the happineſs you expected; and might have found yourſelf as ſenſibly diſappointed by her grant, as you were by her change.

But I forget, *Lindamor*, that I reſolved not to inſiſt on parallels; and therefore, inſtead of proſecuting the diſcourſe my pen has ſlipt into, concerning the advantages of *Seraphick*, compared with ordinary love, I ſhall venture to encourage you to the former, by ſhewing you, that your paſt addictedneſs to the latter may prove ſerviceable to you in it. Yes, *Lindamor*, I ſhall not ſcruple to tell you, that your paſſion for *Hermione* may not a little facilitate your devotion, partly by breaking all the chains, excepting one, that faſtened your affection to unſatisfying objects, and reſtrained it from ſoaring to the ſublimeſt; and partly, by exalting your paſſion to a height fit for ſeraphick flames. For love hath this of noble, that it makes us diveſt ourſelves of ſelfiſhneſs, ſlight fortune, quiet, ſafety, honour, life, and all our own concernments, when their coming into competition with the loved party's intereſt, may render their ſacrifice acceptable to her; and make us think goods or ills deſerve thoſe names, but as they come to us from or through her. You could ſcarce have learned a better leſſon, even from a much better maſter; for love having thus accuſtomed you to (what is thought moſt difficult in that virtue) the acts of ſelf-denial, you need almoſt but transfer your flames from an inferior and miſtaken to their true and nobleſt object, and you will have exalted and refined your love into devotion; to the latter of which a ſublime elevation of the firſt is ſuch a diſpoſition, as the having formerly by looſer airs and (perchance) wanton ſongs, learned to improve and to command one's voice, is, to the ſkill of ſinging thoſe devout hymns and heavenly anthems, in which the church militant ſeems ambitious to emulate the triumphant, and eccho back the ſolemn praiſes and hallelujahs of the celeſtial quire. And, as by hunting, though (poſſibly) we follow but ſome poor fugitive hare, or ſome ſuch trifling game, we gain that vigour, that ſufferance, and agility, that fits us for the toils and military hardſhips that are exacted in the purſuit of glory, and of empire: ſo though in love (devotion's prenticeſhip) the courted creature be often conſiderable enough to make our elections fit to be numbered among ſuch as thoſe that made love be painted blind; yet in the progreſs and conduct of our paſſion, we contract ſuch diſintereſted and reſigned habitudes, as, being preferred to ſerve celeſtial objects, do excellently qualify us for devotion. And in effect, a fervent love ſeems little elſe than devotion miſaddreſſed, where our own very expreſſions may ſerve to diſabuſe us: for, when you give your miſtreſs the ſtyle of goddeſs, and talk of nothing to her, but offering up of hearts, adoring, ſacrifices, martyrdoms; does not all this imply, that though it be ſaid to her, it is meant to a divinity: which is ſo much the true and genuine objects of men's love, that we cannot exalt that paſſion for any other, without inveſting it with the notion and attributes of God? as children diſcloſe the inbred kindneſs they have for thoſe perſons, by calling the babes they moſt doat upon by their dear mother's, or loved nurſe's names. And as *Aaron*, and the revolting Jews, by juſtifying to themſelves their adoration of the idols they had ſet up, by attributing the title of God to what they adored, did tacitly acknowledge adoration to be due only to the Deity; ſo does a lover, by naming what he worſhips a divinity, tacitly confeſs the deity to be the proper object of that higheſt and peculiar ſtrain of worſhip.

Ex. xxxii. 4, 5. *In the latter of which it is expreſly ſaid that the feaſt was proclaimed* ליהוה *To* Jehovah, *the peculiar name of the true God.*

And this truth, *Lindamor*, the very fickleneſs of lovers concurs to teſtify: for, what men call, and think inconſtancy, is nothing but a chace of perfect beauties, which our love fruitleſly follows and ſeeks in ſeveral objects, becauſe he finds it not entire in any one. For creatures have but ſmall and obſcure fragments of it, which cannot fix, nor ſatisfy an appetite, born for, and (though unwillingly) aſpiring unto *Sect.* 8. God; who is proclaimed the true and proper object of our love, as well by men's fickleneſs to women, and the angel's conſtancy to him. Juſt as the trembling reſtleſsneſs of the needle, in any but the north point of the compaſs, proceeds from and manifeſts its inclination to the pole; its paſſion for which both its wavering and its reſt bear equal witneſs to. That unſatisfiedneſs with tranſitory fruitions, that men deplore as the unhappineſs of their nature, is indeed the privilege of it; as it is the prerogative of men not to care for, or be capable of, being pleaſed with whiſtles, hobby-horſes, and ſuch fond toys as children doat upon, and make the ſole objects of their deſires and joys. And by this you may, *Lindamor*, in ſome degree imagine the unimagineable ſuavity, that the fixing of one's love on God is able to bleſs the ſoul with; ſince, by ſo indulgent a father, and competent a judge as God himſelf, the decreed uncontentingneſs of all other goods is thought richly repaired by its being but an aptneſs to prove a riſe to our love's ſettling there.

Sect. 9. AND hitherto, my deareſt *Lindamor*, I have endeavoured to recommend unto you *Seraphick Love*, by mentioning ſome of its properties, which ſeem to relate more to the love itſelf, than to the divine object of it. But I fear you'll think I have too long entertained you with conſiderations, which, beſides that they are not altogether the importanteſt that belong to this diſcourſe, I have been by haſte reduced to pen in the unaccurate order, wherein they offered themſelves to my thoughts, not the method, wherein I ſhould have preſented them to yours.

Sect. 10. AND therefore, *Lindamor*, ſince the nobleſt and ſupreme motives to the love of God conſiſt in his own infinite perfections and prerogatives; and ſince the properties of God's love to us do advantage us much more (and conſequently are likelier to endear devotion to us) than thoſe of ours to him; the former not only moving God to kindle in us, but to cheriſh and foment, and (if our own wilful extinction interpoſe not) to crown the latter: for both theſe reaſons, I ſay, *Lindamor*, I doubt not, but you'll think it ſeaſonable for me to proceed, to conſider that higher ſort of motives to devotion, and to evince, that the ſeveral things, which are wont moſt to engage and heighten our affections, do, in a peculiar and tranſcendent manner, ſhine forth and conſtellate in God. That, you know, which enamoured you of *Hermione*, I need not prove to you, to have been your ſuppoſing her full of lovelineſs and excellencies in herſelf; and your believing, that the love ſhe vouchſafed you was great, free, conſtant, or advantageous to you. And that all theſe properties do not only eminently exiſt, but illuſtriouſly concur in God, and his love, I muſt now, *Lindamor*, (with ſtrong deſires of doing it proſperouſly) attempt to maniſeſt.

Sect. 11. FIRST then, our higheſt love is made God's due by the excellency and prerogative of his nature. But, truſt me, *Lindamor*, when neceſſitated (by a method exacted by the nature of this diſcourſe) I find myſelf engaged to ſay ſomething by way of celebration of God's perfections; I am very ſenſible, I can but detract from what I deſire to praiſe, and muſt inevitably appear unable to ſpeak worthily of a theme, to which even Seraphim themſelves cannot do right. And if, as the ſcripture aſſures us, thoſe things never fell under the ſenſes, nor entered the thoughts of men, which God 1 Cor. ii. 9. has reſerved for thoſe that love him; how ineffable and incomprehenſible muſt thoſe things be, which he has reſerved for himſelf? the infinite ſuperiority of his nature, above all created beings, placing a vaſt diſparity betwixt his greateſt communicated vouchſafements, and his boundleſs, and therefore to creatures incommunicable, perfections.

WONDER not therefore, *Lindamor*, that my weak eyes dare not dwell long upon an object, which they cannot ſtedfaſtly gaze on long without being dazzled; and do not marvel, that I ſcruple not to uſe ſeeming hyperboles, in the mention of perfections, which make the higheſt hyperboles but ſeeming ones; both God's nature and his word Neh. ix. 5. declaring him to be *exalted above all bleſſing and praiſe*. If it were ſeaſonable, *Lindamor*, to entertain ourſelves but with thoſe attributes of God, which are legible or conſpicuous in the creation; we might there diſcern the admirable traces of ſuch immenſe power, ſuch unſearchable wiſdom, and ſuch exuberant goodneſs, as may juſtly raviſh us to an amazement at them, rather than a bare admiration of them. And I muſt needs acknowledge, *Lindamor*, that when with bold teleſcopes I ſurvey the old and newly diſcovered ſtars and planets, that adorn the upper region of the world; and when with excellent microſcopes I diſcern, in otherwiſe inviſible objects, the unimitable ſubtilty of nature's curious workmanſhip; and when, in a word, by the help of anatomical knives, and the light of chymical furnaces, I ſtudy the book of nature, and conſult the gloſſes of *Ariſtotle*, *Epicurus*, *Paracelſus*, *Harvey*, *Helmont*, and other learned expoſitors of that inſtructive volume: I find myſelf oftentimes reduced Pſal. civ. 24. to exclaim with the *Pſalmiſt*, *How manifold are thy works, O Lord? in wiſdom haſt thou made them all!* And when I have been loſing myſelf in admiration of what I underſtand not, but enough to admire, and not to comprehend; I am often obliged to interrupt or break off my inquiries, by applying to the works of God's creation Rom. xi. 23. the expreſſion uſed by St. *Paul*, of thoſe of his providence, *O the depth of the riches, both of the wiſdom and knowledge of God! how unſearchable are his judgments, and his* *ways untraceable!* And exclamations of this nature may the attentive conſideration of any other of God's attributes deſervedly produce. But having elſewhere treated of this ſubject in a peculiar diſcourſe, I ſhall now, *Lindamor*, invite you to conſider with me, how much you, and thoſe that are conſcious to their having virtue enough in themſelves, to make them prize it in others, are in love with *Cato*, *Scipio*, and thoſe other heroes, that did ennoble, and almoſt exceed mankind, upon the bare knowledge of their virtues; although from them we derive no perſonal advantage, (their death having numerous ages preceded our nativity). Since then we pay ſo much diſintereſted love to ſome few faint and ill-refined virtues, that never did profit us; how much on ſuch a ſcore, and at that rate, ſhould we love him, who ſo poſſeſſes all perfections, that each of his perfections is infinite? Were you and I our own creators, *Lindamor*, and wholly independent upon God, without either need or hope to taſte his bounty; his native excellencies, and what he has done for others, ſhould ſurely raviſh us, and enamour us of him. Though his benefits to us did not entitle him to our love, his eſſence (the ſource and only motive of thoſe benefits) would give him a right to it; and though we owed him nought for what we are, we yet ſhould owe him love for what he is. He is that glorious ſun, from whom (as beams) all created perfections flow, and in whom they all concenter. To omit God's ſovereign majeſty (which places him ſo high, that but to own for him ſo familiar and levelling an affection as love, much more to expect to be re-loved by him, were not the leaſt ſaucy preſumption man could be guilty of, did not his own commands make it a duty) not to inſiſt on this, I ſay, let us a while conſider that proper and peculiar attraction of love, his lovelineſs; which is ſuch, that, did we but once ſee it, all creature-competitions (even we being judges) would then be as impoſſible as they are now unjuſt. In the fifth Evangeliſt's prophetic viſions, the Seraphim themſelves Iſa. vi. 7. (thoſe glorious ornaments of the celeſtial hierarchy) are repreſented as covering their faces in God's preſence, either bluſhing at their comparative deformity, or unable to ſuſtain the unqualified ſplendor of ſo divine brightneſs; whence perhaps it became of old the Jewiſh faſhion (as ſome frequent expreſſions in their writers intimate) when they went to pray, to veil their heads and faces (though now I have in their ſynagogues ſeen them cover their heads, not their faces, with thoſe white garments they wear at their public devotions). And, *Lindamor*, if *Moſes*'s face, by but a few days converſe with God, reflected ſuch a light as dazzled mortal eyes; and if his ſwift poſts, the angels, when ſent on errands to us here on earth, even when they may be ſuppoſed (if I may ſo ſpeak) to wear their travelling clothes, and ſtood as much to our frailty in the form, as the region, they appear to us in, do, in ſpite of that darkening condeſcenſion, ſo much tranſcend all objects here on earth, that the ſcripture often mentions, that even thoſe, that aſpired to imitate their virtues, were confounded at their preſence: and if, in this veiling habit, they appear ſo glorious, that their thus diſadvantaged beauty is made the compliment and hyperbole of that quality; what may we, or rather, what may we not conclude of God himſelf, of whom the ſcripture ſays, *He that planted the ear, ſhall he not hear? he that formed the eye, ſhall* Pſal. xciv. *he not ſee?* That is, he, that imparts a faculty, or an excellence to the creature, 9. ſhall not he himſelf much more eminently poſſeſs it? And in effect, the moſt unblemiſhed created beauties are but faint ſhadows (or trulier, foils) of his. Thoſe drops of prettineſs, ſcatteringly ſprinkled amongſt the creatures, were deſigned to defecate and exalt our conceptions, not to inveigle or detain our paſſions: for God did never intend them to terminate our love, but only by our eyes to exalt our faith above them, and by the beauties our ſight can apprehend to raiſe us to a confidence, 2 King ii. 11. that there is in their author more than we can either ſee or comprehend. Like *Elijah*'s fiery chariots, though they be pure and bright, and conſiſt of the nobleſt and Gen. xxiv. 10-53. gloriouſeſt materials, they are meant by God but to carry us up to him. And as the patriarch's ſteward was furniſhed with ſo ſumptuous an equipage to court *Rebecca*, not for himſelf, but for *Iſaac*; ſo all the lovelineſs imparted to the creature is lent it, but to give us ſome more enlarged conceptions of that vaſt confluence and immenſity, that exuberates in God. To make the righteſt uſe of fading beauties, you muſt conſider God and them, as you were wont to do your miſtreſs's picture and its cryſtal cover: where, though that native glaſs were pure and lovely, and very richly edged, yet to gaze on it was not the chiefeſt buſineſs of your eye; nor did you in it terminate your ſight, but greedily look through and beyond it, upon the adored image, that ſolid veil betrayed. Methinks, *Seraphic* and our common lovers behold exterior beauties with a difference reſembling that, wherewith children and aſtronomers conſider *Galileo*'s optic glaſſes (with one of which teleſcopes, that I remember I ſaw at *Florence*, he merrily boaſted, that he had *trovato la corte a Giove*;) which the one prizes moſt for what they appear; the other, for what they diſcover. For children contenting themſelves to wonder at the length, and fall in love with the workmanſhip and gildings of the tube, do thus but gaze upon them; whereas aſtronomers look through them, and, ſcarce taking notice of the unuſual ornaments, or the ſhape, employ them to find out unknown lights in the ſky, and to deſcry in heaven bright ſtars, unſeen before, and other celeſtial novelties and beauties.

Sect. 12. I DENY not, *Lindamor*, that God has been pleaſed to adorn ſome of his creatures with hints and impreſſes, as well of his lovelineſs as his other excellences; but they do much more faintly and imperfectly reſemble him, than the counterfeit ſun we ſometimes ſee in a cloud emulates the true one, by whoſe refracted and reflected beams, ſome acute modern naturaliſts ſuppoſe it to be produced. For though this derivative ſun ſhine with a not-inconſiderable luſtre, comparatively to the reſt of the dark cloud it ennobles; yet is it not only as much inferior to the true ſun in brightneſs, as in height, but it enjoys alone a precarious, dependent, and almoſt momentary being, which often it loſes in a very ſhort time, and never attains to preſerve a very long one. And therefore, to neglect that ſupreme reſplendency, that ſhines in God, for thoſe dim repreſentations of it, that we ſo doat on in the creature, is as prepoſterous and abſurd, as it were for a Perſian to offer his ſacrifice to a parhelion (as the Greeks call that meteor) inſtead of adoring the ſun. And certainly, *Lindamor*, if 2 Kings vi. 17. our dim ſight, like that of the prophet's ſervant, who ſaw the mountains near *Dothan* covered with horſes and chariots of fire, were privileged to diſcern otherwiſe inviſible objects; our raviſhed eyes would behold lovelineſs enough in God, to make us in- Zech. ix. 17. ceſſantly exclaim in the language of the prophet, *How great is his goodneſs, and how great is his beauty!* Agreeably whereunto we may obſerve, that as the loadſtone doth attract moſt powerfully the brighteſt needles, made of the pureſt ſteel, and thoſe that moſt approach it; ſo angels, who, of all created beings, enjoy the unclouded'ſt light, and the moſt clear knowledge of their Maker, do love him with a conſtancy ſo fixed, that in five thouſand and ſome odd centuries of years (effluxed ſince the creation) they could never ſee any thing, either in God, or out of him, capable to ſeduce them to a change; God's lovelineſs (which is ſuch, that the eternal fruition of himſelf creates even his felicity) is like rare muſic, which, though it do delight all its partakers, the knowingeſt artiſts ſtill do highlieſt value, and are moſt raviſhed with the tranſports it produceth, being proportioned to the degrees of the ſkilfulneſs of its admirers. The apoſtate ſpirits indeed deſerted their firſt ſtation, not frighted thence by any blemiſh they deſcried in God; but probably fell by a ſaucy affectation of a parity; and (as divines tell us) a ſacrilegious aſpiring to perfections, which they were criminally unable to behold, without wiſhing them their own. And for their preſent ſeparation from God, it is their curſe and puniſhment. And I ſhall ingenuouſly confeſs to you, *Lindamor*, that by the advantageous ideas I entertained of God's perfections, I have been ſometimes inclined to think, that the reaſon why God tells *Moſes*, *Thou canſt not ſee my face, for there ſhall no man ſee me and live*, might be, that as tranſcendent objects deſtroy the ſenſe, ſo lovely and glorious a ſight (whoſe continuance ſhall make our happineſs in heaven) would let in joys, and would create deſires, too mighty for frail mortality to ſuſtain. The raviſhed ſoul being ſhewn ſuch game as that, would hate ſo eagerly, that ſhe would break thoſe leaſhes, that tie her to the body (and thereby hinder her flight to that wiſhed union); and the glad heart (too narrow a receptacle for ſo much joy) to make room for ſuch gueſts, would ſtretch unto a rupture. Longing or joy have ſometimes ſingly reached degrees, that have made them fatal; and why then ſhould their union, in ſuch extremes, be thought uncapable of producing the like effect? Nor is it, perchance, more a privilege of the next life, that we ſhall then ſee God, than that we ſhall ſurvive that glorious ſight; which is too dazzling an object for mortal eyes, till (to uſe St. *Paul*'s expreſſion) *our mortal ſhall have put on immortality*. But I muſt no longer loſe myſelf in a theme, on which it is ſo eaſy to ſpeak much, and ſo impoſſible to ſay enough. If I be not very much miſtaken, they are ſo, who preſume to give us ſatisfactory definitions of God's nature, which we may perhaps more ſafely define by the impoſſibility of its being accurately defined. And I muſt confeſs, I do the leaſt expect the beſt deſcription of the divine nature from thoſe, that are moſt forward to take upon them to explicate it; for our words being but the repreſentations of our notions, and they being neceſſarily finite, as our being; few men are (methinks) more likely to be miſtaken in the nature of what is infinite (and conſequently of God's attributes) than thoſe that think deſcriptions can comprize it. Nor will an aſſiduity and conſtancy of our ſpeculations herein relieve us; for too fixed a contemplation of God's eſſence does but the more confound us. As I remember St. *Paul* tells the *Jews*, that *he could not ſee for the glory, or that light*, that ſhone from heaven about him at his converſion. And this the famous *Simonides* experimentally found, who being aſked what God was, took a day's time to conſider of it; after being called upon for his anſwer, he deſired two days longer to think upon it; and, when they were expired, demanded four days longer; and ſo continued a great while, each time increaſing his demands for reſpite, till, being preſt to give an account of ſuch a dilatory way of proceeding, he ingenuouſly acknowledged, that the longer he conſidered God's nature, the leſs he comprehended it. And indeed, experience teaches us, that they, that gaze ſtedfaſtlieſt on the noon-day's ſun, can leaſt of all diſcern what it is, being rather dazzled than inſtructed by ſo confounding an object. Nor, is it only, *Lindamor*, a taſk of too great difficulty for a mere man, perfectly to explicate that incomprehenſible nature of God, to which nothing but his own infinite underſtanding can be adequate; but, methinks, even to celebrate God's perfections, cannot be ſoberly attempted, without a very deep ſenſe of a man's own unworthineſs and incapacity. For, the ſame excellencies, that furniſh us with praiſes, do tranſcend them. And the fruitfulneſs of the ſubject may as well deter, as invite our pens; ſince, as we are ſure we ſhall not want theme, ſo we are certain we cannot dignify it. And for my part, *Lindamor*, though my ſubject exacted of me the praiſes I have endeavoured to aſcribe to God, yet I ſhould have thought it rather to decline the laws of method, than be

Exod. xxxiii. 20.

1 Cor. xv. 54.

Acts xxii. 6, 11.

reduced to derogate from what I would extol; if the neceſſity of detracting from God's perfections were not equal to that of mentioning them; and if that neceſſity were not as glorious to God, as greater praiſes than we are able to aſcribe him, could be. But, *Lindamor*, having thus done right to my method, though I could not to my theme, I ſhall only invite you to imitate with me thoſe Perſians, that adored the ſun, thought the light he lent them ſerved but to make the ſource of it admired, and not to pry into his abſtruſe eſſence with it. And though I might ſay much more concerning God's perfections, I muſt henceforth think ſilence the propereſt language I can now employ; for it is ſilence, that beſt expreſſes our wonder: and ſure, wonder is never more ſeaſonable than when God is the object; a proſtrate veneration being the ſafeſt apprehenſion of him, that is incomprehenſible.

Sect. 13. HAVING thus conſidered, *Lindamor*, how fit an object God is of our higheſt love for what he is in himſelf; let us now proceed to derive further proofs of the ſame truth from what he is to us (that your gratitude may contend with your reaſon, which ſhall moſt heighten your devotion) and we ſhall find in the vaſtneſs, freeneſs, diſintereſtedneſs, conſtancy, and advantageouſneſs of his love to us, that more than all the love we can pay him were but a little part of that we owe him.

BUT for the firſt attribute, we have aſſigned his love (the greatneſs of it) it being a general property, diffuſed through all the reſt, and conſpicuous in them, it requires now to be treated of apart.

WE ſhall then proceed to the freeneſs or unmeritedneſs of God's love; to believe which ſtrangely vaſt, we need but conſider, that we ſo little could at firſt deſerve his love, that he loved us even before we had a being; and our felicity in his decrees preceded our exiſtence in this world. God loved you numerous ages before you were; and his goodneſs is ſo entirely its own motive, that even your creation (ſince when, alone you can pretend to merit his love) is the effect of it. This benefit alone were ſufficient to render God the object of our love, though we were that of his averſion. For (as the Perſians adored the ſun even when it ſcorched them) we eſteem ourſelves obliged to love and honour our parents, in ſpight of their being wicked and unkind, though they be but God's inſtruments in our production, and made us what we were born, not arbitrarily, but in virtue of his ordination. But God, to confer on us, in the moſt excellent and endearing manner, the bleſſing promiſed to his ancient people, when he vouchſafed to aſſure them, that *he would love them freely*, was pleaſed to love us, not only when we were not at all, but when we were his enemies; *If when we were enemies* (ſaith St. *Paul*) *we were reconciled to God by the death of his Son*, &c. Our inexiſtence indeed was a condition, wherein nothing in us was capable of being a motive of God's love: but our enmity proceeded farther, and made us worthy of his deteſtation; as if his love were nothing unleſs it vanquiſhed obſtacles, as well as wanted motives. This gave the Apoſtle a juſt cauſe to ſay, that *God commendeth his love towards us, in that while we were yet ſinners Chriſt died for us:* that is, when we wanted all motives to invite his love, unleſs our very want of them ſhould paſs for one. And how did God expreſs his love unto us? even by the gift of the Son of his love; *For God ſo loved the world*; (ſays the divine token of his love) *that he gave his only begotten Son.* And how did that Son love it? *He* (ſays the Apoſtle) *being in the form of God, thought it no robbery to be equal with God; but made himſelf of no reputation, and took upon him the form of a ſervant, and was made in the likeneſs of men. And being found in faſhion as a man, he humbled himſelf and became obedient unto death, even the death of the croſs.* That is, that he would love at no leſs rate than death; and, from the ſupereminent height of glory, ſtooped and abaſed himſelf

Hoſ. xiv. 4.
Rom. v. 10.
Rom. v. 4.
John. iii. 16.
Phil. ii. 6, 7, 8.

ſelf to the ſufferance of the extremeſt of indignities, and ſunk himſelf to the bottom
of abjectedneſs, to exalt our condition to the contrary extreme. *He was wounded* Iſai. liii. 5.
for our tranſgreſſions, he was bruiſed for our iniquities, the chaſtiſement of our peace was
upon him, and with his ſtripes we are healed, ſays the Prophet. *For ye know the grace* 2 Cor. viii. 9.
of our Lord Jeſus Chriſt, that though he was rich, yet for your ſakes he became poor, that
ye through his poverty might be made rich, ſays the Apoſtle. Men having diſpleaſed
God, and conſequently forfeited all right and natural poſſibility to happineſs; even
whilſt they compleated the forlornneſs of their condition, by the lethargy of not being
ſenſible of it, and were as careleſs to ſeek means of recovery, as they had been unable
to deviſe them of themſelves; even then, his reſtleſs love would never be at quiet,
till it had ſet his omniſcience on work, to contrive expedients, and find out a way to
reconcile his juſtice and his mercy, in reconciling ſinners to himſelf. And this merciful
deſign, by the incarnation of his Son, he proſecuted in a way ſo worthy of himſelf,
and ſo advantageous to us, that our juſt wonder at it may keep us from having
any, to find that, as St. *Peter* informs us, the very angels (prompted, queſtionleſs,
by a religious curioſity) ardently deſire to look into thoſe divine myſteries. I find it 1 Pet. i. 12.
hotly diſputed amongſt divines (not only betwixt the Socinians and the Orthodox,
but betwixt Orthodox and Orthodox) whether or no God could, without violating his
juſtice, have deviſed any other courſe for the expiation of ſin, than the paſſion and
death of Chriſt. But, without venturing to determine, whether or no God could,
to redeem us, have choſen any other way; we may ſafely think, that he has choſen
the moſt obliging and moſt endearing way; diſplaying in this divine manner of reſcuing
us the ſevereſt juſtice, and the higheſt mercy; the greateſt hatred of ſin, and
the greateſt love to Sinners; ſince by thoſe unequalled and unvaluable ſufferings, to
which he delivered up, for us, that Son, who is near unto him, that he truly ſaid,
I and the Father are one, he at once manifeſted, both how much he hated ſin, which John x. 30.
he ſo heavily puniſhed in the perſon he moſt loved (though that ſurety but adopted
it, to free men from the inſupportable vengeance of it) and how much he loved
ſinners, by giving up what he ſo loved, for a ranſom of thoſe that were guilty of
what he ſo hated. And therefore our Saviour, though he did ſuch great things to
ſatisfy the unbelieving and contumacious Jews of his being their promiſed Meſſiah,
would not decline death to convince them; and, though he had not ſeldom done ſo
much to make himſelf the object of their faith, would not be invited from the croſs,
though the chief prieſts and ſcribes themſelves ſaid at his crucifixion, *Let him now* Matt. xxiii. 7.
come down from the croſs, and we will believe on him. And Chriſt, to convince the
world of their unableneſs to emerge and recover out of that deep abyſs, wherein the
load of ſin (which, in ſcripture, is called *a weight*) had precipitated fallen man, Heb. xii. 1.
came not into the world, until well nigh 4000 years of ſickneſs had made the diſeaſe
deſperate, and the cure almoſt hopeleſs. So inveterate an obſtinacy at once widening
the diſtance betwixt God and man, and proclaiming the latter's diſability to find, by
his own wiſdom, expedients of reunion. Thus Chriſt healed and diſpoſſeſſed a dumb Mat. ix. 32.
perſon, who was able to make intreaties but by the diſability of pronouncing them;
and might truly ſay to the ſecure world, *I am found of them, that ſought me not.* And Iſai. lxv. 1.
when our Saviour was come into the wretched world, of all the numerous miracles
recorded in the goſpel, he ſcarce did any for his own private relief. And to ſhew,
that as he endured his ſorrows for our ſakes, that *by his ſtripes we might be healed*; Iſai. liii. 5.
ſo were the joys he taſted in relation to us. We read not (which is highly obſervable)
in the whole goſpel, that ever he rejoiced but once; and that was, when his returned
diſciples informed him, that they had victoriouſly *chaſed devils* and *diſeaſes* out of Luke x.

oppreſſed mortals, and that by his *authority* men had been *diſpoſſeſſed* of both the temper
and puniſhment of ſin. He converſed among his contemporaries with virtues, as
well atteſting what he was, as prophecies and miracles could do; and to teach man,
how much he valued him above thoſe creatures, that man makes his idols, he often
altered and ſuſpended the courſe of nature for man's inſtruction, or his relief, and
Mat. xii. reverſed the laws eſtabliſhed in the univerſe, to engage men to obey thoſe of God, by
24. doing miracles ſo numerous and great, that the Jews unbelief may be almoſt counted
Mat. iii. 12. one. Yet were thoſe wonders wrought for a generation, that aſcribed them to the
devil, and returned them with ſo unexemplified an ingratitude, that it is not the
leaſt of his wonders, that he would vouchſafe to work any of them for ſuch blaſphemous
wretches; who were indeed, as ſome of the later Jews have too truly ſtyled
themſelves, in relation to their fathers, *Chometz ben ya-yin*, vinegar the child of wine,
a moſt degenerate offſpring of holy progenitors. He ſuffered ſo much for them, that
made him do ſo, that he ſuffered the addition of miſery of being thought to ſuffer deſervedly;
Iſai. liii. 12. *And he was numbred with the tranſgreſſors.* And though he lived as much
a miracle as any he did, yet did his condition ſometimes appear ſo deſpicable and forlorn,
that men could not know his deity but by his goodneſs, which was too infinite
not to belong incommunicably to God. And though it were once a ſaying of our
John xv. Saviour's, *Greater love hath no man than this, that a man lay down his life for his friends*;
13. yet is not what is ſaid of the love here mentioned, to be underſtood of love indefinitely,
or generally conſidered, but only of the ſingle acts or expreſſions of a man's
love to his friends compared betwixt themſelves. And ſo the alleged paſſage ſeems
to mean but this, that among the ſingle acts of kindneſs to a man's friends, there is
not any one more highly expreſſive of a real and ſincere love, than to part with one's
life for their ſakes. This text therefore would not be indefinitely applied to the
affection of love itſelf, as if it could not poſſibly be greater than is requiſite to make
a man content or willing to die for his friends; for he, that ſacrifices, beſides his life,
his fortune alſo, his children, and his reputation, does thereby expreſs more love to
them, than he could do by parting with his life only for them. And he that is forward
to die for thoſe, that hate him, or, at leaſt, know him not, diſcloſes a more
plentiful and exuberant ſtock of love, than he that does the ſame kindneſs but for
thoſe that love him. And thus our Saviour would be underſtood, unleſs we would
ſay, that he out-practiſed what he taught; for, he came to lay down his life even for
his enemies, and (like the kind balſam tree, whoſe healing wounds weep ſovereign
balm to cure thoſe that made them) he refuſed not to die for thoſe that killed him,
and ſhed his blood for ſome of thoſe that ſpilt it. And ſo little was his injured love
to the ungrateful world diſcouraged or impaired by the ſavage entertainment he met
with in it, that, after he had ſuffered from wretched men (for whoſe ſakes he left
heaven to become capable of ſuffering) ſuch barbarous indignities, as might have
made bare puniſhments appear mercy, and even cruelty itſelf ſeem no more than
juſtice; when, I ſay, to hope for ſo much as his pardon were preſumption, he was
pleaſed to create confidence of no leſs than his love, a virtue. Nor think it, *Lindamor*,
impertinent to our preſent theme, that I inſiſt ſo much on what Chriſt has
John x. 30. done and ſuffered for us, ſince both he himſelf informs us, that *he and his Father are*
See alſo John *one*; and ſome of the texts already mentioned have taught us, that it was an effect of
xiv. *ver.* God's love alſo to the world, that *he gave his only begotten Son to redeem it*; and, that
9, 10, 11. John iii. 16. *God commendeth his love towards us, in that while we were yet ſinners Chriſt died for us.*
Rom. v. 8. Wherefore I ſhall, without ſcruple, proceed to obſerve to you, that ſo free is Chriſt's
dilection, that the grand condition of our felicity is our belief, that he is diſpoſed to

make us happy, on terms, not only ſo honourable to him, but ſo advantageous to us, that, I was about to ſay, that poſſibly faith itſelf would ſcarce be exacted as requiſite to our happineſs, but that the condition does increaſe the benefit, by vouchſafing us bold and early anticipations of it: for, *faith* being (as the Apoſtle terms it) *the ſubſtance of things hoped for, and evidence* (or conviction) *of things not ſeen*, wafts our joys to this ſide of the grave, bows heaven down to us, till our freed ſpirits can ſoar up to heaven; and does us ſuch a ſervice, as the Jewiſh ſpies did to their countrymen, by bringing them over to this ſide *Jordan* into the wilderneſs, ſome of the pleaſant and delicious fruits of the bleſſed Land of Promiſe. I ſaid, *Lindamor*, that faith was the grand condition required in God's free grant of eternal life. Not that I would aſcribe any thing to a lazy, ſpeculative, and barren faith, in oppoſition to that lively and active one, which is called by the Apoſtle, *πίστις δι᾽ ἀγάπης ἐνεργουμένη*, *faith operating by love*; ſince I am informed by St. *James*, that the divorce of faith and works is as deſtructive to religion, as that of ſoul and body is to life: but that I was willing to mind you, that though true faith (which cries like *Rachel*, *give me children or elſe I die*) be ever the pregnant mother of good works, yet are not thoſe works the cauſe, but the effects and ſigns of God's firſt love to men (however afterward the children may nurſe their parents.) As, though the needle's pointing at the poles be, by being an effect, an argument of its having been invigorated by the loadſtone, or received influence from ſome other magnetic body; yet is not that reſpect unto the north the cauſe, but the operation of the iron's being drawn by the attractive mineral, *thou art good, and doſt good*, ſays the Pſalmiſt to his Maker. The greatneſs of his goodneſs is that, which makes it ours; nor doth he do us good, becauſe that we are good, but becauſe he is liberally ſo; as the ſun ſhines on dunghills, not out of any invitation his beams find there, but becauſe it is his nature to be diffuſive of his light; yet with this difference, that whereas the ſun's bounty, by being rather an advantage to us than a favour, deſerves our joy, and not our thanks; becauſe his viſits are made deſignleſsly, and without any particular intention of addreſs (by ſuch a bare neceſſity of nature, as that which makes ſprings flow out into ſtreams, when their beds are too narrow to contain the renewed water that doth inceſſantly ſwell the exuberant ſources;) God, on the contrary, for being neceſſarily kind, is not leſs freely or obligingly ſo, to you, or me. For, though ſome kind of communicativeneſs be eſſential to his goodneſs, yet his extenſion of it without himſelf, and his vouchſafing it to this or that particular perſon, are purely arbitrary. To omit his love to the numberleſs elect angels; the ſtrict relations betwixt the perſons of the bleſſed Trinity, ſupplying God with internal objects, which employed his kindneſs before the creation, and himſelf being able to allow his goodneſs the extent of infinity for its diffuſion. But (having glanced at this only by the by) we may yet further admiringly obſerve, that whereas men uſually give freelieſt where they have not given before, and make it both the motive and excuſe of their deſiſtance from giving any more, that they have given already; God's bounty hath a very different method: for he uſes to give, becauſe he hath given, and that he may give. Conſonantly to which, when the revolting Iſraelites had broken the contents, whilſt *Moſes* was bringing them the tables of the Law, and had thereby provoked the incenſed giver of it to the thoughts of a ſudden extirpation of ſo ingrateful and rebellious a people; we may obſerve, that, whereas God, as unwilling to remember his former goodneſs to them, ſpeaking to *Moſes*, calls them, *Thy people which thou broughteſt out of the land of Egypt*: *Moſes*, on the other ſide, to engage God to the new mercy of a pardon, repreſents to God his former mercy to them, and calls them *God's people*, which *he brought*

Heb. xi. 1.

Numb. xiii. 23, 27.

Gal. v. 6.

Jam. ii. 26.

Gen. xxx.

Pſal. cxix. 68.

Exod. xxxii. 7.

Verſe 12.

brought forth out of the land of Egypt, with great power, and with a mighty hand. And ſo conſpicuous in the eternal Son was this property of the merciful Father, that when ſick *Lazarus*'s ſiſters implored his reſcue for their expiring brother, the motive John xi. 3. they employed, and which proſpered their addreſſes, was, *Lord, behold* (not, he who loveth thee, but) *he whom thou loveſt is ſick.* And as he takes the firſt inducements of his bounty from himſelf; ſo do his former favours both invite and give rates to his ſucceeding bleſſings. And there is reaſon for it; for his pure love being all the merit, by which man can pretend to the effects of his bounty, it is but juſt, that the degree of his love ſhould proportion thoſe favours, which it is our only title to; and that God's liberality ſhould as well afford meaſures as motives to itſelf.

Sect. 14. Nor is God's love leſs diſintereſted than free. His grand deſign upon us is but to make us inſtruments and partakers of his glory, and to bring us to everlaſting happineſs, by a way, that does as well elevate and dignify our nature, as the condition reſerved for us will. His method of ſaving us, if but complied with, does here, as Col. i. 12. the Apoſtle ſpeaks, *fit us for the inheritance of the Saints in light*; we being made (as 2 Pet. i. 4. St. *Peter* ſpeaks) *partakers of the divine nature, having eſcaped the corruption that is in the world through luſt.* So that thoſe things, wherein the nobleſt of the philoſophers placed their felicity, ſerve but to qualify and prepare Chriſtians for that higher bleſſedneſs, that is reſerved by God for thoſe that love him; and cannot but be heightened and endeared by the value, which graces and virtues had given men on earth, for ſuch a noble and rational kind of happineſs, as is apportioned to them in heaven. What ends can he have upon us, whoſe goodneſs and his bleſſedneſs are both infinite? He was inconceivably happy (in his own ſelf-ſufficiency) before the creatures had a being; and ſure that felicity, that needed not themſelves to be ſupreme, needs nothing that they can do *. Nor was it his indigence, that forced him to make the world, thereby to make new acquiſitions; but his goodneſs, that preſſed him to maniſeſt, and to impart his glory, and the goods, which he ſo overflowingly abounds with. Witneſs his ſuſpenſion of the world's creation, which certainly had had an earlier date, were the Deity capable of want, and the creatures of ſupplying it. St. *Paul*, in his epiſtle 1 Tim. i. 11. to *Timothy*, ſtyles God Μακάριος, which we tranſlate *the bleſſed God*, but may, per- 1 Tim. vi. 15. haps, more properly be rendered *the happy God*: and elſewhere, in the ſame epiſtle, Acts xxvii. 24, 25, 26. he truly calls him *the happy*, as well as *only potentate. God* (ſays the Apoſtle) *that made the world, and all things therein, ſeeing that he is Lord of heaven and earth, dwelleth not*, &c. *As though he needed any thing, ſeeing that he giveth to all life, and breath, and all things.* And, *in him we live, and move, and have our being.* And indeed ſo coherent in the mind of a meer man, that does but conſider and underſtand the import of his own notions, is the belief of God's happineſs to that of his being, that I remember the Epicurean, *Lucretius* himſelf, even in that impious paſſage, where he denies divine providence, and in a ſeeming, but injurious complement, would, under the pretence of eaſing God of it, deprive him of the government of the world, does yet confeſs, that the divine nature muſt neceſſarily enjoy a ſupreme and endleſs tranquillity; adding (to bring this to our preſent purpoſe) that it is

——*Privata dolore omni, privata periclis,*
Ipſa ſuis pollens opibus, nihil indiga noſtri.

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* —— *Quid enim immortalibus atque beatis,*
Gratia noſtra queat largirier emolumenti,
Ut noſtrâ quidquam cauſâ gerere aggrederentur? Lucret. ex Epic.

Whereby he acknowledges,

‘ That from all griefs and dangers of them freed
‘ Rich in itſelf, it has of us no need.

Or, if you will have him ſpeak of the gods in the plural, like a heathen poet, that

‘ Far above griefs and dangers, thoſe bleſt powers,
‘ Rich in their native goods, need none of ours.

A MUCH nobler poet tells us, that *the earth is the Lord's, and the fulneſs thereof, the world, and they that dwell therein.* Agreeably whereunto, that *great God, that formed all things* (as in our tranſlation the ſcripture calls him) ſays in one of the Pſalms, *If I were hungry, I would not tell thee, for the world is mine, and the fulneſs thereof.* His ubiquity excludes all wiſhes of remove, by making his eſſence incapable of excluſion; for, whither ſhould he deſire to tranſport himſelf, that is every where, and can wiſh himſelf in no place, where he is not already? His ſufficiency is ſuch, that he can ſee no goods, but what he gives or hath (or rather both beſtows and poſſeſſes) his plenty being ſo unexhauſted a ſpring of goods, that his liberality does leſs impoveriſh God, than the ſun's light does him, or imparted knowlege impairs the teacher's ſtock. And therefore, though St. *James* does very juſtly call God *the father of lights*, who is the beſtower of every good and every perfect gift; yet the *friend of God* (as the ſcripture calls *Abraham*) and that royal prieſt (whom the writer to the Hebrews teaches us to have been ſo illuſtrious a type of him, whom he calls *the high prieſt of our profeſſion*) do both of them, in the ſame chapter, ſtyle him, *The poſſeſſor of heaven and earth.* No, no, God needs not beg from, nor covet in the creatures ſhallow ſtreams, thoſe goods, of which he not only hath, but is the ſource. Our greateſt ſervices to our Creator muſt be to diſcharge ourſelves, not to advantage him, nor as thinking to add any thing to a felicity, which were not infinite, could it admit increaſe. Our higheſt performances, though they be dues, amount not unto tributes, but are rather like thoſe pepper-corns of rent, which freeholders pay, not with hope or with intent to enrich their landlord, but to acknowledge, that they hold all from him. When we admire the ſun, our ſeeing of his light doth not increaſe it; it makes it not greater, but only makes it ours; and when we turn away, or ſhut our eyes, that glorious planet ſuffers no eclipſe, and is not at all darkened or impaired, nor doth he thereby loſe his light, but we: the eaſineſs of the application requires, and excuſes its omiſſion. *If thou ſinneſt* (ſays *Elihu* in *Job*) *what doſt thou againſt him? if thou be righteous, what giveſt thou him? or what receiveth he of thine hand? thy wickedneſs may hurt a man, as thou art, and thy righteouſneſs may profit the ſon of man.* In effect, the wicked's ſpite againſt God is but like a madman's running his head againſt the wall, that leaves the wall unſhaken, but daſhes his own brains out. God inhabits a felicity (as well as light) inacceſſible to all inferiour attempts: his ſovereign tranquillity is ſo ſublimely placed, that it is above the reach of all diſquieting impreſſions; and, like the ſtars, that feel not the diſeaſes their inauſpicious influence produces, he doth not reſent the torments he inflicts. God's juſtice is not leſs eſſential to him than his mercy; witneſs that (the numbers of the ſaints and the reprobate conſidered) thouſands fall ſacrifices to the ſeverer attribute, for one that proves capable of the milder. He ſaid, *He would get himſelf honour upon* Pharaoh *and all his hoſt*, when he deſigned their ruin in the *Red Sea*: and *Moſes* ſaid, *he hath*

Pſal. xxiv. 1.
Prov. xxvi. 10.
Pſal. l. 12.
Jam. i. 17.
Jam. ii. 23.
Heb. vii. *per totum.*
Heb. iii. 1.
Gen. xiv. 18, 22.
Job xxxv. 6, 7, 8.
1 Tim. vi. 16.
Exod. xiv. 17, 18.
Exod. xv. 1.

triumphed gloriouſly in effecting it. And in *Ezekiel* he ſays, *Behold I am againſt thee,*
Ezek. xxviii. 22. O Sidon, *and I will be glorified in the midſt of thee, and they ſhall know, that I am the*
Lord, when I ſhall have executed my judgments in her, and ſhall be ſanctified in her. Thus,
when a flaſh of God's indignation (kindled by that ſtrange fire they preſumed to offer
Levit x. 3. before him) had blaſted the two preſumptuous ſons of *Aaron*, God is ſaid *to have been*
ſanctified in them that come nigh him, and to have been glorified before all the people; ſuch
eminent and exemplary inſtances of ſeverity manifeſting him to be ſo holy in his laws,
and ſo concerned for them, that even the miniſters of his altars ſhall not violate them
Heb. xii. 29. with impunity, but find him (what the writer to the *Hebrews* calls him) *a conſuming*
fire; who will be glorified before all his people, either by the obedience of thoſe
that approach him, or by their deſtruction. So to evidence, that God can derive
ſatisfaction as well from the exerciſe of provoked juſtice, as from that of his provoked
Deut. xxviii. 63. mercy, the ſacred orator uſes this remarkable *antanaclaſis: And it ſhall come to paſs,*
that as the Lord rejoiced over you to do you good, and multiply you; ſo will the Lord rejoice
over you to deſtroy you, and bring you to naught. Thus, though it be truly ſaid of
Lam. iii. 33. God by the prophet *Jeremiah*, that *he doth not afflict willingly, nor grieve the children*
of men; (and therefore the determined conſumption of the whole land, which our
Iſa. xxviii. 21. bibles engliſh, *God's work is ſtrange work*, other tranſlators read, *Opus alienum ſuum*):
yet when the ſins of incorrigible offenders are grown to that provoking height, that
his mercy intercedes no more to avert or ſuſpend the inflictions of his juſtice, than
how much he can ſatisfy himſelf in deſtroying thoſe, that would not be preſerved,
may be gueſſed at by that formidable expreſſion in *Ezekiel*; where, having foretold
what havock the ſword, the famine, and the peſtilence ſhould make amongſt the intractable
Ezek. xiii. 5. and diſperſed Iſraelites, he adds (as a kind of 'Επινίκιον) *thus ſhall mine anger*
be accompliſhed, and I will cauſe my fury to reſt upon them, and I will be comforted. The
howlings of the damned as well ſound forth his praiſes, as do the hallelujahs of the
ſaints; they both do ſing to him an everlaſting canticle of praiſe: only in this great
concert of his whole intelligent creation, the deſignleſsly conſpiring voices are as differing
as the conditions of the reſpective ſingers. Hell's darkneſs doth as well contribute
to God's glory, as heaven's eternal ſplendor; as ſhadows, judiciouſly placed,
do no leſs praiſe the painter, than do the livelier and brighter colours. And as when
the earth doth ſend black, noiſome, and ſulphureous exhalations up toward the ſky,
alas, they reach not heaven, nor diſcompoſe the ſpheres; but all the ſtorms and thunders
they produce fall on that globe they came from, and there do all their miſchief:
ſo, the wicked may wrong God indeed, yet do they really harm but themſelves by all
their greateſt ſins, which trouble him chiefly, but becauſe they neceſſitate him to
puniſh them; for the tranſgreſſions, that do moſt provoke God, do him not the leaſt
harm. An impious perſon may (as *Elihu* lately informed us) hurt a man as himſelf
is, not that ſupremely bleſſed Deity; the reſult of whoſe infinite perfections is a
reſembling happineſs, which is as inſeparable from him as his eſſence. Our offences
may derogate from his acceſſional glory, not from his eſſential felicity; or rather,
the moſt deſperate ſinners, by their greateſt crimes, can but change the attribute they
ſhould bring honour to, and but oppoſe the glorifying of his goodneſs to occaſion
the glorifying of his juſtice; ſince he will be infallibly glorified, ſoon or late, either
by men's actions, or their ſufferings; by their practice of duties, or puniſhment for
ſin. Thus you ſee, how little God is beholden to you for your declining hell; nor
will the ſcore be very much encreaſed by your addreſſes and attempts for heaven.
Job. xxii. 2, 3. *Can a man* (ſays *Eliphaz*) *be profitable unto God, as he that is wiſe may be profitable unto*
himſelf? Is it any pleaſure to the Almighty, that thou art righteous? or is it gain unto
him,

him, that thou makeſt thy ways perfect? Congruouſly to which ſenſe the Pſalmiſt ſays, Pſal. xvi. 2. *my goodneſs extendeth not to thee.* The fire, that we kindle on God's altars, heats and enlightens us, but warms not heaven at ſo diſtant a remove, nor is wanted in the ſun's reſidence. We have all the redolence of the perfumes and incenſe we burn upon his altars; the ſmoke doth vaniſh ere it can reach the ſky, and whilſt it is undiſperſed, but clouds and but obſcures it. Alas, our beſt performances are as uſeleſs ſervices to God, as the heir's bringing wax to his departing father is to him; which adds not any thing to the rich man's ſtore, and is by him deſired and accepted, only to ſeal away a fortune to his ſon. Though therefore it be true, that God is pleaſed with our performances, yet is that welcome he vouchſafes to give them ſo far from enabling us by them to requite his love, that it increaſes the unrequitedneſs of it; ſince he is delighted with them, as they afford him juſt riſes to reward them. How far from mercenary is then God's bounty! ſince he accepts our acknowledgments of his former bleſſings (chiefly) to make them opportunities of conferring freſh ones; as good old *Iſaac* deſired his ſon's veniſon, that from the reliſhing of that ſavory meat he might Gen. xxvii. 4. take an opportunity to bleſs him.

AND the better to diſcover, how diſintereſted God's favours are, let us further conſider, how little they are requitable; for we can give him nothing but his own (nor, heaven knows, all that neither:) and both the will and power to ſerve him are his, upon ſo juſt and many ſcores, that we are unable to retribute, unleſs we do reſtore; and all the duties we can pay our maker are leſs properly requitals than reſtitutions. When *David* and his officers had offered towards the ſtructure of that magnificent temple, which they ſeemed ambitious to make a manſion inferiour to heaven only; the king himſelf gave three thouſand talents of gold, and ſeven of refined ſilver; and the heads of the people five thouſand talents (beſides ten thouſand drachms) of gold, ten thouſand talents of ſilver, eighteen thouſand of braſs, and a hundred thouſand of iron (a treaſure, of which I ſcarce remember to have read the like in any hiſtory) beſides a number of all manner of precious ſtones, capable of impoveriſhing the very *Indies*. They perfumed this noble and unequalled offering with a ſolemn confeſſion, which perhaps, in God's eſteem was much more precious than it: *Thine, O Lord* (ſays the royal prophet in the name of all) *is the greatneſs,* 1 Chron. xxix. 11—16. *and the power, and the glory, and the victory, and the majeſty; for all that is in heaven and in earth is thine. Thine is the kingdom, O Lord, and thou art exalted as head above all. Both riches and honour come of thee, and thou reigneſt over all. And in thine hand is power and might; and in thine hand it is to make great, and to give ſtrength unto all. Now therefore, our God, we thank thee, and praiſe thy glorious name. But who am I? and what is my people, that we ſhould be able to offer ſo willingly after this ſort? for all things come of thee, and of thine own have we given thee. For we are ſtrangers before thee, and ſojourners, as were all our fathers. Our days on earth are as a ſhadow, and there is none abiding. O Lord our God, all this ſtore, that we have prepared to build thee an houſe, for thy holy name, cometh of thine hand, and is all thine own. Who* (ſays the Rom. xi. 35. 36. Apoſtle, in a queſtion that imports its own negative) *hath firſt given to him, and it ſhall be recompenſed to him again? for of him, and through him, and to him, are all things.* Nay, even our love itſelf (that poor man's ſurety and exchequer, that doth pay all his debts by ſupplying him with the prerogative to coin his deſires and wiſhes of an arbitrary value) is here unable to diſcharge our debts; our love being too much the effect, to be capable of being the recompenſe of God's. And ſurely, the divine amoriſts had cauſe to ſay, that *herein is love, not that we loved God, but that he loved* 1 John iv. 10. *us.* And, as the ſame Apoſtle elſewhere ſpeaks, *we love him, becauſe he firſt loved us.* 1 John iv. 19.

If, in effect, we look upon the unworthy conteſt betwixt God's mercies and moſt men's ingratitude, and but reflect upon the ſmall return of love that the greateſt diſburſements of his do uſually bring home; we cannot but acknowledge (as *David* in the lately mentioned ſcripture did) that our loving God for his favours, is one of the greateſt favours that we love him for. So unrequitable is God's love, and ſo inſolvent are we, that that love vaſtly improves the benefit, by which alone we might have pretended to ſome ability of retribution; and ſo unlimited is this impotence of ours to recompenſe or repay God's dilection, that it extends to, and fetters our very wiſhes. For God enjoys an affluence of felicity, ſo perfect and entire, that even our wiſhes can aim at nothing for him worthy of him, unleſs inſtructed by what he already actually poſſeſſes; and the ſenſe of this ſame very impotence, to ſome of the greateſt proficients in *Seraphic Love*, appears not the leaſt uneaſy property of it. It grieves us ſenſibly, to ſee ourſelves reduced to be only paſſive, and the receivers in this commerce. We would fain contribute ſomething, and cannot always refrain from devoting our wiſhes to increaſe his happineſs, to whom we owe all ours. And ſome holy perſons (particularly St. *Auſtin*) have, by the exuberance of their gratitude and devotion, been tranſported to make wiſhes, and uſe expreſſions, wherein their affections had a greater ſhare than their reaſon, and which argued them much better to apprehend, how much God deſerved of them, than how little he needed them. But, upon ſecond thoughts, we ſhall find, that the cauſe of our grief ought to turn it into our joy, ſince the deſires we would frame, aiming at God's being infinitely happy, are all fulfilled before they are conceived, and that in the moſt advantageous and nobleſt way; for could God's happineſs admit acceſſion by our accompliſhed wiſhes, there were then a poſſibility of his wanting ſomething to render it compleat. And ſure, it is a more ſupreme felicity to be by nature tranſcendently above all increaſe of bleſſedneſs, than to receive the greateſt, that men can wiſh.

Sect. 16. To proceed now to the conſtancy of God's love; we cannot entertain of God any apprehenſions, not altogether unworthy of him, and criminally injurious to him, without believing, that to think, that he can be inconſtant, is as great a crime as it were a miſery to find him ſo. His love is like his eſſence, immutably eternal, reaching from everlaſting to everlaſting; it preceded the nativity of time, and will ſurvive John xxxi. 1. its utmoſt period and obſequies. *Having loved his own, which were in the world, he* Jam. i. 17. *loved them unto the end*, ſays the Evangeliſt: and when St. *James* had told us, that *every good gift and every perfect gift is from above and cometh down from the father of lights*, he adds (to complete our conſolation) *with whom is no variableneſs, neither ſhadow of turning. Of his own will begat he us of the word of truth.* And in effect, ſince God takes the motives of his love to us from himſelf, not from us, the unchangeableneſs of his nature ſeems ſtrongly to infer that of his charity, and our hap- Mal. iii. 6. pineſs in it. For, *I am the Lord, I change not; therefore ye ſons of Jacob are not conſumed*, ſays God by the laſt of his prophets: and in *Jeremiah* he tells his people, *I have loved thee with an everlaſting love.* And what God once ſaid to the generous Joſh. i. 5. *Joſhuah, I will never leave thee nor forſake thee*, is by the writer to the Hebrews ap- Heb. xiii. 5. plied to believers in general; for *the gifts and calling of God* (ſays the ſame author Rom. xi. 29. elſewhere) *are without repentance.* Nor do thoſe croſſes, that ſeem due to his anger, deſtroy the immutability of his love, ſince even that anger is an effect of it, proceeding from a fatherly impatience of ſeeing a ſpot unwiped off, in the face he loves too well to ſee a blemiſh in it; and from his deſire to ſee his child an object fit for a larger meaſure of his kindneſs: as when we beat the duſt out of a ſuit we fancy, we ſtrike not out of anger, but only to remove that, which doth ſully it, and hinder us

to take that delight in it, which our fondneſs would be pleaſed with a juſt cauſe to find. *As many as I love, I rebuke and chaſten* (ſays our Saviour.) And, *I know, O* Rev. iii. 19.
Lord (ſays the Pſalmiſt) *that thy judgments are right, and that thou in faithfulneſs haſt* Pſal. cxix. 75.
afflicted me; the furnace of affliction being meant but to refine us from our earthly droſſineſs, and ſoften us for the impreſſion of God's own ſtamp and image. The great and merciful architect of his church (whom not only the philoſophers have ſtiled, but the ſcripture itſelf calls τετνίχης, an artiſt or artificer) employs not on us the Heb. xi. 10.
hammer and the chizzel, with an intent to wound or mangle us, but only to ſquare and faſhion out hard and ſtubborn hearts into ſuch *living ſtones*, as may both grace 1 Pet. ii. 5.
and ſtrengthen his heavenly ſtructure. Nor is God only thus conſtant to his love, but to his lovelineſs. Our female beauties are uſually as fickle in their faces as their minds, and more certainly in the former; becauſe, though caſualties ſhould ſpare them, age brings in a neceſſity of a change, nay, a decay; leaving our doaters upon red and white inceſſantly perplexed, by the incertainty both of the continuance of their miſtreſs's kindneſs, and of the laſting of her beauty (both which are neceſſary to the amoriſt's joys and quiet): for, ſometimes, when the miſtreſs's humour doth not change ſo much, as to prove guilty of the fault of inconſtancy, her face alters enough to make her lovers wiſh inconſtancy no fault; or, that ſhe had committed it, that her fickleneſs might afford them the excuſe of imitation or revenge. But, in devotion, we are equally ſecure from both theſe dangers; ſince God doth not deſiſt from bleſſing us with his love, nor ceaſes ever from deſerving the height of ours. Nor is he only conſtant in making us the objects of his love, but alſo in bending and inclining us to make him the object of our ſtrongeſt affections; ſo that he not only perſiſts in continuing to us both the offer and value of his love, but perſeveres to give us a receptive diſpoſition to welcome it to us, and reflect it up to him. The want of ſuch a diſpoſition loſt *Adam* paradiſe, and the fallen angels heaven; there being to the object, that muſt ſecure our love, ſuch a nature requiſite in reference to our affections, as philoſophers are pleaſed to aſcribe to the world's center in relation to heavy bodies, which, they teach us, that magnetic point has the double faculty, not only to draw thither, but to keep there. For ſo untoward and croſs-grained are we, in point of our own good, and ſo unfit to procure, and ready to deſert, our own felicity, that neither its excellency is a ſufficient motive to carry our addreſſes to it, nor its poſſeſſion a competent tie to intercept in us all deſigns of revolts and divorces: but we muſt be uſed as peeviſh children are, who (on the one ſide) when their mouths are out of taſte, and they refuſe to take what is neceſſary for them, muſt have it not only offered them, but forced upon them, and be (as it were) made to receive it; and who (on the other ſide) muſt be reſtrained from gadding, when the beauty of the manſions they live in cannot invite their ſtay, but they would gladly leave the proudeſt palaces architecture can boaſt, to run into the ſtreet, and dabble in the kennel. All theſe three properties of divine conſtancy are not ill ſhadowed in the operations of the loadſtone (a mineral, in which I have made too many experiments, not to be by you allowed to make ſome compariſons to it.) For, firſt, it never forſakes its inclinations for the ſteel; next, being united to it, it retains ſo conſtantly its attractive qualities, that it gives not the needle any motive of deſerting it; and thirdly, it doth never rightly touch the amorous ſteel, without leaving an impreſſion, which ever after diſpoſes it to a converſion to that magnetic poſture, which beſt fits it to receive freſh influences. To which let me add this other reſemblance betwixt God's work on us, and the loadſtone's on the iron, that the kind ſtone attracts a needle to it, not to advantage itſelf by that union, but to impart its virtue to what

it draws. Beſides, abſence and rivals, thoſe frequent ruiners of other lovers happineſs, can threaten nothing of formidable to yours. For, abſence (which ſo divorces us from that, which animates us, that lovers do not ſo improperly ſtile it death, if death be but the ſeparation of ſoul and body) by God's ubiquity we are ſecured from; he is ever preſent with us, or rather in us. You, that, not long ſince, ſo highly valued the opportunities of converſing with your miſtreſs for ſome few moments, ſhall here find your privileges improved to a permiſſion; nay, an invitation, of entertaining the object of your love at all times; no hour renders your viſits unſeaſonable, nor no length tedious: he is rather welcomeſt to God, that comes to him oftneſt, and ſtays with him longeſt. What favours were vouchſafed to that antient Luke ii. 37, &c. propheteſs (who was likewiſe one of the firſt evangeliſts) who for many years *departed not from the temple, but ſerved God with faſtings and prayers night and day*, the beginning Act. vi. 25, 26, &c. of St. *Luke*'s goſpel may inform me. The midnight hymns of *Paul* and *Silas* did not only not diſturb or offend him they praiſed, but procured the viſit of an angel to bring them miraculous and unexpected liberty, as a proof of the acceptableneſs of their Gen. v. 22, 23, 24. ſeemingly unſeaſonable devotions. When *Enoch had walked with God* as many years as the year has days, God was ſo far from being importuned or tired by that laſting aſſiduity, that vouchſafing him an unexampled exemption from death, he was pleaſed by a new and a nearer cut to heaven, to admit him to a yet cloſer, more immediate, and more undiſtracted communion with himſelf. And when *Moſes* had ſpent no leſs Exod. xxxiv. 30. than *forty days and forty nights* in converſing (if I may uſe ſo familiar a term) with God in the mount, he brought down thence, inſtead of a penance for his importunity, ſo ſignal and radiant a teſtimony of God's peculiar favour, that his dazzled countrymen were as much diſabled, as invited, to gaze on an object of ſo much wonder. And then, how proud do we ſee many lovers of their ſufferings, when ſhe but knows of them, for whom they are endured? But in *Seraphic Love*, there is not the leaſt good wiſh, or privateſt ſuffering, nay, not a whiſpering ſigh, or cloſer thought, that ſilently groans or aſpires in the amorous ſoul, but he both ſees and hears, that puts Pſal. lvi. 8. his ſervants *tears into his bottle*, ſweetening and recompenſing the greateſt misfortunes that his love occaſions, with ſuch ſupport and joys, as hinder us to feel them, and Pſal. cxxxix. 2, 3. make them deſerve a contrary name. Each amorous ſoul may ſay to God with *David, Thou knoweſt my down-ſitting, and my up-riſing; thou underſtandeſt my thoughts afar off; thou compaſſeſt my path, and my lying down, and art acquainted with all my ways.* And Chriſt alſo himſelf has ſo attentive an eye upon the amorous ſoul, that he is Rev. ii. 8. 9. held forth in the Apocalypſe, as telling the ruler of the church of *Smyrna*, *I know thy works, and tribulation, and poverty*; and ſaying to the angel of the church of Verſe 12, 13. *Pergamus, I know thy works, and where thou dwelleſt, even where Satan's ſeat is; and thou holdeſt faſt my name, and haſt not denied my faith, even in thoſe days, wherein Antipas was my faithful martyr, who was ſlain among you, where Satan dwelleth.* So that no endearing circumſtance of our love eſcapes unobſerved by him, who has done and ſuffered ſo much to engage us to it. God remembers not our endeavours to ſerve him Mat. xxv. 37, &c. the leſs, for our having forgotten them: *When ſaw we thee any way diſtreſſed, and relieved thee?* will be the queſtion of thoſe, to whom heaven itſelf will be at the laſt day awarded, as having miniſtered to their Redeemer. Thoſe, that in degenerate times, ſuch as ours, *Lindamor*, did, like *Lot* in *Sodom*, mourn for their ſins, that mourned not for their own, and condoled among themſelves the ſpreading wickedneſs of the times they lived in; though probably the dangers threatened them by the very ſinfulneſs they deplored made them affect ſuch privacies in their conferences, as freed them from the thoughts of being over-heard; yet the ſcripture informs us (and

it is a comfortable as well as memorable paſſage) that *the Lord hearkened and heard it,* Mal. iii. 16, 17.
and a book of remembrance was written before him, for them, that feared the Lord, and
that thought upon his name: then ſhall he return, and diſcern betwixt the righteous and the
wicked, between him that ſerveth God, and him that ſerveth him not. I know, ſays Rev. ii. 9, 10.
Chriſt (not only to the angel of *Smyrna,* but to each true ſufferer for him) *thy works,*
and tribulation, and poverty: fear none of theſe things, that thou ſhalt ſuffer; be thou
faithful unto death, and I will give thee a crown of life. God is often pleaſed to accept
thoſe good thoughts and intentions of his ſervants, which never arrive at actual performances.
Though *David* built not the temple he deſigned, yet his ſon, that did
it, informs us, that God ſaid unto him; *Foraſmuch as it was in thine heart to build an* 2 Chron. vi. 8.
houſe for my name, thou didſt well in that it was in thine heart, &c. And it is the epithet
our Saviour gives God, *your Father which ſeeth in ſecret, &c.* Nor need we fear Mat. vi. 6.
our rivals ſhould ſupplant us, ſince we can have none in devotion, whoſe prayer and
endeavour it is not, that God would love us more. For his love to you being (as
the chiefeſt merit) the ſtrongeſt motive and title unto theirs, they cannot but wiſh
him well, whom God doth love ſo; and cannot wiſh him better, than by imploring
for him freſh additions, both of that love of God, and grateful diſpoſitions to return Luke xv. 7, 10.
it. Our Saviour aſſures us, that *there is joy in the preſence of the angels of God over one*
ſinner that repenteth. And the ſole hymn (except a viſionary one) I find recorded of Luke ii. 13, 14.
the celeſtial choir, was ſung for a bleſſing to mankind, wherein (for aught I know)
their love and ſympathy alone concerned them: *For unto us* (men) *the child is born,* Iſai. ix. 6.
and unto us the Son is given, who took not upon him the nature of angels, but the ſeed of
Abraham. So noble and ſo diſintereſted doth divine love make ours, that there is nothing, Heb. ii. 16.
beſides the object of that love, that we love more than our concurrents in it,
perchance out of a gratitude to their aſſiſting us, to pay a debt (of love and praiſe)
for which alas! we find our ſingle ſelves but too inſolvent.

PERHAPS I need not mind you, *Lindamor,* that divers paſſages of the foregoing Sect. 17.
diſcourſe ſuppoſe the truth of their doctrine, who aſcribe to God, in relation to every
man, an eternal, unchangeable and inconditionate decree of election, or reprobation.
Yet concerning the controverſies betwixt the Calviniſts and the Remonſtrants about
predeſtination, and the coherent doctrines, it were improper to give you here my
ſenſe. Thoſe, that are truly pious of either party, are perhaps otherwiſe looked on
by God than by any other, as contending, which of God's attributes ſhould be moſt
reſpected; the one ſeeming to affirm irreſpective decrees, to magnify his goodneſs;
and the other to deny them, but to ſecure the credit of his juſtice. And even in
honouring the ſame attribute, his goodneſs, theſe adverſaries ſeem rivals, the one
party ſuppoſing it beſt celebrated by believing it ſo irreſiſtible, that to whomſoever
it is intended, he cannot but be happy; and the other thinking it moſt extolled by
being believed ſo univerſal, that it will make every man happy, if he pleaſes: the
one party electing to honour free-grace, by aſſigning it (as to men) an unlimited vaſt
extent; as the other does, by aſcribing it an infallibly victorious degree. But though
my haſte, and the nature of my theme make me decline the controverſies about
predeſtination; yet ſince the doctrine, that maintains it, is not only by almoſt all the
reſt of mankind, but by the reſt of the proteſtant churches themſelves (the Lutherans,
and divers learned divines of the church of *England*) not only rejected, but
deteſted as little leſs than blaſphemous (as indeed they, that judge it an error,
cannot but be tempted to think it a dangerous one, and of very pernicious conſequence,
ſo far forth as its ſequels are permitted to have influence on men's practice)
I think it not amiſs to advertiſe you, that the doctrine of predeſtination is not neceſſary
to

to juſtify the freeneſs and the greatneſs of God's love. For ſo conſpicuous and re-
fulgent a truth is that of God's being the author of man's felicity, that the diſpute
betwixt the Calviniſts and Arminians is not ſo much concerning the thing, as con-
cerning the manner of its being proffered; the former affirming grace to be irre-
ſiſtibly preſented; the latter though they deny it to be irrejectable, yet granting, not
only that it is altogether free and undeſerved, but alſo, that the proffer is made, both
with a power enabling thoſe, to whom it is rendered, to accept it, and with ſuch en-
gaging invitations, that man at his firſt converſion need contribute nothing to his feli-
city, but the not wilfully refuſing it, and may more properly be ſaid to owe it unto
God, than the beggar to owe his alms to his reliever, though he opens his hand to
receive it; which he might have declined to do, if he would have wilfully courted
his own prejudice. Chriſt paid *a ranſom to redeem us*, it is true, and he is therefore
2 Pet. ii. 1. called, *the Lord that bought us*: but it was God's free goodneſs, both to provide us
that Chriſt, and to accept of that ranſom, neither of which he was obliged to do;
and therefore the ſcripture aſcribes it not to the juſtice, but to the love of God to the
John iii. 16. world, that *he ſent his only begotten Son to redeem it*: and St. *Paul* in the ſame text tells
Rom. iii. 24. us, both that *we are juſtified*, δωρεὰν τῇ αὐτοῦ χάριτι, *freely by his grace*, and yet that it is,
διὰ τῆς ἀπολυτρώσεως, *through the redemption* (purchaſed for a ranſom; the original word
Engliſhed redemption relating to the price paid for the redeeming of captives) *that is
in Jeſus Chriſt*. It is confeſſed on all hands, that merit muſt be diſclaimed, and thoſe
that ſeem to expect ſomething from God as a due, acknowledge, that if his promiſe
did not, their actions could not, make it ſo; and that it is to his mercy they owe the
2 Tim. iv. 7, 8. right they have to confide in his juſtice. St. *Paul*, who having *fought the good fight,
finiſhed his courſe, and kept the faith*, expected *a crown of righteouſneſs from the Lord*, under
Eph. ii. 8. the notion of the righteous judge; yet, tells us, *that by grace we are ſaved, through faith,
and that not of ourſelves, it is the gift of God*. Whoſe promiſes, now they are made us, al-
2 Pet. i. 4. low us indeed to expect heaven from his juſtice; but *the making us thoſe great and precious
promiſes* (as St. *Peter* juſtly ſtyles them) muſt be acknowledged the pure effect of *his free
and undeſerved goodneſs*; which to believe infinite, we need but conſider the diſproportion
betwixt ſuch a recompenſe as eternal glory, and the leaſt imperfect performances of ours:
which though they needed not pardon, could not at leaſt challenge any reward from
him, who, as our Creator, has ſuch a right to exact of us what ſervices he pleaſes,
without propoſing us any recompence, that our exacteſt obedience to all his commands
Luke xvii. 10. would yet leave us to confeſs ourſelves *unprofitable ſervants, who have done but what it
was our duty to do*; and what, if we had not done, we had given God, who had the
power, the right and provocation to puniſh us. And indeed, ſo conſcious are men
generally (if not naturally) to their being beholden to God for their felicity, that
even thoſe, that miſtake or oppoſe his way of doing them good, will yet be ſure to
find out ſome notion or other, under which they may conceive themſelves God's
debtors for his bleſſings. That the more ſober ſort of Romiſh catholics themſelves
aſcribe not ſo much to merit (properly ſo called) nor ſo little to God's grace, as the
more quarrelſome writers of their party have given the more eager diſputants of ours
occaſion to reproach them, were perhaps no difficult taſk to manifeſt, if my haſte
would give me leave. That the Arminians own the freeneſs and unmeritedneſs of
* *Eſpecially chap.* xvii. *Numb.* 5, 6. God's grace, the Remonſtrants * confeſſion and apology are very careful to ſatisfy the
world. And even the Socinians (how proſperouſly I determine not) are not a little,
or unınduſtriouſly ſollicitous to free their erroneous doctrine of juſtification from the
Hab. i. 26. objected guilt of its tendency to draw the embracers of it to *ſacrifice to their own nets*,
and thank themſelves for their felicity. Which brings into my mind a paſſage, that
I lately

I lately read in one of the chief modern upholders of that ſect, *Schlichtingius*; who is wont, in my opinion, where his ſubject will bear it, to diſcourſe as fairly and as rationally as almoſt any writer that I have met with of his perſuaſion; and who labours to reconcile *Socinus* his doctrine with the freeneſs of God's grace, by conſiderations, which, not to injure him, I ſhall preſent you with in his own words. *Ad retundendam vero* (ſays he, diſputing againſt the learned *Meiſnerus) arrogantiam juſtificatorum, & ne dicant ſe meruiſſe gratiam, non eſt neceſſe ſervum in homine arbitrium inducere; non debet virtus tolli, ut tollatur arrogantia. Sufficit,* 1. *Quod nec velle nec perficere poſſint, niſi Deus & voluntatem excitet, & vires augeat.* 2. *Quod ea, quæ divinis adjuti viribus faciunt, nullo modo dignitate & pretio divinæ gratiæ reſpondeat, ſed infinito intervallo ab ea abſint.* Nay, though the modern and degenerate Jews be, upon the ſcore of being the great patrons of man's free-will, not cauſeleſsly eſteemed the great oppugners of God's free-grace; yet both from their famous Rabbi, and my learned acquaintance, *Manaſſeh Ben-Iſrael*, and from divers others of their moſt eminent writers, has the truth ſometimes extorted confeſſions, which, though made upon erroneous grounds, were not very far ſhort of orthodox. To which purpoſe I remember, that a Jewiſh profeſſor of Hebrew (who aſſiſted me in my ſtudies of that myſterious tongue) being, as the reſt of his nation, an eager and peremptory champion of free-will, conceived, that even that liberty, which to us ſeems leaſt to indebt men to their Creator, did tranſcendently oblige him unto God. For, one day that we were privately and freely diſcourſing together of matters of religion, he told me, he thought men owed more to God's goodneſs, than the very angels do. For, ſaid he, whereas God, without any good work of theirs, but purely out of his goodneſs, conferred on them that bleſt condition they enjoy, by giving man a free-will, by the good uſe of which he may glorify his Maker, when, by abuſing it, it is in his power to diſhonour him; he allows man that higheſt ſatisfaction and privilege, of co-operating to his own felicity.

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Sect. 18.

And now, *Lindamor*, we are arrived at the laſt property, which qualifies God the fitteſt object for our love, which is, the advantageouſneſs of his to us, both in the preſent and the future life. And firſt, even in this world we owe God no leſs than all the goods we poſſeſs. We owe him both what we have, and that we are: for we may ſay truly of God with the Pſalmiſt, *It is he that hath made us, and not we ourſelves.* And we were not only in his hands ſo like *clay in the potter*'s, that he might have made us any thing; but we were ſo purely that negative, from whence we were extracted, that he (if he had pleaſed) for ever might have left us to our firſt nothing. His love is the firſt original and fountain-bleſſing; all the reſt are but as pipes (and inſtruments) to convey, and ſerve but to hand it to us. Your wit wins you applauſe, your induſtry heaps you up treaſures; be it granted. But who gave you that wit? and did both give and proſper that boaſted induſtry? Certainly, God as much gives us all the goods we poſſeſs, as he that gives a beggar a thouſand pounds, gives him the clothes, and meat, and all the bravery it helps him to. But beſides theſe more obvious preſents of God's bounty, we enjoy other effects of his goodneſs, which, though by the cuſtomarineſs of their being poſſeſſed, they prove leſs conſpicuous than the other, yet grow no leſs prized, when the want or loſs of them makes us ſenſible of the true value of them. Had I the leiſure, *Lindamor*, to lead your thoughts with me to the galleys, and ſhew you there thoſe wretched captives, that are chained to the oars they tug at, and though expoſed to all the miſeries and hardſhips of a tempeſtuous ſea, have oftentimes cauſe given them, by their barbarous uſage aſhore, to fear the ocean leſs than any port, ſave death. Could I draw from you the curtains of

Pſal. cx. 3.
Iſai. lxiv. 8.

ſick and dying men, and open to you that ſad ſcene, where ſome pine and languiſh by diſtempers, that deprive them of all joys, advantages, and (what is more conſiderable) uſes of life, before they eaſe them of life itſelf; others breathe rather than live, perpetually tormented either with their diſeaſes or phyſic to protract a wretched life, upon terms that turn it into a trouble; and others ſtruggling with the rude pangs of death, are yet, perchance, leſs tormented by them, than by the ſad proſpect of their former life, and the remembrance of thoſe criminal pleaſures, *which yet it perhaps leſs troubles them that they muſt now forego, than that they once enjoyed them.* Should I, *Lindamor*, bring you into hoſpitals, and ſhew you there the various ſhapes of human miſery, and how many ſouls, narrowly lodged (if I may ſo ſpeak) in ſynecdochical bodies, ſee their earthen cottages moulder away to duſt; thoſe miſerable perſons by the loſs of one limb after another ſurviving but part of themſelves, and living to ſee themſelves dead and buried by piece-meal. Should I, to diſpatch, *Lindamor*, ſhew you all the ſeveral companies of mourners, that almoſt make up mankind, and diſcloſe to you, how copious ſhowers of tears do almoſt every where water (not to ſay overflow) this vale of miſeries; you would, perchance, ſee cauſe to think, that God's privative (if I may ſo ſpeak) may contend with his poſitive favours; and that you owe little leſs for what you are not, than for what you are, to that diſcriminating mercy of his, to which alone you owe your exemption from miſeries, as great as 1 Cor. iv. 7. the bleſſing it confers on you. For, *who maketh thee to differ?* is a queſtion, that may be as well aſked in reference to our external, as to our ſpiritual condition. Which invites me to mind you, *Lindamor*, that you are yet more engaged to God's love for protecting you from thoſe groſs vices, that disfigure moſt men's minds, than from thoſe leſs dangerous, though more reſented diſeaſes, that diſtemper their bodies.

1 Pet. i. 18. FOR, *ambition*, *luſt*, *avarice*, *revenge*, and even that *vain converſation*, which young gentlemen are generally pleaſed to think ſo innocent, are really more formidable and pernicious diſeaſes and calamities, than thoſe, that reduce men to take phyſic, or thruſt them into hoſpitals. To evince the truth of which paradox, I hope I ſhall not need to mind you of judging of the dangerouſneſs of diſeaſes by the nobleneſs of that part affected, ſince I can tell you, that he, that cannot err, ſeems daily to juſtify our aſſertion, by inflicting ſickneſs and the ſharpeſt outward calamities on his own deareſt children, to preſerve them from the contagion of ſin, or cure them of the unfilial habitudes of it. And therefore, ſince, when we ſee a tender mother apply a painful cauſtic to the neck of her favourite-infant, threatened by the apoplexy, we ſcruple not to conclude, that ſhe thinks the trouble of an iſſue, an evil inferior to the convulſion fit; ſo when we ſee our heavenly father ſends infirmities and croſſes, to reſcue thoſe he loves, from the contagion or dominion of ſin; we may ſafely conclude, he thinks affliction a leſs evil than guilt, ſince he is too wiſe and indulgent a phyſician to cure with a remedy worſe than the diſeaſe. In the eighth of *Deuteronomy*, there is a caution given the Iſraelites, leſt proſperity (which is wont to be a kind of *Lethe*, that makes men forget all but their enjoyments) ſhould make any of them ſay Deut. viii. 17, 18. in his heart, *my power, and the might of my hand hath gotten me this wealth. But* (ſaith my text) *they ſhall remember the Lord their God*; *for he it is, that giveth them power to get wealth.* It is not the revolting Iſraelites only, of whoſe ignorance of his bounty Hoſ. xi. 3. God may complain, as he did by the prophet, by whom he ſaid, *I taught Ephraim to go, taking them by their arms, but they knew not that I healed them*: and there are Hoſ. i. 8, 9. but too many, of whom he might ſay, as he did by the ſame prophet: *For ſhe did not know, that I gave her corn, and wine, and oil, and multiplied her ſilver and her gold, which*

which they prepared for Baal; *therefore will I return and take away my corn in the time*
thereof, *and my wine in the ſeaſon thereof*, *and will recover my wool and my flax given to*
cover her nakedneſs. And this will make way for the deſign I had to recommend the
advantageouſneſs of God's love, by ſaying, that as for ſpiritual goods, he gives us
in this life ſo rich an earneſt of expected joys, that even the earneſt is a ſtock large
enough to ſubſiſt with comfort on, and really out-values and tranſcends all thoſe momentary
pleaſures, it requires us to forſake, to keep up a title to eternal ones. But
to particularize God's mercies to us in this very life, would certainly take up a conſiderable
part of it. And yet the love God bears us dies not with us, nor doth (as
men's affections) either endure a funeral in our tombs, or ſurvive only in a uſeleſs
grief (or an eſteem as bootleſs). No, God's love is ſo far from reſembling the uſual
ſort of friends, who, when they have accompanied us to the grave, do there leave
us; that, like the angels, that *carried Lazarus's ſoul to Abraham's boſom*, its officiouſneſs Luke xvi,
begins then moſt to appear, when our dark eyes are cloſed, and is then trueſt to 22.
the beloved ſoul, when ſhe forſakes the body; giving each bleſſed ſaint cauſe to ſay of
God, what *Naomi* did of *Boaz*, that *he hath not left off his kindneſs to the living*, *and* Ruth. ii. 20.
to the dead. Now, indeed (ſays our Saviour's favourite) *are we the ſons of God*, *and* 1 John iii.
it doth not yet appear what we ſhall be; *but we know*, *when he ſhall appear*, *we ſhall be* 1.
like him. This bleſſed expectance muſt be now my theme, becauſe the narrow limits,
which my deſign hath placed to this diſcourſe (of the advantages accruing from God's
love) will leave no more room untaken up by heaven.

BUT, *Lindamor*, before I proceed to ſet forth to you the greatneſs of the felicity Sect. 19.
reſerved for us in heaven, it will, I fear, be requiſite to mind you of the lawfulneſs
of having an eye to it. For many, not undeſervedly, applauded preachers have of
late been pleaſed to teach the people, that to hope for heaven is a mercenary legal,
and therefore unfilial affection. Indeed, to hope for heaven as wages for work performed,
or by way of merit, in the proper and ſtrict acception of that term, were a
preſumption, to which none of the divines, we diſſent from, can be too much an
enemy, nor perhaps more ſo than I am. But to take in God's bleſſings among the
motives of loving God, is, but to do as he did, who ſaid, *I love the Lord becauſe he* Pſa. cxvi. 1.
hath heard my voice and my ſupplications; and to look upon the joys of heaven, to
comfort and ſupport us in the hardſhips and loſſes to be undergone in our journey
thitherward, is to imitate no worſe a man than *Moſes*, of whom it is ſaid, that *he* Heb. xi. 26.
eſteemed the reproach of Chriſt greater riches than the treaſures in Egypt; *for he had reſpect*,
or turned his eye (ἀπέϐλεπε) *unto the recompence of the reward*. It is indeed, *Lindamor*,
a happy frame of mind, to be able to love God purely for himſelf, without
any glance at our own advantages. But though I dare not deny, that it is poſſible
to attain to ſo high and diſintereſted a kind of love, yet I think, that that excellency
ſuppoſed to be vouchſafed to ſome men, is not by the ſcripture exacted as a duty
from all men. Were all the recompenſe of piety of a worldly nature, and to be here
received, the actions invited to, by the intuition of it, might paſs for mercenary. But
when heaven is chiefly hoped for, as it will admit us unto the fruition of God himſelf
in Chriſt, and that the other joys expected there are ſo far from being of a ſenſual
or a worldly nature, that they are known not to be attainable, till by death, the ſenſes
and bodies themſelves, and all the merely animal faculties be aboliſhed: for a heaven
ſo conſidered, I ſay, to forego readily all the pleaſures of the ſenſes, and undergo
chearfully all the hardſhips and dangers, that are wont to attend a holy life, is *Lindamor*,
ſuch a kind of mercenarineſs, as none, but a reſigned, noble, and believing
ſoul is likely to be guilty of. If I ſhould ſay; that fear itſelf, and even the fear of
hell,

hell, may be one juſtifiable motive of men's actions, though I ſhould propoſe, what
thoſe I am reaſoning with would think a paradox; yet I ſhould perhaps hold forth
Heb. iv. 1. therein no more than the ſcripture does, *Let us therefore fear*, (ſays the writer to the
Hebrews) *leſt a promiſe being left us of entering into his reſt, any of you ſhall ſeem to come*
ſhort of it. And no leſs eminent an herald of the goſpel than St. *Paul*, who ſucceſsfully
maintained the evangelical againſt the legal ſpirit, thus profeſſeth of himſelf;
1 Cor. ix. 27. *I keep under my body, and bring it into ſubjection, leſt by any means, when I have preached*
to others, I myſelf ſhould be a caſt-away. And it was not to ſlaves or hirelings, that
Chriſt directs this admonition; *I ſay unto you*, my friends, *be not afraid of them, that*
kill the body, and after that have no more that they can do: but I will forewarn you whom
you ſhall fear; fear him, which, after he hath killed, hath power to caſt into hell; yea, I
ſay unto you (a gemination, which the preſent controverſy ſhews not to have been cauſeleſs)
Luk. xii. 4, 5. *fear him.* Where the paraphraſe given of God is not barely deſcriptive, but
ratiocinative (to borrow thoſe terms of the ſchools) in warning us not only whom we
ſhould, and ſhould not fear, but why we ſhould fear the one, and not the other. As
when St. *Paul* ſays, *I know whom I have truſted*, he means what manner of perſon,
1 Pet. iv. 19. how *faithful* (as St. *Peter* elſewhere calls God) *and how omnipotent:* whence immediately
2 Tim. i. 12. he adds, *And I am perſuaded, that he is able to keep that which I have committed*
unto him againſt that day. More texts of the ſame import might be added, if the
deſign of thoſe already alleged were other than to facilitate the admiſſion of the more
plauſible truth we have been making out, and which to us ſeems very clearly held
forth in thoſe and the like ſcriptures, which are therefore cited out of the new teſtament,
that they might have the greater authority with one ſort of our antagoniſts. *I*
Phil. iii 14. *preſs toward the mark of the prize of the high calling of God in Chriſt Jeſus. Bleſſed are*
Rev. xxii. 14. *they, that do his commandments, that they may have right to the tree of life, and may enter*
in through the gates into the city; laying up in ſtore for themſelves a good foundation againſt
1 Tim. vi. 19. *the time to come, that they may lay hold of eternal life. To them, who by patient continuing*
in well-doing, ſeek for (ζητοῦσι) *glory, and honour, and immortality, eternal life.* And of
Chriſt himſelf, whoſe love to God is queſtionleſs filial and unequalable, it is ſaid,
Rom. ii. 17. *Looking unto Jeſus the author and finiſher of our faith, who, for the joy that was ſet before*
Heb. xii. 2. *him, endured the croſs, deſpiſed the ſhame, and is ſet down at the right hand of the*
throne of God. Nor ſee I, why it ſhould be unfilial for a child of God to further the
raiſing of thoſe paſſions, which his heavenly Father intends to have raiſed in him,
upon the ſame grounds and motives, that God is pleaſed to employ to excite them.
And ſince the ſcripture ſeems plainly to invite our hopes, by recording St. *Paul*'s having
1 Cor. ix. 25. ſaid, *Every man, that ſtriveth for the maſtery, is temperate in all things; now they do*
it to obtain a corruptible crown, but we an incorruptible: And by repreſenting our Saviour
Mat. v. 12. himſelf, as ſaying in one place, *Rejoice, and be exceeding glad, for great is your*
Rev. ii. 10. *reward in heaven:* In another, *Be thou faithful unto death, and I will give thee a crown*
of life. And in divers others ſpeaking to the like purpoſe; ſince, I ſay, the ſcripture
ſeems thus to allure our hopes, would it not be a kind of accuſing it of an aptneſs
to delude and enſnare us, to teach, that it propoſeth to us the powerfulleſt objects to
incite our paſſions, if it be ſinful to cheriſh and harbour the paſſions naturally belonging
to thoſe objects? And certainly, *Lindamor*, ſince God, who, as our Creator,
knows the frame and conſtitution of man's ſoul, incomparably better than he himſelf,
is pleaſed to deal with our hopes and our fears, to engage us to his ſervice; it very
ill becomes us, either to quarrel with his methods of working on our ſpirits, or to
reject any help, which he has been pleaſed to afford a piety, which, for aught ever I
could obſerve, does even in the beſt men find reſiſtance enough to keep any help, that

can be employed to promote it, from being ſuperſtitious. And truly, the animating or diſcouraging influence, that hope, or the want of it, is wont to have upon our endeavours, makes me very apprehenſive, that ſince the enlivening hopes of heaven are not able to make moſt men's endeavours other than very languid, the forbidding thoſe ſupporting hopes would ſoon weaken and decreaſe our endeavours into none at all.

Sect. 20. BUT, *Lindamor*, though I may perhaps have taken ſome pains in ſtudying controverſial divinity, yet I take ſo little pleaſure in writing of it, that though not only a ſeaſonable duty to truth, but a neceſſary one to the enſuing part of this diſcourſe, have preſſed me to ſerve in this cauſe; yet I ſhall perhaps obtain your pardon ſooner than my own, for having thus long ſuſpended the diſcourſing to you of the advantageouſneſs of God's love to us, as it gives us here a right, and will hereafter give us admiſſion to heaven; heaven, the bright ſeat of ſo much happineſs, that we ſhall ſcarcely count amongſt our joys, that heaven is the ſeat of them. There the excellency of the poſſeſſed goods ſhall as much diſappoint our expectations, as in other fruitions the emptineſs is wont to do. The Apoſtle tells us, that *eye hath not ſeen, nor ear heard, neither have entered into the heart of man, the things which God hath prepared for them that love him.* (1 Cor. ii. 9.) Such pure refined delights not only ſtoop not unto ſenſe, but are ſublime enough even to tranſcend imagination. When fancy hath formed and ſhaped the perfecteſt ideas, that its abſtractions can make, of bleſſedneſs; our own more happy experiences of greater muſt diſabuſe us, when we come to heaven; which is a ſoil, whoſe fruitfulneſs is ſo confined to joy, that even our diſappointments and miſtakes ſhall there contribute to our happineſs; which will ſo much partake of his immenſity, whoſe gift it is, that you ſee the Apoſtle gives it a negative deſcription; and to create in us apprehenſions underogatory from what we ſhall poſſeſs, not only removes our thoughts from all we do enjoy, but exalts them above all that we can fancy. At which way of proceeding, that you may the leſs wonder, *Lindamor*, be pleaſed to conſider, that in heaven our faculties ſhall not only be gratified with ſuitable and acceptable objects, but ſhall be heightened and enlarged, and conſequently our capacities of happineſs as well increaſed as filled. A child not yet releaſed out of the homely priſon of the womb cannot there poſſibly frame ideas of thoſe delights, which will be afforded him by the pleaſing noiſes, and the glittering objects, that will preſent themſelves to him after his birth. And the ſame child, whilſt he continues in his non-age, though he may with delight look upon emblems finally drawn and painted, and may take ſome pleaſure in beholding the neat and ſurprizing characters and flouriſhes of a Greek and Hebrew bible curiouſly printed; yet he cannot then imagine the pleaſure the ſame objects will afford him, when age and ſtudy ſhall have ripened and inſtructed his intellectuals, and made him capable both of underſtanding and reliſhing the excellent moralities, couched in thoſe ingenious emblems, and the profound and ſaving myſteries, wherewith that divine book, the ſcripture (eſpecially in its original tongues) does, to an intelligent and religious peruſer, appear repleniſhed. Such a double advantage, *Lindamor*, among others, the admiſſion into heaven brings thoſe, to whom that bleſſing is vouchſafed: for beſides that ſet of objects (if I may ſo ſpeak) ſo new and ſo peculiar to heaven, that their ideas could never enter into men's thoughts before their admiſſion there; beſides this, I ſay, our then enlarged capacities will enable us, even in objects which were not altogether unknown to us before, to perceive things formerly undiſcerned, and derive thence both new and greater ſatisfactions and delights. Wonder not, *Lindamor*, that in mentioning the joys of heaven, I uſe the expreſſions I find leſs detractory from a theme, as much above our praiſes, as the heaven they are enjoyed in is above our heads. For, though ſuch expreſſions may ſeem ſomewhat tumid and aſpiring, and fitter much for one that celebrates, than for one that but aſſerts; yet cannot I ſcruple to uſe ſeeming hyperboles in the mention of felicities, which make the higheſt hyperboles but ſeeming ones. For thoſe joys, *Lindamor*, of heaven, are like its ſtars, which, by reaſon of our remoteneſs from them, appear extremely little, though really in themſelves they are ſo vaſt, that a leſs than the largeſt is by odds greater than the biggeſt object upon earth, nay, than the whole earth itſelf. And therefore as if I were to take you with me to contemplate the planets, I would ſhew you them through ſuch a teleſcope, as by a greatning thoſe bright objects in compariſon of what to an unaſſiſted eye they appear, doth ſomewhat leſſen the diſadvantage of remoteneſs, and ſhews them with leſs detraction from their true magnitude; ſo mentioning to you the felicities of heaven, I think it not unlawful or improper to endeavour, by repreſentations tranſcending what they appear, to give you notions leſs inferiour to what they are.

In heaven, then, we ſhall taſte happineſs enough, to enable us to rectify the definition of it. We may there be inſtructed, how to name and rate all goods, by thoſe that will concentre into the felicity we ſhall poſſeſs, which ſhall be there made up of the confluence, perfection, and perpetuity of all true joys. For heaven will make us happy, not (as philoſophy pretends to do) by the confinement, but by the fruition of our deſires; which ſhall neither fail in the choice of their objects, nor miſs of the enjoyment of them, but be both unerringly juſt, and infallibly accompliſhed; in the former of which properties (as our then rectified reaſon will conſider things) we ſhall think ourſelves happier (yet) than in the latter. We ſhall there reſemble the ſaints we here admire, and ſhall not only ſee, and be like thoſe pious worthies, whoſe virtues eclipſe theirs, which among the heathen deified leſs deſerving heroes; thoſe excellent perſons, that did as well ennoble as inſtruct mankind, giving us cauſe to bluſh, that we are men, and whoſe ſtories have the unparallelled honour
Heb. xii. 23. of being recorded by inſpired pens; thoſe ſpirits, I ſay, *of juſt men made perfect*, as
the ſcripture terms them, ſhall be our conſtant and familiar company, into whoſe bleſſed ſociety we ſhall not only be welcome, but increaſe it. In heaven, we ſhall have a bleſſed and familiar converſation with thoſe ſame glorious ſpirits, whoſe nature doth inveſt them with ſuch a luſtre, that all the diſadvantage of their diſguiſes, when they appear to us, doth ſcarce ſuffice to confine our raptures to reſpects below idolatry, and darken them into objects for our wonder, not adoration. There we ſhall ſee (a ſight worthy dying for) that bleſſed Saviour, of whom the ſcripture does ſo much and ſo excellently entertain us, and who having done and ſuffered ſo much for us, does ſo highly deſerve of us, both upon the account of his ineſtimable benefits.
Ἐσκήνωσεν ἐν ἡμῖν. Yes, there ſhall we ſee that holy and divine perſon, who, when he vouchſafed (as his
John i. 11. favourite-diſciple ſpeaks) *to pitch his tent among us*, and dwell with men on earth, to
fit them by his merits and example to dwell with him in heaven, did ſo admirably mix an awful majeſty with an humble meekneſs, and the aſſumed infirmities of his human nature with the ſeaſonable coruſcations of his divine; and expreſſed in his whole life ſo perfect and exemplary a virtue, and yet ſo much ſweetneſs and gentleneſs towards thoſe aſpirers to it, that were the moſt ſhort of it; that the Jews them-
Mat. vii. 37. ſelves could ſay of him, that *he had done all things well*; and his very enemies, that
were employed to apprehend him as a malefactor, confeſſed even to thoſe, that ſent
John vii. 46. them to do ſo, that *never man ſpake like him*. And his Apoſtles, who had moſt op-
portunity to pry narrowly into his actions, and were of a condition and breeding very unlike to infuſe into them heroic reſolutions, did in ſpight of the frequent reproofs

their failings extorted from him, and of the hardſhips that attended his ſervice, think
even death itſelf in his company, more eligible, than life led out of it; *let us alſo go*; John xi. 14
that we may die with him (ſays even the diſtruſtful *Thomas* to his fellow-diſciples.)
But, *Lindamor*, we ſhall there ſee the Son of God, not in that *form of a ſervant*, Phil. ii. 7.
which he put on that he might ſuffer for us, and exerciſe his prieſtly and prophetic
function here below; but in that regal ſtate and condition, which belongs to him by
virtue of his kingly office; on whoſe ſcore he is ſtyled in the ſcripture, *King of Kings*, Rev. xix.
and Lord of Lords, all power or authority being (as himſelf ſpeaks) given him both 16.
in heaven and in earth. And how nobly attended may we ſuppoſe this divine mo- Mat. xxviii.
narch to be in his exalted condition in heaven, when in his ſtate of humiliation on 18.
earth, whilſt he was in the *wilderneſs among the wild beaſts*, *the angels* are recorded to Mat. i. 13.
have *miniſtred unto him*; and whilſt he lay ſwathed in a homely manger, the multitude Luke i.
of the celeſtial hoſt were heard to ſolemnize his miraculous birth, according to that
paſſage of the writer to the Hebrews, *when he bringeth in the firſt-begotten into the* Heb. i. 6.
world (he ſaith) *and let all the angels of God worſhip him!* And yet ſuch conſiderable
and noble creatures thoſe immaterial intelligences, called angels, are, that one of
them in one night was able to deſtroy above a hundred and fourſcore thouſand men
in the blaſphemous *Senacherib*'s impious camp. And ſo much majeſty and ſuperio- 2 King xix.
rity does their nature give them in reference even to the eminenteſt of mortals, that 35.
when the undaunted *Joſhua* had boldly challenged one of them, that appeared to
him in the likeneſs of a man, and demanded, whom he was for; when he knew him
to be an angel (unleſs he ſuppoſed him to be that promiſed Meſſiah, who is elſewhere
called *the angel of the covenant*, as it is in the original, and in the ſame text *the Lord)* Mal. iii. 1.
he alters his addreſs unto him into this ſubmiſſive one, *What ſaith my Lord unto his* Joſh. v. 13,
ſervant? And even wiſe and holy *Daniel* himſelf, who was the ſecond perſon of the 14.
world for power, and the firſt for much nobler attributes, wiſdom, and underſtanding, who ſaw ſo many governors of vaſt provinces, and profeſſors of admired magic,
inferiour to himſelf, yet confeſſes to the angel, that appeared to him, that he was
confounded and diſanimated at his preſence, and adds, *How can the ſervant of this my*
Lord talk with this my Lord? We may ſafely therefore expect, that we ſhall then Dan. x. 17.
behold our Saviour, not in thoſe diſguiſes, which disfigured him in their eyes, that
only conſidered his ſufferings; but in that triumphant condition, wherewith they are
now rewarded. The thorns of his (now no more galling, but adorning) crown will
appear upon that radiant head of his more glorious, than thoſe of the flaming buſh,
wherein God appeared to *Moſes*. And we ſhall not ſee that deſpicable form, which Exod. iii. 2.
made the prophet ſay (perſonating the generality of the Jewiſh nation contemporary
with the Meſſias)—*he hath no form, nor comelineſs, and when we ſhall ſee him, there is* Iſa. liii. 2.
no beauty that we ſhould deſire. But we ſhall ſee him encompaſſed with ſo much majeſty, and ſhining with ſo much of his genuine ſplendor and beauty, that we ſhall
deſervedly eſteem him *Nephle, the Admirable.* And our raviſhed ſouls ſhall, by the Iſa. ix. 6.
more attentive contemplation of him, but find more cauſe to imitate the ſpouſe in
Solomon's myſtic epithalamium; who having dwelt upon the beauty of the ſeveral
parts, that concurred to the accompliſhing the divine bridegroom, exclaims in an
epiphonema, very contrary to the expreſſion lately mentioned in the prophet, he *is*
altogether deſires, or by an Hebraiſm, *moſt deſirable*, which our Engliſh renders *he is* Cant. v. 10.
altogether lovely. His eyes will there appear as St. *John* repreſents them, of active Rev. i. 14.
fire, and will into the willing breaſts of the raviſhed beholders ſhoot flames as pure,
as holy, and as deathleſs, as thoſe fathers of the church, who believed not the angels
altogether incorporeal, may be ſuppoſed to have conceived the ſeraphims to conſiſt
of.

of. Certainly, ſince (as the ſcripture informs us) *it never entered into the heart of man, what God hath reſerved in heaven for thoſe that love him*; that glory can be but imperfectly expreſſed by the bare epithet of inconceivable, with which God rewards the meritorious ſufferings and obedience of that only begotten ſon of his love, for whoſe Dan. xii. 3. ſake he is pleaſed to confer on ſo many thouſands of men unimaginable glories. He, that vouchſafes even to many of his ſervants a brightneſs like that of the ſtars, will Mal. iv. 2. ſure communicate a more radiant luſtre to the *ſun of righteouſneſs*, that only ſon of Heb. i. 2, 3. his, *whom* (to borrow inſpired expreſſions) *he hath made heir of all things, by whom alſo he made the worlds: who being the brightneſs of his glory, and the expreſs image of* Eph. i. 21. *his perſon, aud upholding all things by the word of his power, when he had by himſelf purged our ſins, ſate down on the right hand of the majeſty on high, far above all principality, and power, and might, and dominion, and every name that is named, not only in this world, but in that which is to come.* Thereby exalting him not only above all earthly princes and potentates, but even above the higheſt orders of the ſpirits of the celeſtial hierarchy.

Sect. 21. BUT apprehend not, *Lindamor*, that this ſublime exaltation of Chriſt will make him deſpiſe the meaneſt of his ſaints, or diſdain communion with him. For, St. Phil. ii. 7. *Paul* aſſures us, that he was in the *form of God*, when he vouchſafed to take upon him the form of a ſervant, to make us free: and ſure, he that condeſcended ſo far, and ſtooped ſo low, to invite and to bring us to heaven, will not refuſe us a gracious reception there. In the days of his fleſh he was pleaſed to own *Lazarus*, even in the diſhonours of the grave, and vouchſafed him in that deſpicable condition the glorious title of his friend; and when he deſcended the Mount of Olives, all the loud acclamations of the glad multitude, that ſang *Hoſannahs* to him, and ſtrewed his way with their palms, their veſtments, and their praiſes, could not divert him from deploring, Luke xix. 37, 38, 41. even with tears, *Jeruſalem*'s approaching fate; and expreſſing in the midſt of his triumphs, a concern for the very worſt and ſtubborneſt of his enemies. And, leſt it ſhould be thought, that he was thus concerned for worthleſs mortals, only whilſt he ſhared and felt the miſeries of their condition; give me leave to obſerve to you, *Lindamor*, that immediately after his reſurrection, whilſt the ſenſe of the ſudden and unexampled change of his condition was freſh upon him, and the remembrance of the Apoſtles ingratitude in deſerting him, almoſt as recent, he ſticks not to give thoſe very diſciples, that forſook him, the glorious title (and that a more familiar one Matt. xxviii. 10. than we find him to have allowed them on earth) of his brethren; and particularly Mark xvi. 7. ſhews himſelf ſolicitous for that *Peter*, who, in ſpight of all thoſe empty boaſts, wherewith he not only ſeemed to defy death, but Chriſt's prediction too, did not only forſake his Maſter, but denied him. He can in heaven be ſo concerned for his diſ-Acts vii. treſſed members here on earth, that not only he can take notice of expiring *Stephen*, through that cloud of fatal ſtones that battered down his priſon; and can, when Acts ix. 4. *Saul* was perſecuting the vulgar Chriſtians, cry out unto him, *Saul, Saul, why perſecuteſt thou me?* as if he, and thoſe that love him, were but one; but in the meſſages *In the ſecond and third chapters of the* Apocalypſe. he ſends to the Angels or governors of the ſeven churches, he ſufficiently manifeſts, that ſingle Chriſtians may be as particularly in his thoughts, as if they had no other object: his greatneſs will not make his kindneſs leſs familiar, but only more obliging; Rev. iii. 20. he diſdains not, even after his aſcenſion, to ſay, *Behold, I ſtand at the door and knock, if any man hear my voice, and open the door, I will come in to him, and will ſup with him, and he with me.* That king, in the parable, by whom himſelf is repreſented, is Matt. xxv. pleaſed himſelf to welcome each individual truſty ſervant with a peculiar *Euge, bone ſerve.* And in another parable he ſcruples not to repreſent the great condeſcenſions

and familiarity, wherewith the Lord ſhall remunerate the faithful and watchful ſervant,
by telling his diſciples, *Bleſſed are thoſe ſervants, whom the Lord, when he cometh,* Luke xii.
ſhall find ſo watching. Verily I ſay unto you, that he ſhall gird himſelf, and will make 37.
them to ſit down to meat, and will come forth to ſerve them. At which expreſſion you
will the leſs admire, if you conſider, that beſides that it is parabolical, and probably
hyperbolical, and therefore not to be taken (as the French ſpeak, *au pied de la lettre)*
in a ſtrict ſenſe; we find in the evangelical ſtory, that our Saviour diſdained not to
converſe familiarly with publicans and ſinners, nor even to waſh the feet of his own
diſciples. And ſince he condeſcended to ſuch familiarity to thoſe frail mortals, ſo
many of whom were apt to turn the favours of God into wantonneſs, we may the
leſs admire at his gracious condeſcenſions to thoſe, the ſinleſſneſs of whoſe condition
will keep them from turning his vouchſafements into any thing, but occaſions of joy
and gratitude. Marvel not, *Lindamor*, as at an impertinency, that I appear ſo ſolicitous
to poſſeſs you with high apprehenſions of the ſupereminent dignity and felicity
of our exalted Saviour; and that in the loftieſt and moſt magnificent expreſſions, the
ſcripture affords me, I have endeavoured to make out to you, that the bright ſun of
righteouſneſs is now grown uncapable of ſuffering eclipſes, but ſhines with unclouded
and unequalled ſplendor; and that we ſhall in heaven (as the writer to the Hebrews
ſpeaks) *ſee Jeſus, who was made a little* (or for a ſhort while, βραχυ τι) *lower than the* Heb. ii. 9.
angels, for the ſuffering of death, crowned with glory, and honour. For it will make
heaven more heavenly to us, to find him reigning there, who hath ſuffered ſo much
for us; and for whom, if we ever come thither, we ſhall have ſo much ſuffered. For,
ſince our love will, as the reſt of our ſurviving graces, be perfected and grow compleat
in heaven; the ineffable happineſs of our dear Redeemer muſt needs bring an
increaſe to ours, commenſurate to the ardency of our love for him, and we ſhall reliſh
no happineſs more than that, which is therefore ours, becauſe he is happy, and
ſuppoſes in him, what it confers on us. Beſides that, ſince the regal dignity, wherein
(as Mediator) he is inſtated, may make us owe our ſalvation to his ſentence, as well
as to his merits; heaven itſelf will be ineſtimably endeared to us, by our injoying it
upon his account. I need not tell you, what a value lovers are wont to ſet upon the
leaſt favours, that can be beſtowed on them by their miſtreſſes; and that oftentimes
a worthleſs ribband, a bracelet of hair, or ſome ſuch trifle, that nothing can make
precious but her repreſenting it, is highlier valued by the tranſported lover than the
richeſt preſents of nature, or of fortune. How great a bleſſing muſt it then be, *Lindamor*,
to owe joys, that need not endearing circumſtances to merit the name of happineſs,
to a perſon ſo beloved, that even heaven itſelf will bring more felicity to the
amorous ſoul, as it is a proof of his love, than that it is a donative of his bounty:
as paſſionate brides do incomparably more value their wedding-rings which their glad
lovers give them, as pledges, than as gold. It is ſaid of the perſecuted and diſgraced
Apoſtles, that *they rejoiced to have been counted worthy to ſuffer ſhame for his name.* But Acts v. 41.
if they deſervedly rejoiced to be allowed to ſuffer for him, of how much joy ſhall
they have juſt cauſe, that are admitted to reign with him? His having ſupported
the hardſhips and the toils (for I muſt not now call them evils) to which our afflicted
condition of mortality is expoſed, does ſo much alleviate them, and refreſh us under
them, that in this ſenſe alſo it may be truly ſaid, that *the chaſtiſement of our peace was* Iſa. liii. 5.
upon him, and by his hurts we are healed. But he that relieved us even by his croſs,
will ſure do more for us by his crown, when he ſhall admit, and even invite, each
faithful ſervant to no leſs a bleſſing than *to enter into his maſter's joy.* So rich a ſource Matt. xxv.
of happineſs did Chriſt deſign to make himſelf to us in all his capacities and conditions, 21.
that

that in heaven and in earth, it was, and will be, his gracious and conſtant imployment to ſhare our griefs, or impart to us his joys; and either leſſen our miſeries by his ſufferings, or increaſe our happineſs by his felicity.

Expect not, *Lindamor*, that I ſhould ſolemnly beg your pardon for this ſeeming excurſion, till I come to think it a digreſſion, to inſiſt on the bleſſedneſs of Chriſt in heaven, treating of the happineſs, which thoſe that love him will enjoy there. Wherefore, not to ſpend time in imploring a needleſs forgiveneſs, I ſhall proceed to tell you, *Lindamor*, that in heaven we ſhall not only ſee our elder brother Chriſt, but probably alſo our kindred, friends, and relations, that living here in his fear, died Luke xx. 36. in his favour. For, ſince our Saviour tells us, that *the children of the reſurrection ſhall be* ἰσάγγελοι, *equal to*, or like *the Angels*; who yet, in the viſions of *Daniel* and St. *John*, appear to be acquainted with each other; ſince, in the parable of the miſerable Luke xvi. Epicure, and the happy beggar, the father of the faithful is repreſented as knowing, not only the perſon, and preſent condition, but the paſt ſtory of *Lazarus:* ſince the 1 Theſſ. ii. 19, 20. Dan. ii. inſtructer of the Gentiles confidently expects his converted and pious Theſſalonians, to be *his crown at the great day*, *when the having turned many to righteouſneſs*, ſhall, as the ſcripture foretels, confer a ſtar-like and immortal brightneſs: ſince (which is chiefly conſiderable) the knowledge of particular actions, and, conſequently, perſons, ſeems requiſite to the attainment of that great end of God, in the day of judgment, the manifeſtation of his punitive and remunerative juſtice: ſince, I ſay, theſe argumets, beſides divers others, are afforded us by the ſcripture, we may ſafely conclude it probable, that we ſhall know each other in a place, where, ſince nothing requiſite to happineſs can be wanting, we may well ſuppoſe (at leaſt, if we can imagine here, what we ſhall think there) that we ſhall not want ſo great a ſatisfaction, as that of being knowingly happy, in our other-ſelves, our friends. Nor is this only probable, *Lindamor*, but it is not improbable, that thoſe friends, that know us in heaven, ſhall welcome us thither. For, ſince Chriſt aſſures us, that the very angels (though they be ſo far from being related to our perſons, that they are foreigners to our very Luke xv. 7. nature) receive acceſſion of joy for a relenting ſinner, that by repentance begins to turn towards God; you will think it abſurd, that in a place where charity ſhall not 1 Cor. xiii. 8. only continue (as St. *Paul* ſpeaks) but grow perfect, our dear friends ſhould rejoice to ſee us, not only begin to turn towards God, but come home to him. Nor is it unlikely, that our tranſported ſouls ſhall mutually congratulate each other, their having now fully eſcaped the numerous rocks, and ſhelves, and quickſands, and threatening ſtorms, and no leſs dangerous calms, through which they are at length arrived at that peaceful haven, where innocence and delight (which are here ſo ſeldom matched) are inſeparable companions, both of each other, and each bleſſed reſident. With thoſe friends we here lamented, we ſhall there rejoice. And it will be but need, that the diſcovery of each other's virtues ſhould bring us to a mutual knowledge of our perſons; for otherwiſe, we ſhall be ſo changed, that we ſhould never know our friends; and ſhould ſcarce know ourſelves, were not an eminent increaſe of knowledge a part of that happy change. For thoſe departed friends, whom at our laſt ſeparation we ſaw disfigured by all the ghaſtly horrors of death, we ſhall then Phil. iii. 21. ſee aſſiſting about the majeſtick throne of Chriſt, with their once *vile bodies transfigured into the likeneſs of his glorious body*; mingling their glad acclamations with the hallelujahs of thrones, principalities, and powers, and the moſt dignified favourites of the celeſtial court. I need not tell you, *Lindamor*, that we ſhall be more juſtly tranſported at this meeting, than was good old *Jacob* at that of his ſon *Joſeph*, whom having long mourned for dead and loſt, he found not only alive, but a great favourite,

rite, ready to welcome him to an unknown court. For, whereas the patriarch ſaid to his ſon, *Now let me die, ſince I have ſeen thy face*; the ſeeing of our friends in heaven will aſſure us, that we ſhall for ever live with them there. Gen. xlvi. 30. The re-union of friends being there as privileged from divorce, as that of ſoul and body, (which ſcarce will be more ſtrict and ſatisfactory.) For, here indeed, if our friends do not allay our love or affection by unwelcome actions, or their contagious ſufferings, we commonly doat on them to a degree, that, as it were, reduces God to deprive us early of them, and ſnatch our idols, and his rivals, from us. But there, our, on both ſides, compleated graces will not only deſerve, but allow a higher ſtrain of friendſhip. The near contemplation and fruition of the infinitely tranſcendent perfections of the Creator, keeping all our kindneſs to the creatures not only ſubordinate to the love we owe to God, but grounded on it; as excited needles, when they ſtick faſteſt to each other, owe their union to their having both been touched by the loadſtone; to which they have therefore both of them ſtronger inclinations, than either to the other.

THERE, probably, we ſhall ſatisfactorily underſtand thoſe deep and obſcure myſ- Sect. 24.
teries of religion, which the profoundeſt clerks, that love not to flatter themſelves, acknowledge they are unable fully to comprehend; being, after all the toil and induſtry of their anxious enquiries reduced to ſit down with the Apoſtle's ῶ Βάθος, in Rom. xi. 33. admiration of that depth, whoſe bottom they cannot fathom. There we ſhall underſtand thoſe obſcure paſſages of (that divine book, and uncapable of flattery) the ſcripture, which, notwithſtanding all that bold criticks, and learned expoſitors, have attempted to illuſtrate it, does ſtill continue, in many places, obſcure. There, diſcerning how exquiſitely the ſeveral parts of ſcripture are fitted to the ſeveral times, perſons and occurrences, wherein their all-foreſeeing author intended moſt to uſe them; we ſhall diſcern not only a reconcileableneſs, but a friendſhip, and perfect harmony, betwixt thoſe texts, that here ſeem moſt at variance; and ſhall diſcover not only the ſenſe of the obſcurer paſſages, but the requiſiteneſs of their having been written ſo obſcurely. That ſtrange and peculiar, as well as otherwiſe cryptical method and ſtyle of ſcripture, which often coſts us ſo much ſtudy to find it rational, we ſhall there diſcover to be admirable, and worthy of its omniſcient author. There, I hope, we ſhall have clearly expounded to us thoſe riddles of providence, which have, but too often tempted, even good men, to queſtion God's conduct in the government of the world; whilſt the calamities and perſecutions of virtue, and innocence, ſeem approved by him, who accumulates proſperities on their criminal oppoſers. There we ſhall be convinced, that all theſe ſeeming irregularities, which the heathen thought fit to impute to the giddy whimſies of a blind female deity, are not only conſiſtent with God's juſtice and goodneſs, but are productions of it. And though ſuch a belief do here, to intelligent perſons, ſeem perhaps a greater piece of ſelf-denial, than to refrain from wine, or gold, or miſtreſſes; yet in heaven it will appear as reaſonable, as here we find it difficult. For, as *Bildad* ſpeaks in *Job*, *We are but of yeſterday, and know nothing*; *becauſe our days upon earth are but a ſhadow.* And the ſhortneſs of our tranſitory lives not permitting us to continue long enough ſpectators here, to ſee above a ſcene or two at moſt of that great play, acted by mankind upon the ſtage of the world; it is no wonder we are apt to harbour ſiniſter thoughts of the contriver of a plot, whoſe neither beginning nor end we are acquainted with: which yet is little leſs injurious, than it were to cenſure the lofty tragedian *Seneca*, or ſome other matchleſs artiſt, having peruſed but a piece of ſome tragedy, whereof the latter part never arrived at our view. But, when once God's whole plot (if I may ſo ſpeak) and conduct in the adminiſtration of the world ſhall come to be diſcloſed; all

thoſe revolutions and occurrences of empires, ſtates, families, and particular perſons, which men are here ſo prone to quarrel with, will there appear ſo juſt, ſo requiſite, and ſo ſeaſonable, that thoſe very things, which here tempted us to deny God, ſhall there engage us to praiſe him; and we ſhall not ſo properly be ſatisfied with his providence, as raviſhed. But eſpecially, we ſhall be tranſported with wonder and gratitude, when God ſhall vouchſafe to diſcover to his particular ſervants, the reaſons of his diſpenſations towards them, and make out to them not only the neceſſity and juſtice, but even mercifulneſs of thoſe very afflictions, that were moſt imputed to his ſeverity, (no ſtroke from God's paternal hand, either lighting ſooner, or falling heavier, or ſtaying longer, than the occaſion, that extorted it, exacted) and convince them, that their hopes were never diſappointed, but to ſecure their title to better things than thoſe they hoped for; nor their inferior intereſts prejudiced, but for the advantage of their ſupreme ones. Yes, all that unwelcome darkneſs, that here ſurrounded our purblind underſtandings, will vaniſh at the dawning of that bright, and (as St. *Peter*'s * expreſſion may be interpreted) eternal day; wherein the reſolution of all thoſe difficulties, which here exerciſed (and perhaps diſtreſſed) our faith, ſhall be granted us to reward it. And I muſt profeſs to you, *Lindamor*, (as unfaſhionable as ſuch a profeſſion may ſeem to a gentleman not yet two and twenty) that I find the ſtudy of thoſe excellent themes, God's word, and his providence, ſo difficult, and yet ſo pleaſing and inviting, that could heaven afford me no greater bleſſing, than a clear account of the abſtruſe myſteries of divinity and providence, I ſhould value the having my underſtanding gratified and enriched with truths of ſo noble and precious a nature, enough to court heaven at the rate of renouncing for it all thoſe unmanly ſenſualities and trifling vanities, for which inconſiderate mortals are wont to forfeit the intereſt their Saviour ſo dearly bought them in it.

2 Pet. iii. 18.
* ἡμέρα αἰῶνος.

Sect. 25.

Nor ſhall we only converſe with ſaints and angels, but with that infinitely more glorious Deity, that made them what they are, without at all impoveriſhing himſelf. In heaven, we ſhall enjoy (its maker) God, and *ſee him as he is*, who (as the ſcripture telleth us) ſhall then be *all in all*; compriſing all the goods, we value in the creatures, as eminently and fully, as the ſun doth the light, that twinkleth in the ſtars.

1 John iii. 2.
1 Cor. xv. 28.

If one, that was none of the leaſt of the philoſophers, ſcrupled not to tell the man, that aſketh him, what he was born for, that it was to contemplate the ſun; if our beſt naturaliſts themſelves, though the darkneſs, that is here caſt upon things, and the dimneſs of our intellectual eyes, (which I remember *Ariſtotle* fitly compares to thoſe of an owl at noon-day) permit us to diſcern but very little of that wiſdom, power, and goodneſs, which he has expreſſed in the creation; are yet often tranſported and raviſhed with a juſt admiration of the perfections he has diſplayed in his workmanſhip: if the wiſe queen, that came ſo far to viſit *Solomon*, was put almoſt into an extaſy, by the ſight of his (though wiſe, yet human) ordering of things: and if the *angels themſelves* (as St. *Peter* informs us) *deſire to look into the myſterious contrivances of God*, in order to man's redemption: how great a ſatisfaction, *Lindamor*, may we juſtly conceive it muſt needs be, to be admitted to ſee ſo much of God, as heaven will diſcover to us? eſpecially, ſince ſo much of our future happineſs will conſiſt in that beatific viſion, (as ſchoolmen juſtly call it) that St. *John* concluded, that *we ſhall be like God*, becauſe *we ſhall ſee him as he is.* And our Saviour himſelf paraphraſes our celeſtial felicity by this bleſſed viſion, where he ſays, *Bleſſed are the pure in heart for they ſhall ſee God:* As on the other ſide, the writer to the Hebrews employs the being denied the ſight of that divine object, as a deſcription of extreme wretchedneſs, in that text, where, having exhorted thoſe, to whom he writes, to

Anaxagoras.
1 Pet. i. 12.
1 John iii. 2.
Mat. v. 8.

follow peace and holineſs, he adds, as the formidableſt menace which he could make Heb. xii. 14. uſe of, to deter them from ſlighting his exhortation, *without which, no man ſhall ſee the Lord*. And by this viſion our Saviour ſeems to deſcribe the happineſs even of angels; where, forbidding the ſcandalizing of any of thoſe little ones that believe in him, he adds, to enforce what he had ſaid, that *their angels do always ſee the face of his* Mat. xviii. 10. *Father in heaven*. We ſhall be ſo taken up with the contemplation and fruition of that glorious object, (in whoſe infinity all goods are included and dilated) that ages, numberleſs as the joys that beatific viſion abounds with, will ſcarce afford us leiſure for a diverſion to any other pleaſures, than thoſe itſelf creates. Which are ſo numerous and ſo entire, that we ſhall there deſire nothing that we have not, except more tongues to ſing more praiſes to him, or at leaſt a capacity to pay him greater thanks for what we have. And even thoſe deſires, God's gracious acceptation will make, in being conceived, accompliſhed: for, otherwiſe heaven's reſidents ſcarce know any other want, than that of need to wiſh; the compleat bleſſedneſs of their condition reducing them to a happy uſeleſſneſs of wiſhes, by affording them a full prepoſſeſſion of all the objects of deſire. There time, like fire, having deſtroyed whatever it could prey on, ſhall, at laſt, die itſelf, and ſhall go out into eternity. Whoſe nature is ſuch, that though our joys, after ſome centuries of years, may ſeem to have grown elder, by having been enjoyed ſo many ages, yet will they really ſtill continue new, not only upon the ſcores of their welcomeneſs and freſhneſs, but by their perpetually equal (becauſe infinite) diſtance from a period. There our felicity ſhall always be the ſame, yet ever new. Wearineſs arguing imperfection, either in the object or the appetite; the former of which is impoſſible in God, and the latter ſhall ceaſe in heaven. Where our felicity ſhall be ſo great, that variety itſelf ſhall not be needed as a part of it. And, if heaven do admit variety, it may be ſuppoſed ſuch a one, as ſhall conſiſt in a further knowledge of the firſt object (God) not a forſaking of it; and ſuch as ariſes from the fixed beholding of the changing necks of doves, or ſuch as we may ſee in the diverſified refractions of the ſame ſparkling diamond. In God there is (if I may ſo ſpeak) ſuch a various identity, that the fruition of him both ſatisfies and creates deſires: though that, without ſatiety; and this, without diſquiet. Other delights, like the uſual clothes we wear, quickly grow ſtale, and are ſoon worn out; whereas celeſtial pleaſures participate the prerogative of the Jews garments in the wilderneſs, of not impairing by being uſed long. But as the amorous needle, once joined unto the loadſtone, would never, Deut. xxix. 5. uncompelled, forſake the inchanting mineral; but, after ages, cling no leſs cloſely to it than the firſt moment of their union: ſo do the ſaints in heaven, with the ſame undiminiſhed freſhneſs, ever poſſeſs their joys, as if each moment were the firſt that they poſſeſſed them in. And, if their happineſs does not improve by laſtinger enjoyment, it is becauſe it was firſt uncapable of increaſe. Or, if our pleaſures do admit acceſſion, they ſhall receive it from our aſſurance, that we ſhall taſte them for ever, and perpetually repeat (or rather continue) the ſame renewed fruitions to an eternity, endeared by nothing more than by the quiet leiſure it will afford us, undiſtractedly to employ it, in celebrating of the author's praiſes; and in a condition, happier in that by it we grow paſt doing, than by paſt ſuffering ill. In heaven (in a word) our inexhauſted joys will be ſo numberleſs, and ſo immenſe, that we ſhall need (as well as have) eternity itſelf to taſte them fully.

But, *Lindamor*, it was not my deſign to give you a particular topography of this Sect. 26. celeſtial *Canaan*; but only in a few words to let you ſee, that *it is a land flowing with milk and honey*. And though I acknowledge, I have given you but dark deſcriptions (and ſometimes rather poetical than chorographical) of what the apoſtle ſtyles *the*

Col. i. 12. *inheritance of the ſaints in light:* yet ſince, to whet our longings for fruitive (or experimental) knowledge, it is reſerved among the prerogatives of being in heaven, to know how happy we ſhall be, when there; and ſince, what the ſcripture hath revealed of paradiſe, ſeems meant rather to quicken our obedience, than ſatisfy our curioſity; I may, for thoſe purpoſes, have perhaps tolerably performed that taſk (of heavenly topography) by the acknowledgments of my diſability to do it worthily. I ſhall now only add this property of our expected bliſs, that the vaſt multitude of partners does detract nothing from each private ſhare, nor does the publickneſs of it leſſen propriety in it. This ocean of felicity being ſo ſhoreleſs, and ſo bottomleſs, that all the ſaints and angels cannot exhauſt it; it being as impoſſible for an aggregate of finites to comprehend or exhauſt one infinite, as it is for the greateſt number of mathematick points to amount to, or conſtitute a body. Our neighbour-regions do all enjoy the benefit of light as well as we; yet we enjoy not leſs, than if they enjoyed none. Indeed there is this difference between the ſun of righteouſneſs and that of heaven; that, whereas the latter, by his preſence, eclipſes all the planets, (his attendants;) the former, though radiant with a much brighter ſplendor, will, by his Col. iii. 4. preſence, impart to his ſaints, according to that of the Apoſtle; *When Chriſt, who is our life, ſhall appear, then ſhall we alſo appear with him in glory.* So that the elect, in relation to this ſun, ſhall not be like ſtars, which his ſhining obſcures and makes to diſappear; but, like poliſhed ſilver, or well-glazed arms, or thoſe vaſter balls of burniſhed braſs, the tops of churches are ſometimes adorned with, which ſhine not till they be ſhined upon, and derive their glittering brightneſs, and all the dazling fire that environs and illuſtrates them, from their being expoſed (unſkreened) to the Cant. vi. 3. ſun's refulgent beams. *I am my beloved's, and my beloved is mine,* ſays every ſaint (with the ſpouſe in the *Canticles*) to his Redeemer. *David* ſays of them that put Pſal. xxxvi. 6. their truſt in God, *That he ſhall abundantly ſatisfy them with the fatneſs of his houſe, and make them drink of the river of his pleaſures:* as if he meant to inſinuate, that, as when a multitude of perſons drink of the ſame river, none of them is able to exhauſt it, and yet each of them may have the full liberty of drinking as much as he can, or as much as he could, though none but himſelf ſhould be allowed to drink of it; ſo, whoſoever enjoys God, enjoys him wholly, or at leaſt doth enjoy him ſo entirely, in relation to that man's capacity, that the fruition of whatſoever reſts unenjoyed of God is forbidden by the immenſity of the object, and not the prepoſſeſſion of his rivals. The angels, though of a nature differing from ours, and thereby placed above the perſonal experience of our ſufferings and infirmities, do yet ſo ſympathiſe with us, that (as our Saviour informs us) they rejoice at the repentance of a ſinner. Luke xv. 7. And, though the members of the Church militant, and thoſe of the triumphant, live as far aſunder as heaven is from earth, and are not more diſtant as to place, than Eph. iii. 15. differing as to condition; yet St. *Paul* reckons all the ſaints to be but *one family in heaven and in earth.* If then the diſparity of reſidences, of qualities, and of conditions, cannot now hinder the lovers of God from being ſo concerned in one another; how much of endearing kindneſs may we ſuppoſe, that they will interchange, when both their love ſhall be perfected, and all thoſe other graces too that are proper to cheriſh and increaſe it? For the ſame Apoſtle, who, to aſſiſt us to conceive the ſtrictneſs of the union both betwixt Chriſt and his ſaints, and the ſaints among them- 1 Cor. xii. 27. ſelves, tells us, that *he is the head, and they are his body, and members in particular*; Ver. 26. teaches us to make this inference, that (to expreſs his doctrine in his own words) *If one member ſuffer, all the members ſuffer with it; and if one member be honoured, all the members rejoice with it.* Yes, *Lindamor*, in that bleſſed condition, our wills being perfectly conformed unto our Maker's, no ſaint nor angel can enjoy his love without poſſeſſing a proportionate degree of ours. And then, ſince perfect friendſhip appropriates to each friend the croſſes and proſperities of the other, (as good *Barzillai* could not be highlier obliged by *David*, than by the king's kindneſs to his ſon) each ſeveral beatitude in heaven ſhall (in ſome ſort concern the whole ſociety, and) be ours: as aſtronomers teach us, that the earth receives addition of light by the ſun's beams beſtowed upon the ſtars, and from the moon reflecting upon her. And becauſe our perſonal capacities are too narrow to contain all that joy, we are (by the ſtrange arithmetick of friendſhip) in a manner multiplied into as many happy perſons, as there are ſaints and angels bleſſed in heaven. Our perfect union to our common head, and mutual communion with each other, applying and bringing home every felicity of theirs to us: this friendly reciprocal ſympathy teaching us each glorified ſaint's bleſſedneſs, and him ours, by a bleſſed circulation, which makes us increaſe, by our reſenting them, thoſe joys (of others) whoſe increaſe we reſent. 2 Sam. xix. 37.

But my thoughts are engaged in ſo good a company, *Lindamor*, that they keep me from conſidering how faſt the hours paſs, and have almoſt made me forget, that the time, which my occaſions allowed me for ſcribbling to you, is ſo far ſpent, that not now at laſt to reprieve you from the perſecution of my blunt pen, were to be almoſt as injurious to my own affairs, as to your patience. Hereafter yet I may poſſibly make you ſome amends for this, with riper diſcourſes of the nature and duties, or (if you will) the properties and returns of this love, to which I have hitherto preſented you ſome motives. To (the laſt of) which I might add, that, our love to the creatures is an earneſt, but to God it is a title; the one makes us the objects, but the other makes the object ours: that, ſince there is in love ſo ſtrange a magick, as to transform the lover into the object loved; we ought to be extremely careful of the dedication of a paſſion, which, as it is placed, muſt either dignify our nature, or degrade it: and not to addreſs any lower (or, which is all one, to any other) object, the higheſt intenſity of a love, which cannot ſtoop without our degradation. And theſe I might expatiate on, and recruit them with many other motives, additional to thoſe I have already inſiſted on; but that I may more properly reſerve them to the treatiſe of the properties of that love, whoſe nature ſo partakes that of its object, that there can hardly be produced more powerful motives to it, than the conditions of it. Since then (as I freſhly intimated) I cannot but fear, that your tired patience, as well as my urgent occaſions (though theſe will recall me to-morrow morning to my own weſtern hermitage) doth at preſent ſummon me to leave you: and ſince I cannot do ſo in a happier place than heaven, I ſhall ſuſpend my farewels, only to beg you to believe, that ſo noble a motive of exalting friendſhip, as the ambition of rendering mine a fit return for yours, hath ſo improved my kindneſs, that my affection, without wronging its greatneſs, could not expreſs itſelf by any leſs attempt than this of gaining you the greateſt, and the moſt deſirable of all goods, by elevating (that noble harbinger of your ſoul) your love to heaven: whoſe joys alone are not inferior to thoſe, which the being made inſtrumental to procure them you would really create in,

My deareſt *Lindamor*,
Your moſt faithful,
Moſt affectionate, and
Moſt humble Servant,

From *Leeſe*, this 6th of
Aug. 1648.

ROBERT BOYLE.

ADVERTISEMENT.

WHILST the fourth edition of the foregoing treatiſe was upon finiſhing, it happened, that a devout and very ingenious friend of the author's having preſſed to have the peruſal of ſome occaſional meditations, that lay by him, on ſeveral ſubjects, met, among others, with that which follows: and finding the ſubject to be of affinity with what is rather touched than inſiſted on, in the 65th page of the paſt treatiſe, * touching men's inability to praiſe God worthily, and having a great deal of partiality for the reflection, would needs have it annexed, as it was, to the newly finiſhed edition: notwithſtanding the author's having too juſt cauſe to urge, that, beſides the imperfections, that are made neceſſary by the ſublimity of the theme, ſuch occaſional papers, eſpecially his, are wont to have peculiar unaccurateneſſes, as being deſigned rather to entertain the writer himſelf than any other reader.

[* page 169. of the volume.]

AN
OCCASIONAL REFLECTION
UPON A
LETTER

Received in *April* 1662;

Containing an account of what paſſed on the King's Coronation-day, in a little country town.

I NEED not, *Pyrocles*, after what we have been reading, tell you, that the writer of this letter thinks, that both in what he has ſaid of the king, and in what he has done to ſolemniſe his coronation, he has behaved himſelf rarely well. For I doubt not, but you eaſily diſcern by his way of writing, that he is highly ſatisfied with his performances, and expects that he ſhall, if not be thanked by the king, at leaſt be mentioned in the news-book. But it will, I fear, be requiſite to tell you, that this honeſt man is not alone of his mind; for being his landlord's bailiff, he is eſteemed at that rate by his neighbours, and looked upon as a man very conſiderable in his pariſh; and is perhaps thought to have a right to pity moſt of thoſe, that do not admire what he has now been doing. And yet you and I, who pretend not to be courtiers, can, in his rural encomiums, and in his ill-contrived way of honouring his prince, eaſily diſcover ſo much that might have been mended, and ſo much that might be laughed at, that if the king, according to his wonted graciouſneſs, vouchſafe this action his ſmiles, it muſt not be in conſideration of the ſuitableneſs of the performances to the occaſion, but, partly as they proceed from a hearty, though ill-expreſſed, loyalty and love, and partly, as they afford him a ſubject of merriment.

And not only the nice critics, who have ſeen thoſe magnificent ſolemnities, and heard the eloquent panegyrics, wherewith the principal cities and aſſemblies in the nation have thought they did but part of what they ſhould; and not only thoſe aſſiduous courtiers, who, by the honour of a nearer acceſs, have opportunities (denied to others) of diſcovering thoſe particularities, that may beſt give a high veneration for a great perſon and a great prince, to thoſe, that are qualified to diſcern and reliſh ſuch things: not only theſe, I ſay, will have a quite other opinion of the rural praiſes, and antique ceremonies, that were ſo well liked a hundred miles from *London*; but this country-man himſelf, if he were admitted to the court, and bred a while there, would, in time, ſee ſo great a diſtance betwixt what he has done, and what a perſon better bred might have done, that he could not remember without bluſhes, what he now looks upon with triumph.

And now I muſt on this occaſion confeſs to you, *Pyrocles*, that I have (on other riſes) ſeveral times been revolving in my thoughts, what the angels think of thoſe praiſes and deſcriptions of God, that men deviſe (for I intend not here to ſpeak of thoſe the ſcripture ſuggeſts) and wherein we are moſt applauded by others, and do oftentimes, perchance, applaud ourſelves. For thoſe celeſtial courtiers (if I may ſo call them) have ſeveral advantages to aſſiſt them in the celebration of our common maſter, which we poor mortals want. For firſt, they are free from thoſe ſelfiſh and inordinate affections, that too often hinder us, either from diſcerning the excellency of divers of God's attributes and ways, or from duly acknowledging it. They have no ſins to keep them from deſcrying the juſtneſs of what he does; they have no ingratitude to oppoſe the fuller reſentments of his goodneſs; and they are not tempted, not to diſcern and adore his wiſdom, for fear they ſhould appear culpable for repining at his diſpenſations. And indeed, their longevity allowing them the full proſpect, from end to end, of thoſe intricate tranſactions of providence, of which ſhort-lived mortals do commonly ſee but a part; they are queſtionleſs far more ſatiſfied with the incomparably better contrivances they diſcern in the management of human affairs, than we are with the conduct or plots of the moſt ſkilfully written plays and romances. Beſides, thoſe happy ſpirits, of whom the ſcripture tells us, that *they ſtand before God*, and that they *continually ſee his face*, have by that privilege the bleſſed opportunities of diſcovering in the Deity they contemplate and ſerve, many excellences, which even they could never, but by experience have formed any thoughts of; and they ſee in one another's ſolemn adorations and praiſes a way of honouring the object of them, ſo much tranſcending the utmoſt of what we here aim at, that their homages to their Creator may well be ſuppoſed of a far nobler kind than ours. And laſtly, when I conſider, how much leſs unworthy thoughts and expreſſions touching things divine the ſame perſon may have, when come to his full maturity of age and parts, and whilſt he was but a child in both; and when I conſider, how much more advantageous conceptions of the wiſdom diſplayed in the univerſe, and particularly in the contrivance of a human body, one, that is a true philoſopher, and a ſkilful anatomiſt, may have, in compariſon of a man illiterate, and unacquainted with diſſections: when, I ſay, I conſider theſe things, and compare the dim twilight of human intellects in this life with that clear and radiant light, which the ſcripture aſcribes to angels; I cannot but think, that, having to the privilege of a much nearer acceſs than is allowed us to contemplate God's perfections, the advantage of having incomparably more illuminated intellects to apprehend them with, they muſt frame otherguiſe conceptions of the divine attributes, and glorify the poſſeſſor at an otherguiſe rate, than is allowed to thoſe, whoſe underſtandings are ſo dim, and whoſe reſidence is ſo remote from that bleſſed place, where the perfections they would extol are moſt diſplayed.

Luke i. Matth. xviii.

Assisted by theſe and the like advantages, *Pyrocles*, thoſe happy ſpirits may well frame notions, and employ expreſſions in honour of their Maker, ſo far tranſcending ours, that, though the angel's goodneſs keeps them, doubtleſs, from beholding them with contempt; yet, we may well think, they look upon them with ſuch a kind of pity, as that, wherewith great wits and courtiers look upon the miſtakes and imperfections of what they did and writ, when they were but ſchool-boys; and as that, wherewith, when we ſhall be admitted to the ſociety of the angels, we ſhall look back upon our former ſelves. No, *Pyrocles*, to praiſe God is a debt, which, though we ſhould ever be paying, we muſt always owe; not only becauſe the renewed obligations will laſt as long as we, but becauſe, though the intire ſum were poſſibly to be paid, we have no coin of the value, that would be requiſite to make a payment of that nature. It is true indeed, that ſome men ſay much more than others upon a Heb. x. ſubject, on which none can ſay enough, and which even *the ſpirits of juſt men made perfect* can but imperfectly celebrate. It may be too, that the praiſes we pay to God procure us ſome from men, and perhaps even from orators and encomiaſts; and though I hope no man can ſo far flatter himſelf, as to think he can flatter what he can never do right to; yet the zealouſneſs of our endeavours, and the applauſe that others entertain them with, may perhaps tempt us to think, that, becauſe in our expreſſions we have ſurpaſſed ourſelves, we have almoſt equalled our theme: as if to make our praiſes too great for any other ſubject, were ſufficient to make them great enough for God. But alas, how widely muſt we be miſtaken! ſince our expreſſions, if we ſpeak ſenſe, can at beſt but fully repreſent our conceptions, and thoſe being but the notions of a finite creature, muſt needs fall extremely ſhort of perfections, which were not what they are, if they were not infinite. No, when we have employed the loftieſt hyperboles, and exhauſted all the celebrating topics and figures of rhetoric; when we have dreſſed metaphyſical abſtractions in poetic raptures; when we have ranſacked whatever things are moſt excellent among the creatures, and having defæcated them, and piled them up together, have made that heap but a riſe to take our ſoaring flight from; when we have ſummed up, and left beneath our expreſſions, all that we are here wont to acknowledge above them; nay, when inſtructed, as well as inflamed, 1 Tim. vi. 16. and tranſported by that *inacceſſible light*, that is inhabited by what we adore, we ſeem raiſed and elevated above all that is mortal, and above ourſelves, and ſay things, that nothing elſe could either inſpire or merit: even then, I ſay, thoſe expreſſions, which any otherwiſe applied would be hyperboles, do but expreſs our devotion, not the divine object of it, and declare how much we honour him, rather than what he is. And indeed, none but the poſſeſſor of an infinite intellect can be able to ſay, what the poſſeſſor of other infinite perfections deſerves to have ſaid of him. And whatever zealous ſkill we praiſe God with, we do far leſs honour him than injure him, if we think our aſpiringeſt praiſes can arrive ſo far, as, I ſay not to reach, but ſo much as to approach their ſubject.

But, let not this inevitable impotence, *Pyrocles*, trouble or diſcourage us. Thoſe Rev. xiv. 4. bleſſed ſouls, that *follow the lamb whitherſoever he goes*, do (as we are taught in the Apocalypſe) make it their buſineſs, and find it their happineſs, to ſpend a great part of their eternity in extolling him, by whom they are placed in a condition, where they can have no employment, but what is holy and noble. And even here below, the praiſing of God is a work, wherein we imitate, though we do not equal the angels, and are buſied in the ſame employment, though not with the ſame ſkill. Nay, heaven itſelf exempts not its reſidents from an impotence, which belongs to creatures, not as they are imperfect ones, but as they are creatures. Even the members of the church triumphant do not triumph over this neceſſary impotence; their praiſes may need pardon, even in a place where they can ſin no more; and they can expect but from God's goodneſs the acceptance of thoſe praiſes, that are improved, as well as occaſioned, even by their being made partakers of his glory. Nay, in the prophet *Iſaiah*'s extatic viſion, the Seraphim themſelves, that are introduced, as anſwering one another's glad acclamations to God, are likewiſe repreſented, as covering (out of reſpect their faces with their wings. But, *Pyrocles*, as I was ſaying, this unavoidable diſability to ſay things worthy of God need not at all trouble us; ſince we pay our homages to one, whoſe goodneſs our expreſſions can as little equal, as they can his other attributes. He that created us, will not impute it to us, that we act but as creatures; and ſince he has declared, that *where there is a willing mind, a man is accepted according to what he has, and not according to what he has not*; the impotence I have been ſpeaking of, ought to bring us rather joy than trouble, ſince the infinite diſtance betwixt us, without leſſening his favourable acceptance of our praiſes, ſuppoſes the boundleſs perfections of him, whom thoſe praiſes (through his goodneſs) help to give us an intereſt in; and no ſon would repine at his royal father's greatneſs, how immenſe ſoever, being ſure, that greatneſs would not leſſen his kindneſs. For it is leſs deſirable to be able to deſcribe the power and excellencies of him we have an intereſt in, than to have an intereſt in one, whoſe power and goodneſs exceeds whatever we can ſay or fancy of them.

Iſa. vi. 2.
2 Cor. viii. 12.

To conclude, *Pyrocles*, ſince on the one ſide God is moſt truly ſaid in the ſcripture to be ſo *glorious, that he is exalted above all bleſſing and praiſe*, and conſequently, though *I could* (to uſe St. *Paul*'s phraſe) *ſpeak with the tongues of men and angels*; yet the higheſt things I could ſay of the divine perfections, muſt needs be therefore far below them, becauſe a creature were able to ſay them. And, ſince on the other ſide, it is of us men, that God vouchſafes to ſay, *whoſo offereth praiſe, glorifieth me*; and his tranſcendent excellency is ſo far from being inconſiſtent with a reſembling graciouſneſs, that ſuch a benignity is one of the moſt conſpicuous parts of it: I will not forbear to pay my praiſes unto one, whoſe deſerving infinitely more than I can offer keeps him not from accepting as much leſs than he deſerves. But then I muſt not preſume to *fill my mouth with his praiſes*, without ſenſibly acknowledging, that there is not any ſubject, whereon my expreſſions can more want eloquence, than on this ſubject; even eloquence itſelf would want expreſſions.

Nehem. ix. 5.
1 Cor. xiii. 1.
Pſal. xxiii.

CERTAIN
PHYSIOLOGICAL ESSAYS,
AND OTHER
TRACTS;

Written at diſtant Times, and on ſeveral Occaſions.

[In the ſecond Edition, ſome of the Tracts were enlarged by Experiments, and the work increaſed by the addition of a Diſcourſe about *ABSOLUTE REST* in BODIES.]

An Advertiſement to the Reader: Prefixed to the firſt Edition, put forth, A. D. 1661.

THE publiſher deſires, that the reader ſhould be informed, that neither were the following treatiſes written near about the ſame time (ſome of them being divers years older than ſome others) nor yet are they now publiſhed in the ſame order that they were written in. For the firſt of theſe diſcourſes (though penned about four years ſince) was not only written after the ſecond, third, and fourth, but after divers other eſſays, which the author has yet lying by him among his papers; it being intended for a kind of introduction to all thoſe treatiſes, which under ſeverel names, but chiefly that of *Phyſiological Eſſays*, the author had then compoſed. But having, during the late confuſions, ſo diſpoſed of his papers to ſecure them, that he could not himſelf ſeaſonably recover them; and being engaged by promiſe to ſome friends, to let about half a dozen of his ſmall tracts come abroad into the world by ſuch a time, he was fain to ſend the following treatiſes to the preſs as they came, ſome at one time, ſome at another, to his hands. And this his occaſions did now and then reduce him to do in ſuch haſte, that he could not attend the correcting either the printer's lapſes or his own, and particularly was was obliged, partly by haſte, and partly by a diſtemper in his eyes, to ſend away the *Hiſtory of Firmneſs* without ſo much as reading it over. All which it is hoped the equitable reader will conſider in his favour, and bear with what may be imputable to ſuch circumſtances.

I SHOULD add nothing further, were it not that to ſave the reader the trouble of gueſſing, who is meant by that *Pyrophilus*, to whom moſt of the following treatiſes are addreſſed, I think it requiſite to inform him, that the perſon veiled under that name, is that hopeful young gentleman, Mr. *Richard Jones*, only ſon to the Lord Viſcount *Ranelagh* and an excellent Lady, ſiſter to the author.

A

PROËMIAL ESSAY,

WHEREIN, WITH SOME

Conſiderations touching EXPERIMENTAL ESSAYS in general,

Is interwoven ſuch an Introduction to all thoſe written by the Author, as is neceſſary to be peruſed for the better underſtanding of them.

I KNOW not, *Pyrophilus*, whether what you will meet in the enſuing diſcourſes will prove worthy of your taking notice of; yet I dare tell you, that if all my endeavours to ſerve you were not duties, I ſhould think I might deſerve your thanks for venturing to write them for your ſake. For I am ſufficiently ſenſible, both how unlearned I am, and in how learned an age I preſume to write: nor has the great number of thoſe eſcaped my obſervation, who finding it a much eaſier taſk to cenſure experimental compoſures than to write ſuch, endeavour to acquire the title of judicious, by condeming all things, that themſelves have not written, or thought on. And indeed, *Pyrophilus*, I had, beſides theſe, ſo many other diſcouraging conſiderations in my eye, whilſt I was ſetting down the following eſſays, that I ſhould ſcarce have proſecuted a deſign ſo full of trouble, and ſo unlikely by its ſucceſs to make amends for it, if I had thought it free, for the ſecuring of my own quiet and credit, to ſuppreſs obſervations which might prove ſerviceable to you, who having ſufficiently converſed with books, are now deſirous to begin to converſe with things themſelves. But I muſt confeſs, I look upon experimental truths as matters of ſo great concernment to mankind, that in ſpite of the juſt ſenſe I have of my own diſabilities, I am deterred from complying with thoſe inclinations and motives that endear ſilence to me, by conſidering the fate of him, who though he had leſs entruſted to him than any of his companions, was yet ſeverely puniſhed for burying his ſingle talent. And though, *Pyrophilus*, I could not without ſuch reluctancy reſolve to write, yet I found it much more uneaſy to reſolve to write ſo ſoon. For I could not but conſider, that being yet but very young, not only in years, but, what is much worſe, in experience, I have yet much more need to learn, than ability to teach. And I conſidered too, that after a man is grown ſomewhat acquainted with things themſelves, and has taken ſome general notice of the cognations, differences, and tendencies of their properties, he may every day ſo much improve his knowledge, that I am apt to think, that if God ſhould be pleaſed to protract my life a few years longer, I ſhall at the end of them be able to look upon what I have hitherto written with pity, if not with bluſhes. And I have often obſerved, that it is wont to happen in the productions of the mind, as in thoſe of the body. For as thoſe, that apply themſelves to procreation too young, and before they have attained to their full vigour and ſtrength, do generally both hinder their own growth, and become the parents but of weak and ſhort-lived children; ſo they, that too early, and before their judgment and experience be fully ripe, addict themſelves to write books, do commonly both hinder their own proficiency in knowledge, and write but immature, and therefore ſeldom laſting treatiſes. Nor ſhould I, *Pyrophilus*, have ever prevailed with myſelf to preſent you ſo early theſe diſcourſes, ſince, by keeping them longer by me, I might eaſily by ſecond thoughts, and freſh experiences, be enabled to correct and enrich them, did not the frequent and dangerous diſtempers, to which my very ſickly conſtitution has of late rendered me too obnoxious, make me juſtly doubt, whether or no, if I ſhould long forbear to write, death would not ſooner come than the expected maturity of age and judgment. And though I had no ſuch conſideration to move me to make haſte to tender to you the enſuing diſcourſes, yet this would ſuffice to engage me to preſent them you with all their preſent defects; that if I ſhould keep them till I can make them leſs unworthy of you, I muſt keep them till you are grown paſt the need of them.

AND now that I have told you, *Pyrophilus*, both why I have written the enſuing diſcourſes, and why I keep them not by me long enough to preſent them you with fewer imperfections; I ſuppoſe you will expect, that I ſhould next tell you, why I have caſt them into eſſays, rather than into any other form. To ſatisfy you about this particular, *Pyrophilus*, I muſt freely acknowledge to you, that it has long ſeemed to me none of the leaſt impediments of the real advancement of true natural philoſophy, that men have been ſo forward to write ſyſtems of it, and have thought themſelves obliged either to be altogether ſilent, or not to write leſs than an entire body of phyſiology: for, from hence ſeem to have enſued not a few inconveniences.

AND firſt, when men, by having diligently ſtudied either chymiſtry, anatomy, botanics, or ſome other particular part of phyſiology, or perhaps by having only read authors on thoſe ſubjects, have thought themſelves thereby qualified to publiſh compleat ſyſtems of natural philoſophy, they have found themſelves, by the nature of their undertaking, and the laws of method, engaged to write of ſeveral other things than thoſe, wherein they had made themſelves proficients; and thereby have been reduced, either idly to repeat what has been already, though perhaps but impertinently enough, written by others on the ſame ſubjects; or, elſe to ſay any thing on them rather than nothing, leſt they ſhould appear not to have ſaid ſomething to every part of the theme, which they had taken upon themſelves to write of.

IN the next place, the ſpecious and promiſing titles and comprehenſive method of theſe ſyſtems have been often found to perſuade unwary readers, that all the parts of natural philoſophy have been already ſufficiently explicated; and, that conſequently it were needleſs for them to put themſelves to trouble and charges in making further inquiries into nature, ſince others having already ſufficiently made it their buſineſs to inveſtigate and explicate phyſiological truths, our buſineſs needs now be no more than to learn what they have taught, and thankfully to acquieſce in it.

NOR has the ſyſtematical way of writing been prejudicial only to the proficiency of ſome readers, but alſo to the reputation of ſome writers of ſyſtematical books. For it not unfrequently happens, that when a writer, to vent ſome few peculiar notions or diſcoveries of his own, preſumes to write a whole body of philoſophy, what is truly his own, though excellent in its kind, is either loſt in the croud of the things he has borrowed from others, and ſo comes to be over-looked, or at leaſt not ſufficiently taken notice of, by the reader; or elſe the unwelcome, and yet in ſuch compoſures ſcarce evitable, repetition of many things, that others had, I know not how often, written before, occaſions the laying aſide of the whole book, as a rhapſody of trite and vulgar notions, ſcarce worth the peruſing. And by this means the author often loſes the reputation of his peculiar notions, as well as the reader the benefit of them; and that, which would have made an excellent and ſubſtantial eſſay, paſſes but for a dull and empty book.

BUT the worſt inconvenience of all is yet to be mentioned; and that is, That whilſt this vanity of thinking men obliged to write either ſyſtems or nothing is in requeſt, many excellent notions or experiments are, by ſober and modeſt men, ſuppreſſed; becauſe ſuch perſons being forbidden by their judgment and integrity to teach more than they underſtand, or aſſert more than they can prove, are likewiſe forbidden by cuſtom to publiſh their thoughts and obſervations, unleſs they were numerous enough to ſwell into a ſyſtem. And indeed it may be doubted, whether the ſyſtematical writers have not kept the world from much more uſeful compoſures than they have preſented it with. For there are very few men, if any at all, in the world, that are enriched with a competent ſtock of experiments and and obſervations to make out clearly and ſolidly, I ſay not all the phænomena of nature, but all thoſe, that belong to chymiſtry, anatomy, or any ſuch conſiderable ſubordinate doctrine of phyſiology. And thoſe very men, that are diligent and judicious enough to ſtudy proſperouſly any of thoſe parts of phyſiology, are obliged to ſpend ſo much time in the accurate proſecution of that, and are wont to be thereby made ſo wary, and ſo thoroughly acquainted with the difficulty of phyſiological inveſtigations, that they will leaſt of all men be forward to write ſyſtems.

AND what I ſay, *Pyrophilus*, of the inconveniences, that have hitherto been obſerved to flow from men's forwardneſs to write entire bodies of philoſophy, may be, in its degree, applied to the practice of thoſe, that pretend to give us compleat accounts of chymiſtry, or almoſt (I ſay almoſt) any other conſiderable and comprehenſive part of natural philoſophy: though I deny not, that in ſuch attempts, which are much leſs difficult than the former, ſome men have done mankind conſiderable ſervice, though they have not fully performed what the titles of their writings ſeem to promiſe. Nor am I ſo rigid as to be unwilling, that, from time to time, ſome very knowing writer ſhould publiſh a ſyſtem of phyſiology, or any part of it, according to the beſt authors and obſervations of that time: for ſuch a work may be uſeful, partly, for the inſtructing of youth in ſchools and academies; and partly, that men may have, from time to time, an inventory of what hath been already diſcovered; whereby the needleſs labour of ſeeking after known things may be prevented, and the progreſs of mankind, as to knowledge, might the better appear. But then it is to be wiſhed, that ſuch writings were not publiſhed but by very intelligent perſons, nor till ſome conſiderable improvement have been made in knowledge ſince the laſt work of that nature. Nor would I be thought to diſallow ſuch writings of very learned men, as though they may bear very general titles, yet are not publiſhed by their authors as compleat bodies or ſyſtems of phyſiology, but rather as general principles (almoſt like the hypotheſes of aſtronomers) to aſſiſt men to explicate the already known phænomena of nature. For of ſuch kind of writings, if their authors be (as for the moſt part they are), ſubtile and inquiſitive men, there may be very good uſe, not ſo much by their gratifying the intellect with the plauſible account of ſome of nature's myſteries; as becauſe on the one ſide their writers, to make good their new opinions, muſt either bring new experiments and obſervations, or elſe muſt conſider thoſe, that are known already, after a new manner, and thereby make us take notice of ſomething in them unheeded before; and on the other ſide, the curioſity of readers, whether they like or diſapprove the hypotheſis propoſed, is wont to be thereby excited to make trial of ſeveral things, which ſeeming to be conſequences of this new doctrine, may, by their proving agreeable or repugnant to experiment, either eſtabliſh or overthrow it.

AND that you may know, *Pyrophilus*, what kind of writings I mean, I ſhall name to you the learned *Gaſſendus* his little *Syntagma* of *Epicurus*'s philoſophy, and that moſt ingenious gentleman Monſieur *Deſcartes* his principles of philoſophy. For though I purpoſely refrained, though not altogether from tranſiently conſulting about a few particulars, yet from ſeriouſly and orderly reading over thoſe excellent (though diſagreeing) books, or ſo much as Sir *Francis Bacon*'s *Novum Organum*, that I might not be prepoſſeſſed with any theory or principles, till I had ſpent ſome time in trying what things themſelves would incline me to think; yet beginning now to allow myſelf to read thoſe excellent books, I find by the little I have read in them already, that if I had read them before I began to write, I might have enriched the enſuing eſſays with divers truths, which they now want, and have explicated divers things much better than I fear I have done. But of ſuch writers the number is yet (and will I fear always be) ſo ſmall, that I ſhall not need to make many exceptions, when I treat of the uſefulneſs of writing books of eſſays, in compariſon of that of writing ſyſtematically: or, at leaſt, *Pyrophilus*, whilſt I preſume not to judge of other men's abilities, I hope it may be lawful for me to confeſs freely to you concerning myſelf, that I am very ſenſible of my being far from having ſuch a ſtock of experiments and obſervations, as I judge requiſite to write ſyſtematically; and I am apt to impute many of the deficiencies to be met with in the theories and reaſonings of ſuch great wits as *Ariſtotle*, *Campanella*, and ſome other celebrated philoſophers, chiefly to this very thing, that they have too haſtily, and either upon a few obſervations, or at leaſt without a competent number of experiments, preſumed to eſtabliſh principles, and deliver axioms. For it very rarely otherwiſe happens, than that theories, that are grounded but upon few and obvious experiments, are ſubject to be contradicted by ſome ſuch inſtances, as more free and diligent inquiries into what of nature is more abſtruſe, or even into the leſs obvious qualities of things, are wont to bring to light. I remember, that being once at *Leyden*, I was brought to the top of a tower, where, in a darkened room (ſuch as is now uſed in many places to bring in the ſpecies of external object) a convex glaſs, applied to the only hole, by which light was permitted to enter, did project upon a large white ſheet of paper, held at a juſt diſtance from it, a lively repreſentation of divers of the chief buildings in the town; all which, upon the admiſſion of more light into the room, by opening the window, did immediately diſappear. And methinks, *Pyrophilus*, that in divers of the philoſophical theories, that have been formerly applauded, ſomething not unlike this may be eaſily obſerved. for though, whilſt they are looked on with ſuch a weak and determinate degree of light, they may appear very artificial and well-proportioned fabrics, yet they appear ſo but in that twilight, as it were, which is requiſite to their conſpicuouſneſs. For if but a full light of new experiments and obſervations be freely let in upon them, the beauty of thoſe (delightful, but fantaſtical) ſtructures does immediately vaniſh.

AND truly, *Pyrophilus*, if men could be perſuaded to mind more the advancement of natural philoſophy than that of their own reputations, it were not, methinks, very uneaſy to make them ſenſible, that one of the conſiderableſt ſervices, that they could do mankind, were to ſet themſelves diligently and induſtriouſly to make experiments and collect obſervations, without being over-forward to eſtabliſh principles and axioms, believing it uneaſy to erect ſuch theories, as are capable to explicate all the phænomena of nature, before they have been able to take notice of the tenth part of thoſe phænomena, that are to be explicated. Not that I at all diſallow the uſe of reaſoning upon experiments, or the endeavouring to diſcern as early as we can the confederations, and differences, and tendencies of things: for ſuch an abſolute ſuſpenſion of the exerciſe of reaſoning were exceeding troubleſome, if not impoſſible. And, as in that rule of arithmetic, which is commonly called *regula falſi*, by proceeding upon a conjecturally-ſuppoſed number, as if it were that, which we inquire after, we are wont to come to the knowledge of the true number ſought for; ſo in phyſiology it is ſometimes conducive to the diſcovery of truth, to permit the underſtanding to make an hypotheſis, in order to the explication of this or that difficulty, that by examining how far the phænomena are, or are not, capable of being ſolved by that hypotheſis, the underſtanding may, even by its own errors, be inſtructed. For it has been truly obſerved by a great philoſopher, that truth does more eaſily emerge out of error than confuſion. That then, that I wiſh for, as to ſyſtems, is this, that men, in the firſt place, would forbear to eſtabliſh any theory, till they have conſulted with (though not a fully competent number of experiments, ſuch as may afford them all the phænomena to be explicated by that theory, yet) a conſiderable number of experiments, in proportion to the comprehenſiveneſs of the theory to be erected on them. And, in the next place, I would have ſuch kind of ſuperſtructures looked upon only as temporary ones; which though they may be preferred before any others, as being the leaſt imperfect, or, if you pleaſe, the beſt in their kind that we yet have, yet are they not entirely to be acquieſced in, as abſolutely perfect, or uncapable of improving alterations.

It were very poſſible, *Pyrophilus*, to let you ſee, that all that has been ſaid to recommend to you that form of writing, which (in imitation of the French) we call eſſays, is but a part of what may be pertinently ſaid to the ſame purpoſe. But becauſe this introductory diſcourſe itſelf is to be but an eſſay, not a book, I dare not long inſiſt upon the advantages of this ſort of diſcourſes. Only becauſe I think, that if I could engage you, *Pyrophilus*, and ſuch other ingenious perſons, to caſt their phyſiological obſervations and reflections into experimental eſſays, I ſhould thereby do real learning no trifling ſervice, by bringing ſo uſeful a way of writing into the requeſt it deſerves; upon this conſideration, I ſay, I muſt beg leave to repreſent to you this great conveniency of eſſays, that as in them the reader needs not be clogged with tedious repetitions of what others have ſaid already, ſo the writer, having for the moſt part the liberty to leave off when he pleaſes, is not obliged to take upon him to teach others what himſelf does not underſtand, nor to write of any thing but of what he thinks he can write well. And if ſuch eſſays be but as they ſhould be competently ſtocked with experiments, it is the reader's own fault, if he be not a learner by them: for indeed when a writer acquaints me only with his own thoughts or conjectures, without inriching his diſcourſes with any real experiment or obſervation, if he be miſtaken in his ratiocination, I am in ſome danger of erring with him, and at leaſt am like to loſe my time, without receiving any valuable compenſation for that great loſs: but if a writer endeavours, by delivering new and real obſervations or experiments, to credit his opinions, the caſe is much otherwiſe; for let his opinions be never ſo falſe, his experiments being true, I am not obliged to believe the former, and am left at liberty to benefit myſelf by the latter; and though we have erroneouſly ſuperſtructed upon his experiments, yet the foundation being ſolid, a more wary builder may be very much furthered by it in the erection of more judicious and conſiſtent fabrics. Such a writer, if I be not wanting to myſelf, will certainly teach me uſeful truths, and if it be not my fault, he can lead me into no errors; and oftentimes the very experiments, that he delivers, beſides that they may be applicable to many other purpoſes unthought of by him, may be either ſufficient, or at leaſt helpful to the very diſcovery of the erroneouſneſs of the opinions they are alleged to countenance: and I may account, that a man, that gives me, together with his conjectures (though erroneous) in matters of phyſiology, ſome noble experiment or obſervation, by which he pretends to verify them, does me no greater injury, than *Galileo* upon his firſt invention of the teleſcope would have done an aſtronomer, if he had told him, that he had diſcovered in heaven thoſe imaginary new ſtars, which a late mathematician has fancied himſelf to have deſcried there, and at the ſame time had made him a preſent of an excellent teleſcope, with expectation thereby the receiver ſhould be made of the giver's opinion; for by the help of his inſtrument the aſtronomer might not only make divers uſeful obſervations in the ſky, and perhaps detect new lights there, but diſcern alſo his miſtake, that gave it him.

AFTER what has been ſaid, *Pyrophilus*, of the uſefulneſs of experimental eſſays, we muſt proceed to ſay ſomething concerning the manner of writing them: but becauſe you will alſo expect to receive ſome account of the enſuing diſcourſes, I ſhall not treat of thoſe two ſubjects a-part, but, in diſcourſing of the following eſſays, ſhall take occaſion to acquaint you with part of my thoughts concerning ſuch kind of compoſures in general, the other conſiderations belonging to the ſame ſubject being already upon ſeveral occaſions diſperſed among, and to be met with in the enſuing diſcourſes themſelves.

AND firſt, as for the ſtyle of our experimental eſſays, I ſuppoſe you will readily find, that I have endeavoured to write rather in a philoſophical than a rhetorical ſtrain, as deſiring, that my expreſſions ſhould be rather clear and ſignificant, than curiouſly adorned: for to a ſubject of the ſerious and important nature of phyſiology that ſaying may unqueſtionably be applied, *Ornari res ipſa negat, contenta doceri.* And certainly in theſe diſcourſes, where our deſign is only to inform readers, not to delight or perſuade them, perſpicuity ought to be eſteemed at leaſt one of the beſt qualifications of a ſtyle; and to affect needleſs rhetorical ornaments in ſetting down an experiment, or explicating ſomething abſtruſe in nature, were little leſs improper, than it were (for him that deſigns not to look directly upon the ſun itſelf) to paint the eye-glaſſes of a teleſcope, whoſe clearneſs is their commendation, and in which even the moſt delightful colours cannot ſo much pleaſe the eye, as they would hinder the ſight. And that it may not be ſuſpected, that thoſe, that would not have it requiſite to imploy a florid ſtyle in treating of philoſophical ſubjects, do but in their own excuſe deny the neceſſity of ſuch rhetorical embelliſhments as they are not able to afford their compoſures, give me leave to ſubjoin, that it was not an unpoliſhed naturaliſt, but that prince of orators, *Cicero* himſelf, who made this ſtudious declaration; *Omne* (ſays he) *quod de re bona dilucidè dicitur, præclarè mihi dici videtur: iſtiuſmodi autem res velle ornatè dicere, puerile eſt: planè autem & perſpicuè expedire poſſe, docti & intelligentis viri.* But I muſt not ſuffer myſelf to ſlip unawares into the common place of the unfitneſs of too ſpruce a ſtyle for ſerious and weighty matters; and yet I approve not that dull and inſipid way of writing, which is practiſed by many chymiſts, even when they digreſs from phyſiological ſubjects. For though a philoſopher need not be ſollicitous, that his ſtyle ſhould delight its reader with his floridneſs, yet I think he may very well be allowed to take a care, that it diſguſt not his reader by its flatneſs, eſpecially when he does not ſo much deliver experiments or explicate them, as make reflections or diſcourſes on them: for on ſuch occaſions he may be allowed the liberty of recreating his reader and himſelf, and manifeſting, that he declined the ornaments of language, not out of neceſſity, but diſcretion, which forbids them to be uſed, where they may darken as well as adorn the ſubject they are applied to. Thus (to reſume our former compariſon) though it were fooliſh to colour or enamel upon the glaſſes of teleſcopes, yet to gild or otherwiſe embelliſh the tubes of them, may render them more acceptable to the uſers, without at all leſſening the clearneſs of the object to be looked at through them.

AND as for exotic words and terms borrowed from other languages, though I expect, that perſons not converſant in the philoſophical compoſures written (eſpecially of late) in our language will be apt to ſuſpect me for the willing author of divers new words and expreſſions; yet as for you, *Pyrophilus*, who peruſe other than moral, theological, and hiſtorical books in Engliſh, and find how much uſe is made in them of exotic terms, I hope you will find, that I have not at all affected them, but have rather ſtudiouſly declined the uſe of thoſe, which cuſtom has not rendered familiar, unleſs it be to avoid the frequent and unwelcome repetition of the ſame word (ſo troubleſome to the ear, and ſo much forbidden by orators) or for ſome peculiar ſignificancy of ſome ſuch word, whoſe energy cannot be well expreſſed in our language, at leaſt without a tedious circumlocution. And in ſuch caſes, *Pyrophilus*, I ſuppoſe a writer may be allowed to uſe exotic terms; eſpecially when cuſtom has not only denizened them, but brought them into requeſt. For, as in the faſhions of clothes, though perhaps fools begin them, yet wiſe men, when they are once generally received, ſcruple not to follow them, becauſe then obſtinately to decline them would be as ridiculouſly ſingular as at firſt it would have been to begin them: ſo in exotic words, when cuſtom has once made them familiar and eſteemed, ſcrupulouſly to decline the uſe of them may be as well a fault, as needleſsly to imploy them: for it is not the uſe, but the affectation of them, that is unworthy a philoſopher. And from the latter of thoſe I hope I have kept myſelf far enough; for I ſhould think myſelf guilty of a very childiſh vanity, if the uſe I made of languages were ſo to write as to be the leſs underſtood. But beſides the unintentional deficiencies of my ſtyle, I have knowingly and purpoſely tranſgreſſed the laws of oratory in one particular, namely, in making ſometimes my periods or parentheſes over-long: for when I could not within the compaſs of a regular period compriſe what I thought requiſite to be delivered at once, I choſe rather to neglect the precepts of rhetoricians, than the mention of thoſe things, which I thought pertinent to my ſubject, and uſeful to you, my reader. And for this fault, *Pyrophilus*, ſince I have made myſelf guilty of it but for your ſake, I think I ought to obtain your pardon at leaſt as eaſily as my own, ſince barely to keep you from loſing any thing, that I conceived might be ſerviceable to you, I knowingly expoſe my ſtyle to be cenſured as diſproportionate to itſelf.

THE next thing, *Pyrophilus*, of which I am to give you an account, is, why I have in the enſuing eſſays delivered many experiments and obſervations, which may ſeem ſlight and eaſy, and ſome of them obvious alſo, or elſe perhaps mentioned by others already. To ſatisfy you about this, I muſt inform you, that many of the particulars, which we are now conſidering, were in my firſt deſign collected in order to a continuation of the Lord *Verulam*'s *Sylva Sylvarum*, or natural hiſtory. And that my intended centuries might reſemble his, to which they were to be annexed, it was requiſite, that ſuch kind of experiments and obſervations, as we have been newly ſpeaking of, ſhould make up a conſiderable part of them. And indeed it were to be wiſhed, that ſuch inquiſitive perſons, as cannot be at the charge, or have not the opportunity, of making new experiments, would buſy themſelves, as they have opportunity, in induſtriouſly collecting, and carefully ſetting down the phænomena to be met with without the aſſiſtance of new experiments, eſpecially ſuch particulars, as ſeem either to be of moment in order to the hinting or confirmation of ſome conſiderable truth, or to the detection of ſome applauded error, or elſe to have been, though obvious enough, yet little taken notice of. For I am confident, that very much may be done towards the improvement of philoſophy by a due conſideration of, and reflexion on, the obvious phænomena of nature, and thoſe things, which are almoſt in every body's power to know, if he pleaſes but ſeriouſly to heed them; and I make account, that attention alone might quickly furniſh us with one half of the hiſtory of nature, as well as induſtry is requiſite, by new experiments, to enrich us with the other. And therefore I confeſs I think myſelf beholden to him, that firſt makes me take notice of what I might eaſily have known, but heeded not before: it not ſeldom happening, that we are prejudiced by, though we do not complain of that ignorance, from which we might relieve ourſelves, if we did but diligently turn our eyes to the obſervations, wherewith even neighbouring and familiar objects would, if duly conſulted, preſent us. But I digreſs; and therefore muſt ſtep back into the way, and tell you, that the reaſons, why I firſt deſigned the narrative of what I had tried and obſerved for a continuation of Sir *Francis Bacon*'s Natural Hiſtory, you will meet with in my preface to that ſpecimen of the intended continuation, which I have given to thoſe of my eſſays, that treat of promiſcuous experiments: and the reaſon, why I have ſince declined that ſuccinct way of writing, is for the ſake of *Pyrophilus*, that I might have, in a more free and uncircumſcribed way of diſcourſing, a greater liberty to inſiſt on and manifeſt the reaſonableneſs of ſuch animadverſions, as I thought ſeaſonable for a perſon, who, though a great proficient in the other parts of philoſophy, is but a beginner in experimental learning. And the ſecond reaſon, why I have often made uſe of ſeemingly ſlight experiments, is, becauſe ſuch are more eaſily and cheaply tried, and they being alleged for the moſt part to prove ſome aſſertion, or credit ſome admonition, I thought their eaſineſs or obviouſneſs fitter to recommend them, than depreciate them; and I judged it ſomewhat unkind, or at leaſt indiſcreet, to refer you moſt commonly for proof of what I delivered, to ſuch tedious, ſuch difficult, or ſuch intricate proceſſes, as either you can ſcarce well make, unleſs you be already, what I deſire my experiments ſhould help to make you, a ſkilful chymiſt; or elſe are as difficult to be well judged, as the truth they ſhould diſcover is to be diſcerned. I was alſo hopeful, that the eaſineſs of divers things inviting you to make trial of them, and keeping you from being diſappointed in your expectations, the ſucceſs of your firſt attempts would incourage you to make trial alſo of more nice and difficult experiments. And till you have tried them, do me the right to think, that I deal not unſincerely with you.

THE reaſons of my having divers times recorded experiments, which you may have formerly met with, and perchance even in printed books, I have elſewhere deduced in a peculiar diſcourſe on that ſubject; and therefore ſhall now only add, that by reaſon of my being as yet a ſtranger to the German tongue, wherein the moſt and beſt chymical books are ſaid to be written, I may have ſet down divers chymical experiments and obſervations, that are extant already in that Hermetical language, (if I may ſo call it) without having had them from their Dutch publiſhers, or ſo much as dreamed of their having been divulged by any man. I have likewiſe in my preface to the eſſays, that you will meet with under the title of *Promiſcuous Experiments*, given you an account, why I have not refrained from mentioning divers things, which may ſeem very ſlight, becauſe very obvious: and I have long had thoughts to inform you in an intire diſcourſe to be written on purpoſe, why I think, that even the trivial, and therefore ſlighted, truths of phyſiology ought not to be deſpiſed. And for my own part, I ſhall not ſcruple to confeſs to you, that I diſdain not to take notice even of ludicrous experiments, and think, that the plays of boys may ſometimes deſerve to be the ſtudy of philoſophers: for as when we go a hunting, though the flight of the hare and the purſuit of the dogs be to us but ſport and recreation, yet the beaſts themſelves are extreamly earneſt, the one to ſave his threatened life by flight, and the other to overtake his deſired prey; ſo nature acts very ſeriouſly in all the other things, that we make ſports with, and is in very good earneſt, whether we men be ſo or no.

PERHAPS you will wonder, *Pyrophilus*, that in almoſt every one of the following eſſays I ſhould ſpeak ſo doubtingly, and uſe ſo often, *perhaps*, *it ſeems*, *it is not improbable*, and ſuch other expreſſions, as argue a diffidence of the truth of the opinions I incline to, and that I ſhould be ſo ſhy of laying down principles, and ſometimes of ſo much as venturing at explications. But I muſt freely confeſs to you, *Pyrophilus*, that having met with many things, of which I could give myſelf no one probable cauſe, and ſome things, of which ſeveral cauſes may be aſſigned ſo differing, as not to agree in any thing, unleſs in their being all of them probable enough; I have often found ſuch difficulties in ſearching into the cauſe and manner of things, and I am ſo ſenſible of my own diſability to ſurmount thoſe difficulties, that I dare ſpeak confidently and poſitively of very few things, except of matters of fact. And when I venture to deliver any thing, by way of opinion, I ſhould, if it were not for mere ſhame, ſpeak yet more diffidently than I have been wont to do. It is not, that I at all condemn the practice of thoſe inquiſitive wits, that take upon them to explicate to us even the abſtruſeſt phænomena of nature; for I am ſo far from cenſuring them, that I admire them, when their endeavours ſucceed, and applaud them even where they do but fairly attempt. But I think it is fit for a man to know his own abilities and weakneſſes, and not to think himſelf obliged to imitate all that he thinks fit to praiſe. I know alſo, that the way to get reputation is to venture to explicate things, and promote opinions; for by that courſe a writer ſhall be ſure to be applauded by one ſort of men, and be mentioned by many others; whereas by the way of writing, to which I have condemned myſelf, I can hope for little better among the more daring and leſs conſiderate ſort of men, ſhould you ſhew them theſe papers, than to paſs for a drudge of greater induſtry than reaſon, and fit for little more, than to collect experiments for more rational and philoſophical heads to explicate and make uſe of. But I am content, provided experimental learning be really promoted, to contribute even in the leaſt plauſible way to the advancement of it; and had rather not only be an under-builder, but even dig in the quarries for materials towards ſo uſeful a ſtructure, as a ſolid body of natural philoſophy, than not do ſomething towards the erection of it. Nor have my thoughts been altogether idle and wanting to themſelves, in framing notions, and attempting to deviſe hypotheſes, which might avoid the deficiencies obſerved in other men's theories and explications: but I have hitherto, though not always, yet not unfrequently, found, that what pleaſed me for a while, as fairly comporting with the obſervations, on which ſuch notions were grounded, was ſoon after diſgraced by ſome further or new experiment, which at the time of the framing of thoſe notions was unknown to me, or not conſulted with. And indeed I have the leſs envied many (for I ſay not all) of thoſe writers, who have taken upon them to deliver the cauſes of things, and explicate the myſteries of nature, ſince I have had opportunity to obſerve, how many of their doctrines, after having been for a while applauded, and even admired, have afterwards been confuted by the diſcovery of ſome new phænomenon in nature, which was either unknown to ſuch writers, or not ſufficiently conſidered by them. For I have found it happen, as well to many others (that have publiſhed their opinions) as to me (who have been more private in my gueſſes) in our theories built on either too obvious or too few experiments, what is wont to happen to the falſifiers of coin; for as counterfeit pieces of money will endure ſome of them one proof, as the touch-ſtone, others another, as aqua fortis, ſome a third, as the hammer or the ſcales, but none of them will endure all proofs; ſo the notions I mention (in which ſort I fear too great a part of thoſe hitherto extant may be comprized) may agree very fairly with this or that or the other experiment; but being made too haſtily, and without conſulting a competent number of them, it is to be feared, that there may ſtill after a while be found one or other, (if not many) their inconſtancy with which will betray and diſcredit them.

I HAVE notwithſtanding all this, on ſome occaſions, adventured to deliver my opinion; not that I am very confident of being leſs ſubject to err in thoſe particulars, than in any of the others, wherein I have refrained from interpoſing any conjecture; but becauſe I would manifeſt to you, that I ſcruple not to run the ſame venture with thoſe incomparably better naturaliſts, that have thought it no diſgrace in difficult matters rather to hazard the being ſometimes miſtaken, than not to afford inquiſitive perſons their beſt aſſiſtance towards the diſcovery of truth.

AND becauſe, *Pyrophilus*, in the reaſons and explications I offer of natural effects, I have not for the moſt part an immediate recourſe to the magnitude, figure, and motion of atoms, or of the leaſt particles of bodies, I hold it not unfit to give you here ſome account of this practice; not ſo much for the ſake of thoſe few paſſages in my eſſays, that may be concerned in it, as for that of many learned men, eſpecially phyſicians, whoſe uſeful writings being to be undervalued, and are in danger to be deſpiſed, by an opinion taken up from the miſ-underſtood doctrine of ſome eminent Atomiſts, as if no ſpeculations in natural philoſophy could be rational, wherein any other cauſes of things are aſſigned than atoms and their properties. I conſider then, that generally ſpeaking, to render a reaſon of an effect or phænomenon, is to deduce it from ſomething elſe in nature more known than itſelf; and that conſequently there may be divers kinds of degrees of explication of the ſame thing. For although ſuch explications be the moſt ſatisfactory to the underſtanding, wherein it is ſhewn, how the effect is produced by their more primitive and catholick affections of matter, namely, bulk, ſhape and motion; yet are not theſe explications to be deſpiſed, wherein particular effects are deduced from the more obvious and familiar qualities or ſtates of bodies, ſuch as heat, cold, weight, fluidity, hardneſs, fermentation, *&c.* though theſe themſelves do probably depend upon thoſe three univerſal ones formerly named. For in the ſearch after natural cauſes, every new meaſure of diſcovery does both inſtruct and gratify the underſtanding; though I readily confeſs, that the nearer the diſcovered cauſes are to thoſe, that are higheſt in the ſcale or ſeries of cauſes, the more is the intellect both gratified and inſtructed.

I THINK it therefore very fit and highly uſeful, that ſome ſpeculative wits, well verſed in mathematical principles and mechanical contrivances, ſhould employ themſelves in deducing the chiefeſt modes or qualities of matter, ſuch as are heat, cold, *&c.* and the ſtates or conditions of it (if we think fit to diſtinguiſh theſe from its qualities) as fluid, firm, brittle, flexible, and the like, from the above-mentioned moſt primitive and ſimple affections thereof. And, I think, the commonwealth of learning

ing exceedingly beholden to thoſe heroick wits, that do ſo much as plauſibly perform ſomething in this kind. But I think too, we are not to deſpiſe all thoſe accounts of particular effects, which are not immediately deduced from thoſe primitive affections of either atoms, or the inſenſible particles of matter; but from the familiar, though not ſo univerſal, qualities of things, as cold, heat, weight, hardneſs, and the like. And perhaps it would be none of the leaſt advantages, which would accrue to Naturaliſts from a ſatisfactory explication of ſuch qualities by the moſt primitive and ſimple ones, that it would much ſhorten the explication of particular phænomena. For though there be many things in nature, that may be readily enough made out by the ſize, motion, and figure of the ſmall parts of matter; yet there are many more, that cannot be well explained without a great deal of diſcourſe, and divers ſucceſſive deductions of one thing from another, if the purpoſed effect muſt be deduced from ſuch primary and univerſal cauſes; whereas if we be allowed to take the notions of cold, heat, and the like qualities for granted, the explications and proofs may be much more compendiouſly made. He gives ſome reaſon, why ſtones and iron, and all other heavy bodies, will ſwim in quickſilver, except gold, which will ſink in it; that teaches, that all thoſe other bodies are *in ſpecie* (as they ſpeak) or bulk for bulk, lighter than quickſilver, whereas gold is heavier. He, I ſay, may be allowed to have rendered a reaſon of a thing propoſed, that thus refers the phænomenon to that known affection of almoſt all bodies here below, which we call gravity, though he does not deduce the phænomenon from atoms, nor give us the cauſe of gravity; as indeed ſcarce any philoſopher has yet given us a ſatisfactory account of it. So if it be demanded, why, if the ſides of a blown bladder be ſomewhat ſqueezed betwixt one's hands, they will, upon the removal of that which compreſſed them, fly out again, and reſtore the blader to its former figure and dimenſions; it is not ſaying nothing to the purpoſe, to ſay, that this happens from the ſpring of thoſe aërial particles, wherewith the bladder is filled, though he, that ſays this, be not perhaps able to declare, whence proceeds the motion of reſtitution, either in a particle of compreſſed air, or any other bent ſpring.

AND as for the reaſons of things aſſigned by phyſicians, they muſt be by moſt of them deſpiſed, unleſs we will allow of ſuch explications, as deduce not things from atoms or their affections, but only either from ſecondary qualities, or from the more particular properties of mixt bodies. If a phyſician be aſked, why rhubarb does commonly cure looſeneſſes, he will probably tell you as a reaſon, that rhubarb is available in ſuch diſeaſes, becauſe it hath both a laxative virtue, whereby it evacuates choler, and ſuch other bad humours as are wont, in ſuch caſes, to be the peccant matter; and an aſtringent quality, whereby it afterwards arreſts the flux. But if you further aſk him the reaſon, why rhubarb purges, and why it purges choler more than any other humour; it is ten to one he will not be able to give you a ſatiſfactory anſwer. And indeed, not only the manner, whereby purgative medicines work, but thoſe other properties, whereby ſome bodies are diuretick, others ſodorifick, others ſarcotick, &c. are not, I fear, ſo eaſy to be intelligibly made out as men imagine, and yet a ſkilful phyſician would juſtly think himſelf wronged, if the reaſons he renders of things in his own profeſſion were denied the name of reaſons, becauſe made without recourſe to atomical principles. And indeed, there are oftentimes ſo many ſubordinate cauſes between particular effects and the moſt general cauſes of things, that there is left a large field, wherein to exerciſe men's induſtry and reaſon, if they will but ſolidly enough deduce the properties of things from more general and familiar qualities, and alſo intermediate cauſes (if I may ſo call them) from one another. And I am the more backward to deſpiſe ſuch kind of reaſons, becauſe I elſewhere declare, that there are ſome (for I do not ſay, many) things, as particularly the origin of local motion, of which, even by the atomical doctrine, no phyſical cauſe can well be rendered; ſince either ſuch things muſt be aſcribed to God, who is indeed the true, but ſupernatural cauſe of them; or elſe it muſt be ſaid (as it was by the old Epicurians) that they did ever belong to matter: which, conſidering that the notion of matter may be compleat without them, is not to give a phyſical efficient cauſe of things in queſtion, but in effect to confeſs that they have no ſuch cauſes. But of this elſewhere more.

IN the mean time, that you may not be drawn away to undervalue ſuch writers as I have been pleading for, nor think you ought to refrain from writing what occurs to you, though true and uſeful, unleſs you deduce it, or at leaſt can do ſo, from the Epicurian notions; I ſhall here briefly repreſent to you (what perhaps you will not hereafter think a deſpicable ſuggeſtion) that there are two very diſtinct ends, that men may propound to themſelves in ſtudying natural philoſophy. For ſome men care only to know nature, others deſire to command her; or, to expreſs it otherwiſe, ſome there are, who deſire but to pleaſe themſelves by the diſcovery of the cauſes of the known phænomena; and others would be able to produce new ones, and bring nature to be ſerviceable to their particular ends, whether of health, or riches, or ſenſual delight. Now as I ſhall not deny, but that the atomical, the Carteſian, or ſome ſuch principles, are likely to afford the moſt of ſatisfaction to thoſe ſpeculative wits, that aim but at the knowledge of cauſes; ſo I think, that the other ſort of men may very delightfully and ſucceſsfully proſecute their ends, by collecting and making variety of experiments and obſervations; ſince thereby learning the qualities and properties of thoſe particular bodies they deſire to make uſe of, and obſerving the power that divers chymical operations, and other ways of handling matter, have of altering ſuch bodies, and varying their effects upon one another, they may, by the help of attention and induſtry, be able to do many things, ſome of them very ſtrange, and more of them very uſeful in human life. When a gunner or a ſoldier employs gunpowder, it is not neceſſary, that he ſhould conſider, or ſo much as know, of what, and of how many ingredients (much leſs of what kind of atoms) it is made, and the proportion and manner, wherein they are mingled; but the notice experience gives him of the power of that admirable concrete, as it is made up and brought to his hands, ſuffices to enable him to perform things with it, that nothing but their being common and unheeded can keep from being admired. The phyſician, that has obſerved the medicinal virtues of treacle, without knowing ſo much as the names, much leſs the nature of each of the ſixty and odd ingredients, whereof it is compounded, may cure many patients with it. And though it muſt not be denied, that it is an advantage as well as a ſatisfaction, to know in general, how the qualities of things are deducible from the primitive affections of the ſmalleſt parts of matter; yet whether we know that or no, if we know the qualities of this or that body they compoſe, and how it is diſpoſed to work upon other bodies, or be brought on by them, we may, without aſcending to the top in the ſeries of cauſes, perform things of great moment; and ſuch, as without the diligent examination of particular bodies, would, I fear, never have been found out *à priori*, even by the moſt profound contemplators. We ſee that the artificers, that never dreamed of the Epicurian philoſophy, have accommodated mankind with a multitude of uſeful inventions. And *Paracelſus*, who (beſides that he ſeems none of the moſt piercing and ſpeculative wits) ſure had little recourſe to atomical notions, if he ever ſo much as heard of them, was able to perform ſome things, that were truly admirable, beſides thoſe he vainly boaſted of; as may appear not only by what I elſewhere repreſent, but by what *Oporinus* himſelf (as ſeverely as he otherwiſe writes againſt his deſerted maſter) confeſſes he ſaw of the ſtupendous cures, which *Paracelſus* wrought with his famous *Laudanum* (whatever he made it of). But we need not go far to find a noble example to our preſent purpoſe, ſince we ſee, that the bare making of trials with the loadſtone, and irons touched by it, though the experimenters were ignorant (as ſome fear we yet are) of the true and firſt cauſes of magnetical phænomena, have produced inventions of greater uſe to mankind, than were ever made by *Leucippus*, or *Epicurus*, or *Ariſtotle*, or *Teleſius*, or *Campanella*, or perhaps any of the ſpeculative deviſers of new hypotheſes; whoſe contemplations aiming for the moſt part but at the ſolving, not the increaſing or applying, of the phænomena of nature, it is no wonder they have been more ingenious than fruitful, and have hitherto more delighted than otherwiſe benefited mankind: I ſay hitherto, becauſe though experience warrants me ſo to ſpeak now, yet I am not unwilling to think, that hereafter, and perhaps in no long time, when phyſiological theories ſhall be better eſtabliſhed, and built upon a more competent number of particulars, the deductions, that may be made from them, may free them from all imputation of barrenneſs. But of theſe things I otherwhere (though not, as I remember, in any of the following eſſays) more fully diſcourſe.

AND therefore I ſhall now reſume the ſubject, that occaſioned this long excurſion, and add to what I ſaid in excuſe of my venturing ſometimes to deliver ſomething as my opinion in difficult or controverted caſes, that I muſt declare to you, *Pyrophilus*, that as I deſire not my opinions ſhould have more weight with you, than the proofs brought to countenance them will give them; ſo you muſt not expect, that I ſhould think myſelf obliged to adhere to them any longer, than thoſe conſiderations, that firſt made me embrace them, ſhall ſeem of greater moment, than any that I can meet with in oppoſition to them. For *Ariſtotle* ſpoke like a philoſopher, when to juſtify his diſſent from his maſter *Plato*, he ſaid, among other things, that for the ſake of truth, men (eſpecially being philoſophers) ought to overthrow even their own tenets (Δόξει δ' ἂν ἴσως βέλτιον εἶναι, ϗ δεῖν ἐπὶ σωτηρίᾳ γε τῆς ἀληθείας, ϗ τὰ οἰκεῖα ἀναιρεῖν, ἄλλως τε ϗ φιλοσόφους ὄντας.) *Ethic. Nicom.* l. 1. cap. 6. And though for a man to change his opinions, without ſeeing more reaſon to forſake them than he had to aſſent to them, be a cenſurable levity and inconſtancy of mind; yet to adhere to whatever he once took for truth, though by acceſſion of more light he diſcover it to be erroneous, is but a proud obſtinacy, very injurious to truth, and very ill becoming the ſenſe we ought to have of human frailties. And it ought to be eſteemed much leſs diſgraceful to quit an error for a truth, than to be guilty of the vanity and perverſeneſs of believing a thing ſtill, becauſe we once believed it. And certainly, till a man is ſure he is infallible, it is not fit for him to be unalterable.

YOU will eaſily diſcern, *Pyrophilus*, that I have purpoſely, in the enſuing eſſays, refrained from ſwelling my diſcourſes with ſolemn and elaborate confutations of other men's opinions, unleſs it be in ſome very few caſes, where I judged, that they might prove great impediments to the advancement of experimental learning; and even ſuch opinions I have been wary of meddling with, unleſs I ſuppoſed I could bring experimental objections againſt them. For it is none of my deſign to engage myſelf with, or againſt, any one ſect of Naturaliſts, but barely to invite you to embrace or refuſe opinions, as they are conſonant to experiments, or clear reaſons deduced thence, or at leaſt analogous thereunto; without thinking it yet ſeaſonable to contend very earneſtly for thoſe other opinions, which ſeem not yet determinable by ſuch experiments

ments or reaſons. And indeed, to allude to our former compariſon, I would endeavour to deſtroy thoſe curious but groundleſs ſtructures, that men have built up of opinions alone, by the ſame way (and with as little noiſe) by which ſuch fantaſtical ſtructures, as thoſe I mentioned to have ſeen at *Leyden*, may be demoliſhed. To deſtroy which, it were needleſs to bring battering engines, ſince nothing is requiſite to that effect but an increaſe of light. And experience has ſhown us, that divers very plauſible and radicated opinions, ſuch as that of the unhabitableneſs of the torrid zone, of the ſolidity of the celeſtial part of the world, of the blood's being conveyed from the heart by the veins (not the arteries) to the outward parts of the body, are generally grown out of requeſt, upon the appearing of thoſe new diſcoveries, with which they are inconſiſtent, and would have been abandoned by the generality of judicious perſons, though no man had made it his buſineſs, purpoſely, to write confutations of them: ſo true is that vulgar ſaying, that *Rectum eſt index ſui & obliqui.*

BUT when at any time, *Pyrophilus*, I have been induced to oppoſe others, as I have not denied myſelf the freedom, that is requiſite unto loyalty to truth; ſo I have endeavoured to uſe that moderation, and civility, that is due to the perſons of deſerving men. And therefore you ſhall find me not only in one eſſay oppoſe an author, whom in another I applaud; but ſometimes you may find me commending a writer in the very ſame page, perhaps, where I am endeavouring to diſprove his opinions: for I love to ſpeak of perſons with civility, though of things with freedom. Nor do I think it reaſonable, either that any man's reputation ſhould protect his errors, or that the truth ſhould fare the worſe for his ſake, that delivers it. And as for the (very much too common) practice of many, who write, as if they thought railing at a man's perſon, or wrangling about his words, neceſſary to the confutation of his opinions; beſides that I think ſuch a quarrelſome and injurious way of writing does very much miſbecome both a philoſopher and a Chriſtian, methinks it is as unwiſe, as it is provoking. For if I civilly endeavour to reaſon a man out of his opinions, I make myſelf but one work to do, namely, to convince his underſtanding; but, if in a bitter or exaſperating way I oppoſe his errors, I increaſe the difficulties I would ſurmount, and have as well his affections againſt me as his judgment: and it is very uneaſy to make a proſelyte of him, that is not only a diſſenter from us, but an enemy to us. And that which makes me the more diſlike the bitter way of diſputing, which I am reprehending, is, that I have often obſerved, that though one of the diſputants alone be at firſt in the fault, yet the other is moſt commonly drawn to ſhare in the guilt, though, to contract it, he muſt imitate his adverſary. For as a mad dog, by biting others, is wont to make thoſe he bites run mad like himſelf, and do promiſcuous miſchief; ſo theſe ſo provoking writers are wont to enrage thoſe they offend, and infect them alſo with their own virulent diſtemper. But, *Pyrophilus*, when I ſpeak of dealing reſpectfully with thoſe I diſſent from, I would be underſtood of ſuch, as have well deſerved of experimental learning, or at leaſt been candid and ſober inquirers after truth. For, as I think, that it would much diſcourage any prudent perſon from venturing to communicate what he thinks he knows to the world, to find, that an error proceeding from human weakneſs, or the darkneſs and abſtruſity of things, ſeldom eſcapes being detected without being made matter of diſgrace or reproach to the author: ſo, on the other ſide, when vain writers, to get themſelves a name, have preſumed to intrude upon the credulous world ſuch things, under the notion of experimental truths, or even great myſteries, as neither themſelves ever took the pains to make trial of, nor received from any credible perſons that profeſſed themſelves to have tried them; in ſuch caſes, I ſee not how we are obliged to treat writers, that took no pains to keep themſelves from miſtaking or deceiving, nay, that cared not how they abuſe us to win themſelves a name, with the ſame reſpect, that we owe to thoſe, who, though they have miſſed of the truth, believed they had found it, and both intended to deliver it, and took ſome (though not proſperous) pains, that they might convey nothing elſe to us.

I FEAR it will be requiſite, *Pyrophilus*, to tell you, why in ſome of the following eſſays you will meet with many paſſages tranſcribed out of other authors, and in ſome very few or none at all. And therefore to give you firſt a ſhort account of the particular mentioned laſt, I muſt mind you, that it was moſt ſuitable both to my humour and deſign, to deliver only thoſe things, wherewith my own obſervations, or trials, or thoughts, had furniſhed me, without troubling you with the repetition of thoſe things, which had been delivered by others already; thoſe kind of repetitions, unleſs they be made upon ſome ſuch grounds as we ſhall preſently mention, ſeeming to me to be as vainly as ambitiouſly affected by many writers, and being deſervedly troubleſome to judicious readers, who can eaſily diſcern, that they add much more to the bulk of books than of knowledge. But this notwithſtanding, *Pyrophilus*, I thought myſelf obliged on ſome occaſions, for your ſake, to overcome my natural averſeneſs to ſtuff any writings of mine with paſſages tranſcribed from thoſe of others, partly for the reaſons elſewhere inſiſted on, and partly for divers others. As firſt, becauſe ſome particulars are of that ſtrangeneſs, and of that moment, that they need and deſerve to be verified by more than a ſingle atteſtation. Next, becauſe according to the Greek proverb, κοινὰ καινῶς, it is not properly to ſay over the ſame thing again, when the obſervation, experiment, or other paſſage of an author, is either illuſtrated, or otherwiſe improved by the repetition, or elſe applied to ſome purpoſe differing from that, to which the author brought it: that being applicable to many a ſingle experiment or obſervation, which *Seneca* ſomewhere ſays, *Nulla res conſummata eſt dum incipit*; and, *Etiamſi omnia à veteribus inventa ſunt, hoc ſemper novum erit, uſus, & inventorum ab aliis ſcientia & diſpoſitio.* And thirdly, becauſe as the planets and other ſtars have (according to aſtrologers) in their great ſynods or conjunctions, much more powerful, and ſometimes other influences on the air and ſome other ſublunary parts of the world, than are aſcribed to one or two of them out of that aſpect; ſo divers particulars, which whilſt they lay ſingle and ſcattered among the writings of ſeveral authors were inconſiderable, when they come to be laid together in order to the ſame deſign, may oftentimes prove highly uſeful to phyſiology in their conjunction, wherein one of them may ſerve to prove one part or circumſtance of an important truth, and another to explicate another, and ſo all of them may conſpire together to verify that ſaying, *Et quæ non proſunt ſingula, multa juvant.* It may then, I hope, ſuffice to juſtify me on this occaſion, that not appealing to other writers as to judges, but as to witneſſes, nor employing what I have found already publiſhed by them barely as ornaments to imbelliſh my writings, and much leſs as oracles by their authority to demonſtrate my opinions, but as certificates to atteſt matters of fact, I may hope, that their teſtimonies will as well be illuſtrated by mine, as mine by their's, and that all of them may contribute to your better information.

Quæſtion. Natural. lib. 6. cap. 5.
Epiſtola 64.

AND if, *Pyrophilus*, you grant, that upon theſe conſiderations I have not done amiſs to apply to my purpoſe divers of thoſe things, which I found delivered pertinently to them by thoſe writers, which I chanced to caſt mine eyes on, I ſuppoſe you will not think I need to make you an apology, for my having made moſt uſe of the paſſages of thoſe writers, which I ſuppoſe will be moſt difficult to be met with (ſuch as are many books of navigation and other voyages) and eſpecially of French books not yet tranſlated into Engliſh or Latin. And I think I ſhall leſs need to make an excuſe for my having for the moſt part ſet down the paſſages I recited in the author's own words, that being one of the readieſt ways I know to ſatisfy the reader, and avoid injuring the writer. And indeed, I have met with abundance of quotations, wherein the tranſcriber doth ſo miſtake, or ſo miſrepreſent the cited author's meaning, ſometimes out of inadvertence, but ſometimes too I fear out of indulgence to his own hypotheſis, that if ever I ſhould be tempted to trouble the world with any of my thoughts, I would beſeech my readers, not to look upon any thing as my opinion or aſſertion, that is not delivered in the intire ſeries of my own words; leſt a tranſcriber ſhould make me deliver thoſe things reſolutely and dogmatically, which I deliver but heſitantly and conjecturally; and leſt I ſhould ſeem to ſet down thoſe things poſitively as proceſſes, for whoſe ſucceſs I undertake, which I record but by way of narrative.

FOR my ſo frequently mentioning what I have borrowed from other writers, or received from my friends, I expect to be excuſed by that of *Pliny*, *Benignum eſt (ut arbitror) & plenum ingenui pudoris, confiteri per quos profeceris.* Though I have ſeen divers modern writers, that ſo boldly uſurp the obſervations and experiments of others, that I might juſtly apply to them what the ſame *Pliny* annexes; *Scito enim, conferentem authores me deprehendiſſe à juratiſſimis & proximis veteres tranſcriptos ad verbum neque nominatos, &c.* If other writers ſhould not prove more equitable (for I will not ſay more thankful) than ſuch as theſe, they would quickly diſcourage thoſe, whoſe aims are not very noble and ſincere, from gratifying the public with inventions, whoſe praiſe and thanks would be uſurped by ſuch as will not name them. But perhaps they would be more juſt, if they reflected on what our author adds, *Obnoxii profectò animi & infelicis ingenii eſt, deprehendi in furto malle, quam mutuum reddere, cum præſertim ſors fiet ex uſura.*

AND now I have ſaid this concerning the paſſages I have borrowed from other authors, it will not be improper to add ſomething about thoſe I have declined to borrow. For you may poſſibly marvel, that in divers of the hiſtorical parts of my writings I have omitted ſuch teſtimonies either of *Pliny*, *Solinus*, *Ariſtotle*, *Theophraſtus*, *Ælian*, or, perchance, ſome of the antient phyſicians themſelves (who yet, as more converſant with things, are uſually more credible) as ſeem very pertinent to my diſcourſe, and fit to prove what I deſign. But when I ſhall come to entertain you about natural hiſtory, I doubt not but to ſatisfy you with the reaſons I ſhall offer you of this practice. In the mean time, I ſhall only tell you in ſhort, that though I have a juſt reſpect for thoſe great names I have mentioned; yet the ſenſe I have of the difficulties I have found to make and relate an obſervation accurately and faithfully enough for a naturaliſt to rely on; and the occaſions I have had of looking into divers matters of fact delivered in their writings, with a bold and impartial curioſity; have made me conclude ſo many of thoſe traditions to be either certainly falſe, or not certainly true, that except what they deliver upon their own particular knowledge, or with peculiar circumſtances, that may recommend them to my belief, I am very ſhy of building any thing of moment upon foundations, that I eſteem ſo unſure, and much leſs upon the ſuſpected paſſages, that *Wecker*, *Paracelſus*, *Porta*, &c. abound with. And therefore (though I well enough know, how much I impoveriſh my diſcourſe by this niceneſs) yet I do not think it fair to imploy that as an argument to convince you, that has not that operation upon me myſelf. And I rather take notice of my forbearing to make uſe of the hiſtorical traditions and chymical or magical ſecrets, that I meet in the above-mentioned authors, or any other makers of collections, unleſs the narrative be (as I was ſaying) expreſsly enough delivered upon the writer's perſonal knowledge, or that of ſome other credible witneſs; not only becauſe I would give you an account, why ſeveral of my writings are unfurniſhed with what moſt readers look on as the richeſt ornaments of other men's; but becauſe if this warineſs could be introduced, it would be the moſt effectual way of perſuading men to write thoſe kind of tracts I would recommend, phyſiological eſſays. For he, that will confine himſelf ſo ſtrictly, will ſcarce be often tempted, on phyſical ſubjects, to write either ſyſtems or volumes.

ANOTHER thing, *Pyrophilus*, I muſt needs advertiſe you of in reference to the enſuing diſcourſes; which is, that beſides thoſe deficiencies in point of ratiocination, which are due to my perſonal diſabilities, I have purpoſely let paſs ſome few (and but very few) inferences, which I diſcerned well enough not to be cogent, becauſe I was willing to acquaint you upon ſome particular occaſions with all the experiments then occurring to me, which I thought might contribute to the illuſtration of the ſubject in hand; though each of them apart did not irrefragably, nor indeed ſo much as ſtrongly infer the concluſion, in order to which they ſeemed to have been mentioned as premiſſes. And this practice I made the leſs ſcruple of, becauſe I was willing to exerciſe thereby your reaſoning faculty, and try how far you would diſcern the tendency of ſeveral things, all of them pertinent enough to the ſubject in hand, but not all of them concluding to the main deſign, in order whereunto they were alleged. And I ſuppoſed, that the things by me mentioned, though not concluſive, being yet experimental, the mention of them, which in a ſtrictly logical way of reaſoning muſt have been forborn, might well make you amends for the exerciſe, to which I intended they ſhould put your reaſon.

THERE remains yet one thing, *Pyrophilus*, of which I ſuppoſe you will expect I ſhould give you an account; and that is, why in the enſuing eſſays I have mentioned divers experiments, which I have not plainly and circumſtantially enough delivered. To ſatisfy you concerning which, I muſt repreſent to you, firſt, that though for your ſake I have oftentimes, contrary to my reaſon and genius, delivered things, to make them more clear, in ſuch a multitude of words, that I now ſeem even to myſelf to have in divers places been guilty of verboſity; yet in ſome other paſſages, treating of things, which uſe had rendered very familiar to me, I may have, to ſhun prolixity, unawares ſlipt into the contrary extreme. Secondly, there are ſome mechanical experiments, wherein I have purpoſely omitted ſome manual circumſtances, becauſe I was unwilling to prejudice ſome ingenious tradeſmen, who make either a livelihood, or at leaſt a gain, by the ſale of the productions of ſuch experiments. And I made the leſs ſcruple to conceal ſuch mechanical circumſtances (if I may ſo call them) becauſe they were not neceſſary to the phiſiological knowledge of the experiments; in naming of which, my intention was to teach you rather philoſophy than trades. Thirdly, I mention ſome things but darkly, either becauſe I received them upon condition of ſecrecy, or becauſe ſome ingenious perſons, that communicated them to me, or others to whom I imparted them, do yet make, and need to make, a pecuniary advantage of them. Fourthly, and ſome things, that, either having been the fruits of my own labours, or obtained in exchange of ſuch, are freely at my own diſpoſal, I have not yet thought fit ſo plainly to reveal, not out of any envious deſign of having them buried with me, but that I may be always provided with ſome rarity to barter with thoſe ſecretiſts, that will not part with one ſecret but in exchange for another, and think nothing worth their deſiring, that is known already to above one or two perſons. And I think it very lawful to reſerve always ſome concealed experiments

riments by me, wherewith to obtain the ſecrets of others, which being thereby gained, the other (as being no longer neceſſary to the former end) may freely be communicated.

AND think not, *Pyrophilus*, that the bare mention of an experiment as having been performed, though the way of making it be concealed, is of no uſe, if the relator of the experiment be a perſon, that may ſafely be credited: for it is ſomething, to be aſſured, that ſuch and ſuch things have been really performed, and conſequently are poſſible to be done, though we be not particularly acquainted with the means of performing them. And he tells you ſomething, that tells you upon his own knowledge, that in ſuch or ſuch bodies, or ways of operating on them, conſiderable things of ſuch or ſuch a nature are to be met with. And for my part, when I go a hawking or ſetting, I think myſelf beholden to him, that aſſures me, that in ſuch a field there is a covey of partridges, though he does no more towards the giving me them. And it is obvious, how much *Europe* is beholden to *Columbus* for the detection of many countries in *America*, which were not diſcovered by him, nor perhaps till long after his death, becauſe he firſt informed us knowingly, that there were unknown regions beyond that vaſt ocean, which ſevers the old world from the new. But I begin to digreſs, and therefore ſhall proceed to tell you, that I am the leſs troubled at my omiſſion of the circumſtantial parts of ſome experiments, becauſe I think it will be much for your advantage to try them over again yourſelf. And as I have taken care by the truth of the experiments I have delivered, to ſecure your ſucceſs, in caſe you try them aright; ſo I cannot be very ſorry, that you ſhould in ſome particulars have a kind of neceſſity laid on you to exerciſe your own induſtry, and thereby increaſe your experience.

BUT beſides all that has been ſaid, *Pyrophilus*, I muſt freely confeſs to you, that there is one thing particularly relating to yourſelf, which has made me refrain from delivering, in the enſuing eſſays, ſome of the chief chymical proceſſes, wherewith they might have been enriched. For not yet knowing with what ſeriouſneſs you will addict yourſelf to promote experimental philoſophy, nor what uſe you will make of what has been unveiledly communicated to you, I was ſomewhat unwilling, that ſome things, which had coſt me a great deal of pains, ſhould yet fall into any man's hands, that ſcorns to purchaſe knowledge with ſome pains; and I was deſirous, in caſe you ſhall prove as induſtrious as I hope you will, to have ſomething by me to encourage and cheriſh your induſtry, which may be more ſuitable to your improved knowledge. For I muſt confeſs to you, that in reference to the chymical proceſſes extant in the following diſcourſes, I look upon moſt of them but as trifles, not only in compariſon of thoſe things, which a knowing chymiſt might have delivered on the ſame ſubjects, but even in regard of divers proceſſes (not impertinent to thoſe diſcourſes) wherewith I myſelf (as little as I am a pretender in theſe matters) am not unacquainted: and perhaps I would have given to the following treatiſes the title of trifles, inſtead of that of eſſays, if I had not been afraid of diſcouraging you thereby, and if the chymical part of them had been the chief thing, wherewith I intended to acquaint you in them. But if the reception you give to what we have already written, prove ſuch as may encourage us to proceed, we may perhaps, if God be pleaſed to vouchſafe us life and opportunity, be invited to impart to you thoſe more conſiderable chymical experiments, which either the communication of our friends, or our own labours, have preſented us. For it will be much in the power of the entertainment, which theſe papers ſhall meet with, to make them either the beginning of our labours of this nature, or the end. And in the mean time, I think I may venture to tell you, that, as inconſiderable as I have confeſſed divers of the chymical proceſſes mentioned in the eſſays to be, yet if ever you take the pains (as I hope you will) to write experimental eſſays, and confine yourſelf to take as little upon truſt as I have done, you will perhaps be ready to believe, that ſometimes a ſhort eſſay of this nature, not to ſay ſome one ſingle experiment, may have coſt me more than a whole treatiſe written on ſuch a ſubject, whereon to be able without diſcredit to write books, it is almoſt ſufficient to have read many. And give me leave to add, that as in ſuch kind of compoſures, oftentimes the enabling himſelf to give a conſiderable advertiſement, or even hint, may coſt the writer more than the making of ſeveral experiments; ſo it may be alſo more beneficial to the reader than the knowledge of them. For we muſt not always meaſure the conſiderableneſs of things by their moſt obvious and immediate uſefulneſs, but by their fitneſs to make or contribute to the diſcovery of things highly uſeful. As, if it be true, what is reported by good authors of the hazel wand, or *virgula divinatoria*, though the hazel tree be much leſs conſiderable in reference to its fruit, or immediate productions, than a peach-tree, an orange-tree, or even an apple-tree; yet may it be made much more valuable than any of them, becauſe, whereas they only preſent us with fruits, this may aſſiſt us to diſcover in latent mines ineſtimable treaſures.

I had almoſt forgot to advertiſe you, *Pyrophilus*, that whereas I have not been ſo ſollicitous as moſt writers are wont to be, to ſwell the enſuing eſſays with the enumeration of the various opinions and arguments of authors about the ſubject I treat of, or to adorn them with acute ſentences, fine expreſſions, or other embelliſhments borrowed from eminent writers; it has not been, becauſe I utterly diſlike the making uſe of thoſe paſſages in claſſic or other authors, that may either give (among the admirers of thoſe writers) ſome authority to our thoughts, or very handſomely and emphatically expreſs them. For I remember, I have known it reprehended by learned men in *Epicurus*, that though he wrote very much himſelf, he would not vouchſafe in his writings to quote thoſe of other men. And that I have not refrained from making uſe, now and then, of thoſe philological ornaments of diſcourſe, when they readily occurred to me, and appeared neither impertinent nor prolix, may, I hope, ſuffice to keep me from being ſuſpected of the vanity of thinking myſelf above other men's aſſiſtance. But the reaſons of my ſo much declining to make uſe of other men's authority, or expreſſions, were chiefly theſe: firſt, That the weakneſs of my eyes has this long time kept me from reading almoſt any books, ſave the Scripture, with ſome critical expoſitions of it, and, here and there, ſome portions of the writings of thoſe that pretend to teach their readers experimental matters: and the unfaithfulneſs of my memory, as to things of no great moment, has made me forget almoſt all the little philological and florid learning I was formerly acquainted with. And really, *Pyrophilus*, as for the books, that treat of natural philoſophy, I am ſo ſenſible of the ſmallneſs of the advantage, which my diſabilities have ſuffered me to make of them, that inſtead of being ambitious to appear a great reader, I could be very well content to be thought to have ſcarce looked upon any other book than that of nature. And in the next place, *Pyrophilus*, though I ignore not, that by this plain and unadorned way of writing, I unkindly deny my eſſays many imbelliſhments, which I might give them, and which perhaps you will think they do abundantly need; yet my frequent diſtempers, journeys, and other avocations, not allowing me ſo much time as I deſired, to entertain you on philoſophical ſubjects, I thought it more requiſite to ſpend thoſe confined hours in acquainting you with my own thoughts, ſuch as they are, than with thoſe of other men; unleſs (as I formerly intimated) I can ſome way or other more than barely recite what I recite of theirs. And you will eaſily pardon me the injury, which for your ſake I do my own reputation by this naked way of writing, if you, as well as I, think thoſe the profitableſt writers, or, at leaſt, the kindeſt to their peruſers, who take not ſo much care to appear knowing men themſelves, as to make their readers ſuch.

TWO ESSAYS,

Concerning the

Unſucceſsfulneſs of EXPERIMENTS,

Containing divers Admonitions and Obſervations (chiefly Chymical) touching that SUBJECT.

Advertiſement about the two following Eſſays.

THE author of theſe diſcourſes had enlarged them in this ſecond edition, with divers obſervations and experiments, but that he has made uſe of them already in other papers belonging to his Sceptical or doubting Naturaliſt.

The Firſt ESSAY,

Of the Unſucceſsfulneſs of EXPERIMENTS.

I AM very ſorry, *Pyrophilus*, that to the many (elſewhere enumerated) difficulties which you may meet with, and muſt therefore ſurmount, in the ſerious and effectual proſecution of experimental philoſophy, I muſt add one diſcouragement more, which will perhaps as much ſurprize you as diſhearten you; and it is, that beſides that you will find (as we elſewhere mention) many of the experiments publiſhed by authors, or related to you by the perſons you converſe with, falſe or unſucceſsful (beſides this, I ſay) you will meet with ſeveral obſervations and experiments, which, though communicated for true by candid authors or undiſtruſted eye witneſſes, or perhaps recommended to you by your own experience, may upon further trial, diſappoint

appoint your expectation, either not at all ſucceeding conſtantly, or at leaſt varying much from what you expected.

THIS advertiſement may ſeem of ſo diſcouraging a nature, that I ſhould much ſcruple the giving it you, but that I ſuppoſe the trouble at that unſucceſsfulneſs, which you may meet with in experiments, may be ſomewhat leſſened by your being forewarned of ſuch contingencies: and that if you ſhould have the luck to make an experiment once, without being able to perform the ſame thing again, you might be apt to look upon ſuch diſappointments as the effects of an unfriendlineſs in nature or fortune to your particular attempts, as proceed but from a ſecret contingency incident to ſome experiments, by whomſoever they be tried.

BUT becauſe, *Pyrophilus*, the advertiſement, which I am about to give you, may ſeem, as paradoxical, as diſcouraging: it will be but reaſonable, that I preſent you with ſome inſtances of the requiſiteneſs of it: which I ſhall the more willingly do, becauſe thereby I may not only evince the truth of it, but ſomewhat leſſen the deſpondency it is apt to produce, by letting you ſee, that though ſome of your experiments ſhould not always prove conſtant, you have divers partners in that infelicity, who have not been diſcouraged by it.

TO make nice and curious diſtinctions of the ſeveral grounds and occaſions of the unſucceſsfulneſs of experiments, would, perhaps, prove a work of greater difficulty than uſe; and therefore I ſhall content myſelf groſly to diſtinguiſh the cauſes of that unſucceſsfulneſs, into the particular or miſtaken properties of the materials imployed about them, and ſome ſuch error committed in the handling of theſe materials, as though it hinder the ſucceſs of the experiment, is not eaſy to be diſcerned. Which therefore I mention, that I may diſtinguiſh theſe kind of errors, that I am now to conſider, from thoſe more obvious ones, which proceeding barely from the unſkilfulneſs of the tryers of the experiments, may be eaſily enough diſcerned, and either rectified or avoided by a knowing artiſt, or a perſon well verſed and expert in making thoſe particular experiments, which through that unſkilfulneſs may have miſcarried.

THE materials to be employed about the experiments we are conſidering may alſo admit of ſeveral diſtinctions; as into natural and factitious, ſincere and adulterate, ſimple and compound, *&c.* But we ſhall likewiſe purpoſely forbear the inſiſting on any of theſe, and content ourſelves to caſt what we have to ſay on this part of our theme, into a few and comprehenſive obſervations.

AND in this firſt place we will obſerve, that divers experiments ſucceeded not, becauſe they were at one time tried with genuine materials, and at another time with ſophiſticated ones: and in this caſe it may be all one, as to the event of the experiment, whether the materials, wherewith it was ſucceſsfully tried, were ſophiſticated or not, if thoſe made uſe of in the latter trial were of differing qualities from thoſe employed in the former; becauſe it may very well happen, that ſophiſticated bodies (as we may have occaſion to ſhew hereafter) by the addition of thoſe things, or by that deceitful way of preparation, whereby they have been ſophiſticated, may acquire an aptitude to produce ſuch effects, as, had they not been adulterated, they would not have been fit to do. Now it is ſcarcely imaginable to him, that has not been very converſant with the drugs and ſimples ſold in ſhops, how generally they are adulterated by the fraudulent avarice of the ſellers, eſpecially if they be ſuch, whoſe preciousneſs may make their ſophiſtication very beneficial to them, that practiſe it. It has been lately much complained of by ſome of the cultivators of clover-graſs, that of a great quantity of the ſeed not any graſs ſprung up; which not being imputable to the ſoil, nor the ſower, proceeds, as ſome analogical obſervations make me ſuppoſe, from the effeteneſs (if I may ſo ſpeak) of the ſuperannuated ſeed ſometimes ſold in the ſhops. And upon this ſubject I cannot conceal from you what was lately affirmed to me by one of the eminenteſt and ſobereſt chymiſts of *Amſterdam*, who was alſo an Indian merchant, who aſſured me, that the moſt of the cinnamon and cloves, that is brought into theſe weſtern regions, is defrauded in the *Indies* of much of the fineſt and ſubtileſt aromatical parts, before it be ſent into *Europe*. And to give a more familiar inſtance to our preſent purpoſe, you may be pleaſed to remember, *Pyrophilus*, that in one of the firſt of theſe eſſays, we have made mention to you of great ſtore of living creatures, which we had obſerved in vinegar; of the truth of which obſervation we can produce divers learned and ſevere witneſſes, who were not to be convinced of it, till we had fully ſatisfied them by ocular demonſtration: and yet, *Pyrophilus*, there are divers parcels of excellent vinegar, wherein you may in vain ſeek for theſe living creatures: and we are now diſtilling ſome of that liquor (which if we did not think to be of the ſtrongeſt and beſt ſort, we ſhould ſcarce think worth the being diſtilled for ſpirit) wherein nevertheleſs we can neither by candle-light nor by day-light diſcern any of thoſe little creatures, of which we have often ſeen ſwarms in other vinegars. Of ſuch fraudulent tricks as thoſe lately mentioned, I could eaſily give you divers inſtances, if I were not afraid of teaching fallacies by diſcovering them. But ſome are ſo notorious, or otherwiſe of ſuch a nature, as that it may be more uſeful than dangerous to mention them.

It is commonly known, that ſublimate is wont to be ſophiſticated with arſenick: and how differing the effects of ſuch ſublimate may be from thoſe of that, which is faithfully prepared, not only upon metals, but (when mercurius dulcis and other preparations are made of it) upon human bodies, they, and ſcarce any but they, who are acquainted with the noxious qualities of arſenick, both to metals and men, can readily imagine. And indeed as for chymical preparations, *Helmont* [*] was not much in the wrong, when he affirmed, there were ſcarce any, vulgarly ſold in ſhops, to be relied on as faithfully prepared. And for my part, I have ſo often met with chymical preparations, which I have found unſincere, that I dare ſcarce truſt any, either in the adminiſtration of phyſic, or ſo much as in the trial of conſiderable experiments, which either my own furnaces do not afford me, or wherewith I am not ſupplied by ſome perſon, of whoſe ſkill and faithfulneſs I have a good opinion. The other day, having occaſion to uſe ſome ſpirit of ſalt, whereof I was not then provided, I ſent for ſome to a chymiſt, who making it himſelf, was the likelier to afford that, which was well made: but though I gave him his own rate for it, at the firſt rectification even in a retort, a ſingle pound afforded us no leſs than ſix ounces of phlegm; and afterwards being further rectified in a high body and gentle heat, the remaining ſpirit parted with a ſcarce credible quantity of the like nauſeous liquor, and after all theſe ſequeſtrations of phlegm, was not pure enough, to perform what we expected from from it. Of which complaining to an excellent chymiſt of my acquaintance, he ſent for ſpirit of ſalt to a very eminent diſtiller of it, who gets much by his profeſſion, and paſſeth for a very honeſt man: but this ſpirit, beſides its weakneſs, diſcovered itſelf to be ſophiſticated with either ſpirit of nitre, or aqua fortis, which betrayed itſelf by its peculiar and odious ſmell; whereas ſpirit of ſalt ſkilfully and ſincerely drawn is commonly of a greeniſh colour, bordering upon yellow, and hath uſually a peculiar, and ſometimes (as I can exemplify to you in ſome of mine) a not unpleaſing ſmell. And let me on this occaſion advertiſe you, *Pyrophilus*, that in divers caſes it is not enough to ſeparate the aqueous parts by dephlegmation, as many chymiſts content themſelves to do; but ſome liquors contain alſo an unſuſpected quantity of ſmall corpuſcles of ſomewhat an earthy nature, which being aſſociated with the ſaline ones, do clog or blunt them, and thereby weaken their activity: and therefore ſuch liquors to be well depurated require the being diſtilled off, and that with a ſlow fire, that the dry fæces may be left behind in the bottom of the glaſs. To ſatisfy ſome perſons, that this obſervation is not groundleſs, we have ſometimes taken of the better ſort of ſpirit of ſalt, and having carefully dephlegmed it, removed it into lower glaſſes (that the leſs heat will ſuffice to make the liquor aſcend) and having gently abſtracted the whole ſpirit, there remained in the bottom and the neck of the retort, whence it was diſtilled, ſo great a quantity of a certain dry and ſtiptical ſubſtance, for the moſt part of a yellowiſh colour, that it ſeemed ſtrange to the beholders, that ſo clear a ſpirit ſhould conceal ſo much of it: and we ourſelves ſhould have wondered at it too, had we not remembered, that in what the chymiſts are wont to call the oil or rectified butter of antimony made with ſublimate, the liquor, though diſtilled and very limpid, almoſt like fair water, conſiſts in great part of the very body of the antimony: which we would here manifeſt, but that we elſewhere do it; and therefore chuſe rather in this place to take notice, that the ſpirit of ſalt after this ſecond depuration was ſo changed, that it ſeemed to be a much nobler, and almoſt another liquor than it was before.

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[*] *Accipe pulverem* Johannis de Vigo *propria manu paratam, nam alioquin admiſtus minio eſt adulteratus, prout qualecunque medicamen chymicum quod venale exſtat fraude plenum eſt.* Helmon. de Febr. c. 14.

Sunt nempe olea eſſentialia venalia, quæque magno ære penduntur, adulterata omnia atque ſingula, &c. Idem de Feb. c. 15.

But to return to our ſophiſticated ſpirit; what differing effects would be produced by true ſpirit of ſalt, and that which is mixt with the ſpirit of nitre, he that knows the great diſparity in the operations of thoſe two liquors, whereof (to mention now no other inſtances) the former will precipitate ſilver, when the latter has diſſolved it, may eaſily inform you. Which inſtances I mention not as the conſiderableſt, which may be produced on this ſubject, but as the freſheſt in my memory.

In the next place, *Pyrophilus*, I obſerve, that even when the materials imployed about experiments are no way ſophiſticated, but genuine, and ſuch as nature has produced them, or art ought to prepare them; even then, I ſay, there may be a very conſiderable diſparity betwixt concretions of the ſame kind and name, and which paſs without ſuſpicion for bodies of perfectly the ſame nature.

This may, to you, *Pyrophilus*, ſeem a great paradox; but perhaps upon examination it will appear a great truth: which becauſe I am, perchance, the firſt, that has ſolemnly aſſerted, I hope I ſhall obtain your pardon, if I inſiſt ſomewhat the longer upon the making it out. For though antimony (and the like is to be underſtood of quickſilver, gold, copper, tin, *&c.*) is wont by almoſt all men without heſitancy to be looked upon as being all of it of the ſame nature as well as denomination; yet he, that will take the liberty to ſuſpect, that they may be deceived in that opinion, and then heedfully obſerve the differing progreſs and event of experiments, may very well diſcern, that there is as well a difference in minerals of the ſame kind, as there is in vegetables and animals of the ſame ſpecies. And as the white roſe, the red roſe, and the damaſk roſe differ much from one another, though all three be roſes; and as the ſour and ſweet orange are very differing betwixt themſelves, and yet both of them from the *China* orange, though all be oranges; and as the hound, the grey-hound, the ſpaniel, the tumbler, the maſtiff, and the water dog, *&c.* are very diverſly qualified, though all of them be dogs: ſo neither are all the parcels of antimony to be met with in mines or ſhops of altogether the ſame qualities, though all of them be antimonial concretes. There is indeed this difference betwixt the variety to be obſerved in vegetables and animals, and that which is to be found in minerals, that the former is wont to be more obvious to the eye, and betray itſelf by ſome difference to be obſerved, either in the ſize of creatures of the ſame kind, or in ſome peculiar ſhape or colour, by which it is eaſy for nature conſpicuouſly to diſcriminate bodies, that conſiſt of many diſcernably diſtinct parts; whereas minerals appearing to the eye either to be perfectly ſimilar, as metals, or at leaſt to conſiſt but of two or three diſtinct ingredients, as cinnabar, and ſome other mineral concretions, the diverſity to be found betwixt minerals of the ſame denomination is hardly to be diſcerned, before experience have diſcovered it.

And on this ſubject I conſider, that the womb (if I may ſo ſpeak) of a mineral body is not always like that of an animal, a place by a competent and peculiar involving fence ſecured from the intruſion of all bodies not of kin to that included in it: but a mineral being generated in the bowels of the earth, its womb is oftentimes acceſſible and open to other mineral juices or ſteams, that paſs that way, though of never ſo differing natures from that of the more copious mineral. Inſomuch that not only I have had the opportunity to obſerve (not without ſome wonder) minerals of differing kinds, as marchaſites and metals, marchaſites and ſtones, (I mean ſtones properly ſo called) ſalt and ſulphur, and the like, blended in the ſame ſmall lump of matter; but I have ſometimes found, in a great maſs of one ſort of mineral, ſmall parcels of a mineral of a quite differing kind perfectly incloſed in the ſubſtance of the other. But to reſume what we were ſaying before, theſe intruding bodies (if I may ſo ſpeak) being coagulated, and perhaps ripened together with the former by length of time, are not eaſily either ſeparable, or ſo much as diſtinguiſhable at their firſt digging out of the ground, and much leſs after their colliquation. For the ignorant or heedleſs mine man aiming only at the obtaining a quantity of ſuch a metal, or other mineral, as may be vendible under ſuch a determinate name, has neither the deſign, nor perhaps the ſkill, to make nice ſeparations of the heterogeneous bodies to be met with in his ore, but melts ſo much of them as he can promiſcuouſly together; and then ſells them, not only to the merchant, but the chymiſt, for that metal or mineral, whoſe outward form and properties (as colour, conſiſtence, weight, ſound, *&c.*) it has: though that metal, under whoſe name it paſſes, be indeed but the predominant ingredient of the lump, wherein divers other minerals may in ſmall quantities lie concealed, and yet upon occaſion be diſcovered by exquiſite ſeparations, or diſcover themſelves by unexpected operations, when they meet with bodies fit to act on them, or diſpoſed to receive impreſſions from them.

I was lately viſited by an ingenuous goldſmith of my acquaintance, who complained to me, that being wont to buy parcels of gold brought in ſmall pieces, and as it were ſandy corpuſcles, from *Guinea*, or ſome country of that coaſt, though he found it upon all trials very right gold, yet was it ſo very pale, that few but expert goldſmiths would meddle with it, as fearing it had been ſome ſophiſticated metal; adding, that this exceeding paleneſs of it ſometimes reduced him to melt it with very high coloured gold, or to heighten its tincture with that of copper, to bring it to the colour of ordinary gold.

The probability of this may be proved by what is related by Monſieur *Flacourt*, governor of the French plantation in *Madagaſcar*, who, in his newly publiſhed hiſtory of that iſland, ſpeaking of the metals of it, ſays, *Il y a bien*, &c. that is, there is certainly gold among the inhabitants of *Madagaſcar*, which has not been

Hiſtoire de Madagaſcar, c. 37.

brought hither by foreign ſhips: for it is not poſſible, that ſuch ſhips ſhould have left them ſo much of that metal as they have; and beſides, it is of a differing nature from that of Europe, which they call in this country *Voulamene Voutroüa*. He adds, that this gold, which they call gold of *Malacaſſe*, is pale, and is not worth above ten crowns (or about fifty ſhillings) an ounce; alſo, that the Negroes affirm, that there are many mines of it in the country, where it was formerly digged; that there is three ſorts of it differing in fineneſs from each other, and diſcriminated by the natives by three peculiar names. But that which he adds as moſt conſiderable, is, *that Malacaſſean gold is of ſo very eaſy a fuſion, that it is almoſt as eaſily melted as lead*; whereas we here find the gold we deal with to require conſiderably ſtrong fires, and are wont to caſt in borax to facilitate the fuſion.

HAVING, upon occaſion, had the curioſity not long ſince to viſit ſome mines of lead, and other metals, I find that there is a great difference, and diſcernible even to the eye, betwixt the ſeveral ores; for inſtance, of lead, ſome of which I can ſhew you ſo like ſteel, and ſo unlike common lead-ore, that the workmen upon that account are pleaſed to call it ſteel-ore, which being of more difficult fuſion than ordinary, they are wont to mix it with other ore, which they call firm-ore, to facilitate the melting of it. And I likewiſe took notice of an ore, which for its aptneſs to vitrify, and ſerve the potters to glaze their earthen veſſels, the miners call pottern-ore, and ſell it (at leaſt where I ſaw it digged up) dearer than other ore, from which it differs both viſibly enough, and as the workmen affirm, in divers other (and thoſe leſs obvious) qualities; and yet all theſe ores, after fuſion, do paſs indiſcriminately under the name and notion of lead. In which therefore it is no wonder, that ſeverer inquiries find a great deal of diſparity. I remember I did not long ſince cauſe ſome lead-ore to be tried, which being the moſt promiſing that ever I ſaw, made me ſuppoſe it might contain ſome conſiderable quantity of ſilver: but though it proved ſo rich in lead, as to yield after the rate of ſeventy pound to the hundred, yet one of the moſt expert artiſts in Europe could not extract one grain of ſilver out of it; whereas the lead of very many mines, being ſkilfully examined, will leave behind it, upon the teſt, a proportion of pure ſilver. And though this quantity of ſilver be not conſiderable enough to make ſuch mines as yield it paſs for ſilver mines, (or, as we are wont to call them, mines-royal) becauſe the ſilver will not quit the coſt of extracting it; yet ſuch mines, though they paſs but for lead mines with the metaliſt, may appear to be mixt mines to the naturaliſt, who may meet with divers experiments, wherein the little ſilver that is in them, may make their lead operate differently from that of thoſe ores, which are wholly deſtitute of ſilver.

AND as this diſparity is diſcernible in lead-ores, ſo it may well be ſuppoſed, that the like would be diſcovered in the ores of other metals, if they were but purpoſely and ſkilfully examined. On which ſubject I remember, that a very experienced perſon in theſe affairs, and otherwiſe very candid and ſober, was lately very deſirous I ſhould procure him ſome tin-ore, alleging, that he had met with a ſort of it, which, after a long digeſtion in lixiviate liquors, afforded him a very conſiderable proportion of the richer metals; inſomuch that having a large quantity of that ore, and finding the experiment on it to ſucceed conſtantly, he promiſed to himſelf a vaſt income by it: but when that ſtock of ore was ſpent, the next that he procured, though with great carefulneſs managed as the former, would by no means be brought to afford either ſo conſiderable a benefit, or ſo much as any at all. Which brings into my mind, that having once bought a parcel of block-tin (as the tradeſmen call that, which is of the moſt pure or unmixt, and as yet unwrought) I was deſirous to try, if I could not make a menſtruum to diſſolve it in ſuch manner as aqua fortis diſſolves ſilver, and aqua regis gold; becauſe chymiſts are wont to complain, that though they have a menſtruum or two that will diſſolve crude tin, yet they want one, that will keep it diſſolved, and will not, which aqua fortis will, let it fall into a calx. Having therefore (by a way that I elſewhere teach) prepared ſuch a liquor as was deſired, I evaporated a ſolution of the forementioned tin, and ſetting it to ſhoot, found ſomewhat, to my wonder, that the cryſtals it afforded were not at all like any kind of vitriol, but broad, flat, and exceeding thin, juſt like thoſe of ſilver. Whereupon for further trial, having examined this ſalt by the tongue, we found not, that it had any ſuch taſte as ſkilfully made calx of tin in ſpirit of vinegar, (wherein it is not every calx of *Jupiter* that is ſoluble) which (the laſt time we tried) ſeemed to us to have, as it were, a chalybeate taſte, but ſuch an exceſſive bitterneſs as may be met with in the cryſtals of ſilver made with aqua fortis. Finding alſo this further reſemblance betwixt the ſalts of theſe two metals, that they did both of them preſently dye upon the nails and ſkin a blackneſs, that could not in a ſhort time be waſhed off; we ſhould have ſuſpected, that the menſtruum had exalted the metal diſſolved in it to a greater cognation to ſilver; but having afterwards proſecuted the ſame trial with the ſame menſtruum, and another parcel of block-tin, (the former being caſually loſt) this metal, though bought very ſoon after the other, and, as I remember, at the ſame place, made us conclude, that the event of our trials proceeded from our having lighted upon a lump of tin, that was of a peculiar nature.

I REMEMBER alſo, that a while ſince a learned and inquiſitive friend of mine found in his land a parcel of ore, part of which he ſhewed me, and ſome of which I can ſhew you, but have not yet made trial of it; which ſeemed to me, among others that looked upon it, to be copper ore, and which did indeed, after fuſion, yield very good copper; but the perſons, to whom he committed the examination of the mine, being very inquiſitive, and extraordinary ſkilful, they did (as one of themſelves immediately after confeſſed to me) find in that ore, beſides the copper, no inconſiderable quantity of ſilver; and in that ſilver (having diſſolved it in aqua fortis) a conſiderable proportion of gold.

BUT to detain you no longer on this ſubject, give me only leave to ſtrenghen the paradox I have propoſed, by the authority of that great and candid chymiſt *Baſilius Valentinus*, who ſpeaking of antimony, after he hath told us, that there are ſeveral kinds of it, and eſpecially two; the one more mercurial, and of a golden property, witneſſed by the ſhining ſtreaks or beams it abounds with; the other more full of ſulphur, but deſtitute of the golden nature that inriches the former; adds, that there is ſuch a different goodneſs betwixt the ſeveral ſorts of antimony, as there is betwixt the ſeveral ſorts of fleſh or fiſh, which, though agreeing in name, and, if you pleaſe, in nature, do exceedingly differ in point of goodneſs. Which brings into my mind the great difference which I have found, even viſible to the eye, betwixt the ſeveral ſorts of antimony; and makes me alſo remember, that the other day being by an excellent chymiſt ſhewed a parcel of antimony as a rarity, upon the ſcore of the various coloured ſulphur, wherewith it was conſpicuouſly inriched, the poſſeſſor of it ſoon after imployed it to make butter of antimony: but though he were very expert in that kind of diſtillation, yet inſtead of the liquor he expected, upon the approach of a gentle fire, he found the neck and body of his retort lined with an antimonial cinnabar, (or at leaſt a red ſubſtance, by him concluded to be ſulphur;) at which being ſurprized, he was pleaſed to withdraw his fire, till he had acquainted me with this accident, and in the yet unbroken retort ſhewed me the cinnabar, which is not wont (as you know) to ariſe till after the butter of antimony is come over, and the remaining matter be urged with a vehement fire. And it is perhaps to the undiſcerned difference of antimonies, that we may ſometimes aſcribe that contingency, which we have divers times had occaſion to take notice of in the making of antimonial cinnabar: for though in our furnaces it hath been very ſucceſsfully made, yet not only we have afterwards failed of making it, but we have ſeen much more expert chymiſts, and who, becauſe of the high value they do (not undeſervedly) place upon that medicine, imploy themſelves oftener than we in making it, divers times unſucceſsfully attempt the preparing it. And it may be perhaps alſo from ſome diverſity either in antimonies or irons, that eminent chymiſts have (as we have obſerved) often failed in their endeavours to make the ſtarry regulus of *Mars* and antimony. Inſomuch that divers artiſts fondly believe and teach (what our experience will not permit us to allow) that there is a certain reſpect to times and conſtellations requiſite to the producing of this (I confeſs admirable) body. Upon which ſubject I muſt not omit to tell you, that a while ſince an induſtrious acquaintance of ours was working on an antimony, which unawares to him was, as we then ſuppoſed, of ſo peculiar a nature, that making a regulus of it alone without iron) the common way, (for his manner of operation I inquired of him) he found, to his wonder, and ſhewed me his regulus adorned with a more conſpicuous ſtar, than I have ſeen in ſeveral ſtellate reguluſes of both antimony and *Mars*. Yet I dare not be too confident that this depended only upon the peculiar nature of that antimony, becauſe ſince that, my own laboratory has afforded me divers ſuch parcels of regulus without *Mars*, (ſome of which I have yet by me very fairly ſtellated) which though made of antimony, that ſeemed (by its various colours) to be more rich than ordinary in ſulphur; yet in regard the antimony did not conſtantly afford a ſtary regulus, though by the ſame perſon ordered as near as could be after the ſame manner, it did not ſo clearly appear to me, whether the different event of the ſeveral trials proceeded from the peculiar nature of this or that parcel of antimony, or from ſome odd and ſcarce diſcoverable circumſtance in the management of the operation. But in either caſe, the mention of theſe uncertain events will properly enough belong to our preſent diſcourſe.

As in antimony, ſo (as I intimated above) in divers other minerals a conſiderable diverſity may be obſerved: and I remember I was lately preſented with a piece of a mineral, which to me, as well as to the reſt who looked on it, ſeemed to be an ordinary and worthleſs marchaſite; and yet a Dutch merchant (a ſkilful mineraliſt) who was the poſſeſſor of it, was very induſtrious to procure a greater quantity thereof, having in ſome of it, on which he had made trials, found a rich proportion of pure gold. And the ſame gentleman, whoſe copper ore I formerly mentioned, digging for more of that ore, found lately a quantity of red earth, which by knowing mineraliſts was gueſſed to be but bolus, and indeed looked very like it; but being melted with *regulus Martis ſtellatus* by a ſkilful trier of metals, it divers times richly recompenſed the examiner's curioſity, by affording him many grains of fine gold: and though I doubt, whether this gold proceeded from the bolus, or the regulus melted with it, yet however it may ſerve for an inſtance to ſhew, that ſome mineral bodies, which paſs without diſpute for minerals of ſuch and ſuch a preciſe nature, may have lurking in them minerals of a quite other nature, which may manifeſt themſelves in ſome particular experiments (wherein they meet with proportionate agents or patients) though not in others.

THAT the talc, which is wont to be employed about coſmeticks, is of very difficult calcination, is ſo known a thing to thoſe that have tried to calcine it, that I have met with good chymiſts, that have looked on all the calces of talcs but as impoſtures, Nor indeed have we calcined Venetian talc without ſome length of time, and much violence of heat. But among many ſorts of talc we have here in *England*, there is none, which a moderate fire will in leſs than an hour reduce into a ſnow-white calx, of which I had lately a parcel by me; and ſome days ſince I met with another ſort of Engliſh talc which I could ſuddenly calcine even with the flame of a candle. And my experienced friend Dr. *K.* aſſures me, that out of a German talc he met with, he did by digeſting it in a ſtrong ſolution of alcalizate ſalts ſeparate pretty ſtore of good gold, and might have made it a very gainful experiment, if all the talc growing in the ſame place had been of the ſame richneſs. The like almoſt has been affirmed to me by a gentleman of eminency, who told me, that from a certain talc he had out of *Norway*, he had once drawn a pretty quantity of very good gold: and it ſeems indeed, that though ſome have been pleaſed to laugh at all attempts of ſequeſtering any thing from any kind of talc; yet ſome parcels of that mineral afford good ſtore of a tincture, which may, for aught I know, be of a golden nature. For I remember I have met with a kind of darkiſh coloured talc (whereof I can yet ſhew you a piece) which when I caſt but into aqua regis, the menſtruum manifeſtly worked upon it, and diſſolved its coloured parts in ſuch plenty, that the filtrated ſolution paſſed without ſuſpicion among divers knowing naturaliſts, to whom I ſhewed it, for a fair ſolution of gold. *Paracelſus* himſelf reckons four kinds of talc, red, white, black, and of that colour, which his interpreter tranſlates luteous: and perhaps each of theſe colours compriſes ſeveral kinds of that mineral. And therefore that mineraliſt did not amiſs, when he added in the ſame diſcourſe, after he had mentioned great variety of marchaſites, ſtones, and other minerals, *Sed & hoc verum eſt, in terra multa adhuc condi, quæ mihi incognita ſunt, ſed eadem nec alii norunt. Certum ſiquidem eſt, progreſſu temporis tot tamque varia à Deo adhuc proditum iri, de quibus nemo noſtrûm nedum unquam ſomniavit.*

Paracel. *de Mineral. Tract.* 1.

Paracel. *ibidem.*

IT is vulgarly known, that there is a great difference between vitriols, that are reputed to be meerly of the ſame metal. And not to mention thoſe vitriols, that I have either made or ſeen, of leſs uſual colours; nor to take notice of the veins, ſlate, and even looſe earth, impregnated with copperas that I have had: to paſs by all this (I ſay) as for thoſe vitriol ſtones, whereof we in *England* are wont to make our vitriol, I have ſeen, at the chief work where copperas is made, ſo great a variety of them, (divers of which I have yet lying by me) that I could ſcarcely believe the workmen, when they affirmed them to be all copperas ſtones; and cannot but think it both very likely, that ſome of them contain other mineral ſubſtances beſides vitriol, and very poſſible, that the ſaline parts of thoſe ſtones, upon their ſolution by the rain, may work upon theſe other ſubſtances formerly concoagulated with them, and thereby imbue ſome parcels of the vitriol made of them with qualities other than are eſſential to the nature of vitriol, or belong ordinarily to it.

THAT there is alſo a difference betwixt thoſe bodies, that paſs under the general name of common ſalt, cannot but be obvious to any chymiſt, that hath occaſion to make accurate trials on that ſubject. And as for thoſe concretes, that paſs under the name of ſalt-petre, there is probably no ſmall diſparity among them: for beſides the difference which we have obſerved, and which is obvious enough betwixt good Engliſh nitre, and that which is brought us over from *Barbary*, (which before it is much refined abounds very much with an adventitious ſalt, that taſtes much like ſea-ſalt)

ſalt) beſides this I ſay, thoſe that do uſe both good European and good Eaſt-India ſalt-petre aſſure me, they find much difference betwixt them, and give the preference to the latter. And indeed I have often thought I diſcerned a conſiderable difference in the operations of ſeveral kinds of ſalt-petre even after purification: and probably that ſort of ſalt-petre, which near *London* an ingenious man of my acquaintance does ſometimes (but cannot always) make, chiefly of our ſea-ſalt, hath ſome differing qualities from that, which is drawn the common way out of the earth. And indeed ſalt-petre being but a kind of ſal terræ, generated in very differingly-qualified parcels of earth, may probably receive divers qualities from the particular ſoil, wherein it grows, though theſe qualities lie concealed and unſuſpected under the wonted exterior appearance of nitre. Which conſideration brings into my mind what was lately told me by a very ingenious gentleman, concerning one of the eminenteſt of our *London* phyſicians, who was wont, as this confidant of his aſſured me, as an excellent ſecret, to employ in ſome of his choice remedies that peculiar ſalt-petre, which he had drawn out of the earth digged up in church-yards.

AND ſuch kind of differences would probably in other mineral bodies be taken notice of, if men's prepoſſeſſions did not make them aſcribe the variations they meet with in their experiments, rather to any other cauſe, than the unſuſpected difference of the materials employed about them.

NOR is it only, *Pyrophilus*, among mineral bodies of the ſame name, that ſuch a diverſity is to be found; but, if narrowly looked into, it is very probable, that a greater diſparity may be diſcovered both among vegetables and animals, reputed of the ſame nature, than hath been yet taken notice of. Herbariſts indeed have exerciſed a commendable curioſity in ſub-dividing plants of the ſame denomination, and few naturaliſts ignore, that there are (for inſtance) many ſorts of roſes, and of apples, which differ widely betwixt themſelves, as we ſee the difference betwixt the red roſe and the white, betwixt the crab, the pippin, and the pearmain. But beſides theſe differenees, which are obvious enough to be regiſtered by botanick authors, there may be more undiſcerned ones (which yet may be conſiderable ones) betwixt the individuals of the ſame ultimate ſubdiviſion of plants, ariſing partly from the temperature of the air, which makes (for example) ſenna growing in *England* to differ much from that, which is denominated from *Alexandria*; partly from the nature of the ſoil, as is obvious in the change produced in wild ſimples tranſplanted into gardens; and partly from many other cauſes, which we have not now leiſure to inſiſt upon. But we ſee oftentimes, that one roſe much differs from another of the ſame kind, and one pearmain from another pearmain. To which we may add, that the upper cruſt or ſurface of the earth being impregnated with ſubterraneal exhalations of ſeveral ſorts, and tempered with variety of juices, it may very poſſibly be, that ſome particular plant may attract ſuch juice out of a determinate ſpot of ground, as may give it exotick qualities, and make it differ even from the neighbouring plants of the ſame kind: to which purpoſe I remember, that travelling divers years ſince from *Geneva* towards *Italy*, I was in my paſſage through *Switzerland* by a gentleman of thoſe parts (whoſe brother had been formerly my domeſtick) invited to his caſtle, and entertained among other things with a ſort of wine, which was very heady, but otherwiſe ſeemed to be ſack; and having never met with any ſuch liquor during my long ſtay in thoſe parts, I was inquiſitive to know whence it was brought: and being anſwered, that it grew amongſt thoſe mountains, I could not believe it, till they aſſured me, that growing on a little ſpot of ground, whoſe entrails abounded with ſulphur, it had from the ſoil acquired its inebriating property, and thoſe other qualities,

lities, which made it ſo differing from the wine of the reſt of the vineyards of that country. And now I mention wine, give me leave, *Pyrophilus*, to put you in mind of taking notice, what a great change is made in that liquor, when upon the receſs of the ſpirits and more volatile ſulphurous parts, or elſe the new texture they make with the others, it degenerates into vinegar; and yet how little either diminution of quantity or any other alteration doth appear upon this change to the beholder's eye. And though no body is like to loſe an experiment by miſtaking vinegar for wine, becauſe both thoſe liquors and the changes of them are ſo familiar unto us; and becauſe we are wont to taſte each of them before we employ it; yet who knows what changes there may be in other bodies, with whoſe alterations we are unacquainted, though the eye, which is oftentimes the only ſenſe employed about judging of them, diſcern no change in them? as may daily be obſerved in the ſuperannuated ſeeds of plants, which, after their having been kept long beyond their due time, loſe all their germinating power, without loſing any of their obvious qualities. And here let me further obſerve to you, that urine is made much uſe of, not only by dyers, but ſeveral other tradeſmen, in divers operations (ſome of which we may elſewhere have occaſion to treat of) belonging to their profeſſions. Now theſe men being wont indiſcriminately to employ urine, without examining, whether it be rich in ſalt or not, and how long it hath been kept, it may not be impertinent to take notice, that chymiſts, who have occaſion to diſtil it often in great quantities, aſſure me, that they find a notable diſparity betwixt urines, that of healthy and young men abounding much more with volatile ſalt, than that of ſickly or aged perſons; and that of ſuch as drink wine freely being much fuller of ſpirituous and active parts than that of thoſe, whoſe drink is but beer or water. But becauſe the differing ſtrength of urines, though it be very probable, is not ſo eaſily to be ſatisfactorily made out, we ſhall rather inſiſt on this other obſervation confirmed to us by experience, which is, that though tradeſmen are often wont to boil ſuch and ſuch things indifferently in any urine, as if it were all one how new or ſtale it is, they may ſometimes thereby commit conſiderable errors. For recent urine, wherein the ſaline parts are yet entangled among the reſt, will ſuffer itſelf to be boiled above one half or two thirds away, without the avolation of its volatile ſalt and ſpirits. Whereas urine that has been divers weeks kept, is liable to a putrefaction, whereby the cement (if I may ſo ſpeak) of the ingredients that it conſiſts of, periſhing, or ſome change of texture occaſioning their disjunction, (if not alſo concurring to produce them) the component parts fall aſunder, and the ſaline particles extricating themſelves from the reſt, will even upon a very gentle heat (as trial made on purpoſe has informed us) fly away, and leave a phlegmatick and unactive liquor behind them. In confirmation whereof I muſt acquaint you, *Pyrophilus*, with what lately befel me in reference to the diſtillation of urine: for having cauſed ſome of it to be buried in earthen veſſels in a dunghill, to be there putrified, for five or ſix weeks, I was by divers occaſional journies kept from employing it, till it had lain there between four and five months; and obſerving, when I cauſed it to be taken out, that the covers of the veſſels had not been, by him I employed to put them in, well luted on, and beſides were in ſome places cracked, I ſuſpected, that the heat of the dunghill had not only looſened the ſaline parts of the liquor, but driven them away: and accordingly by diſtilling it in a very gentle heat, and in a very high cucurbit, we obtained, inſtead of an active and ſaline ſpirit, a languid and nauſeous phlegm. And how great odds there may be betwixt ſome experiments made with recent and putrified urine, may be eaſily conceived by him who knows what operation ſalts have in the buſineſs of colours, and is acquainted with their efficacy in thoſe other mechanical experiments, wherein urine is wont to be employed. But I fear I have dwelt too long upon this theme, and therefore I ſhall proceed to the next.

AND in the third place, *Pyrophilus*, I ſhall obſerve to you, that there is a great difference to be found among many things prepared by art, that paſs under the ſame general name: which difference may proceed partly from that, which we have already obſerved to be found in the materials of which ſuch factitious bodies are made, and partly from the way uſed in preparing them. To theſe heads many particulars may be reduced. But we ſhall at preſent reſtrain ourſelves to the mention of two ſorts of prepared bodies, namely, of ſuch as are not purified and exalted enough, and of ſuch as are ſo too much.

AND to begin with the firſt of theſe; it is very certain, that divers chymical experiments delivered by ſober authors have been believed falſe, only becauſe the menſtruums or other materials employed in the unſucceſsful trials of them were not as highly rectified, or otherwiſe as exquiſitely depurated, as thoſe that were uſed by the deliverers of thoſe experiments; ſo that oftentimes the fault of a bad menſtruum is injuriouſly imputed to a good artiſt. That experienced chymiſt *Van Helmont*, in his paradoxical treatiſe of the ſtone, endeavours (as we have elſewhere mentioned) to explicate the manner of its being generated, by the coagulation immediately enſuing upon the mixture of the two volatile ſpirits of urine and of wine. This noble experiment has been by many unſucceſsfully tried, and has been therefore by them diſcredited as a chymical fiction: and indeed the firſt, and I think the ſecond time we attempted to make that coagulum, we found nothing at all of any ſuch thing as we expected upon the confuſion of the two fore-mentioned liquors; which though never ſo much ſhaken, and afterwards permitted to reſt, did never in the leaſt meaſure coagulate, which made us long ſuſpect the experiment; till at length our favourable thoughts of that expert chymiſt making us think it poſſible, that the ſpirits we employed had not been ſufficiently exalted, we dephlegmated ſome by more frequent, and indeed tedious rectifications (which yet proved but neceſſary) and then were ſatisfied by more accurate trials, that *Helmont* had not miſinformed us.

So likewiſe the ſame author in his treatiſe *de Peſte* much extolling, as a friend to the ſtomach, the entrails, the nervous parts, and even the head, the tincture or ſolution of amber made with ſpirit of wine (which medicine is indeed no ignoble one, when adminiſtered to conſtitutions, that can well bear the heat of it) divers phyſicians and chymiſts have attempted the preparing of this tincture with ſuch bad ſucceſs, that they have given out, that either *Helmont* delivered what was not true, or concealed ſome conſiderable circumſtance of the proceſs.

WHEREAS having digeſted ſufficiently dephlegmed ſpirit of wine upon very finely powdered amber (which, if it be the higher-coloured, yields the deeper tincture) in a very gentle heat (for the neglect of which caution even expert artiſts have often loſt their pains and glaſſes) we have ſeveral times had a good yellow tincture of amber, which was diſcernable in the menſtruum both by the ſmell and taſte; and to ſatisfy ſome, that ſuſpected the tincture to proceed but from the exaltation of the menſtruum itſelf by digeſtion, and to manifeſt, that it was a real ſolution of the ſubtiler parts of the amber, we poured ſome drops of it into beer, or water, into which the ſpirit of wine ſuddenly diffuſing itſelf, the diſſolved amber was plainly diſcernable ſwimming like a thin film upon the ſurface of the liquor, whence, little by little, it ſteamed away into the air.

THERE is likewiſe, as we have tried, to be drawn with ſpirit of wine from pure ſalt of tartar a pretty high tincture, and of a taſte, which I thought not unworthy the taking notice of: but having a while ſince tried to draw this tincture with ſpirit of wine, which (unknown to me) was much too weak for that purpoſe, after I had kept the glaſs a while in digeſtion, coming to look whether or no the ſpirit was tinged, I found, that the ſalt of tartar had drawn to itſelf and imbibed the aqueous particles of the ſpirit of wine, and being thereby (for a great part of it) diſſolved into a liquor like that, which is commonly called oil of tartar *per deliquium*, the ſubſiding ſalt was by the interpoſition of that ſaline liquor protected from the action of the ſpirit of wine; which being by this new way dephlegmed, would not mix with the ſaline liquor, but ſwam entirely above it. To which I ſhall only add in general, that the German chymiſts are divers of them ſo accurate in the rectification of their ſpirit of wine, that in *England*, where we are wont to be leſs careful about that particular, it is uſual enough for thoſe experiments of theirs to be unſucceſsfully tried, wherein the alcohol of wine (as they call it) is requiſite.

And as ſpirit of wine, ſo many other menſtruums are made unfit for the perfecting of divers real experiments, barely by their not being ſufficiently freed from their weakening aquoſity.

Nor is it only, *Pyrophilus*, in menſtruums, but in divers other bodies, that the want of an exquiſite depuration may produce in experiments variety of events. As for inſtance, it has been complained of by ſober men, that their preparations of ſilver, though never ſo carefully made, have been apt to produce violent vomits; whereas we have not obſerved a well-prepared medicine of duly refined ſilver to work emetically, even in women and girls, but by ſeige or urine. But we cannot wonder at the violent operation of medicines made of ordinary ſilver: for not only that, which is coined, is wont, as the mint-maſters themſelves have confeſſed to me, to be allayed with ſometimes about a twelfth part, ſometimes a ſmaller or greater proportion of copper, for the greater conveniency of the coin; but even that ſilver, which is commonly at great rates ſold for refined ſilver, is not wont to be ſufficiently freed from its copper. Which I not long ſince manifeſted in the preſence of one of our richeſt and eminenteſt refiners, by diſſolving ſome of his pureſt ſilver in his own aqua fortis; for the greenneſs of the ſolution quickly betrayed the adherency of *Venus* to the ſilver. And no wonder, for I have ſeldom ſeen our chiefeſt refiners blow off from their ſilver upon the teſt above half its weight of lead, whereas we think not our ſilver ſufficiently refined for ſome purpoſes, till it have been freed from five or ſix times its weight of *Saturn*; and then it has ſometimes afforded a ſolution almoſt as clear as water, with only now and then a light touch of ſky-colour, but nothing near ſo high as the ceruleous (liquor that is ſuppoſed to be a true) tincture of ſilver, artificially ſeparated from the reſt of the body.

Now that ill effects by the mixture of copper may be produced in ſuch medicines, as ought to be of pure ſilver, he, that is acquainted with the violent emetic qualities of *Venus*, can ſcarcely doubt. And as in men's bodies, ſo in other ſubjects, thoſe experiments may eaſily deceive the artiſt's expectation, when he hopes to perform with ſilver and copper together thoſe things, which ſuppoſe and require ſilver without copper, or any adventitious metal: and as ſilver, ſo gold is very often employed for pure, when it is not ſo: for even the foliated gold, which is commonly ſold here in *England*, how fine ſoever it is reputed, is not altogether free from the pollutions of other metals; for our gold-beaters, though for their own profit ſake they are wont to uſe the fineſt coined gold they can get (as that which is capable of the greateſt extenſion under the hammer) yet they ſcruple not to employ coined gold, and that the mint-maſters (as themſelves inform me) are wont to allay with copper or ſilver, to make the coin more ſtiff, and leſs ſubject to be waſted by attrition. And as for thoſe many goldſmiths and chymiſts, who think their gold moſt requiſitely refined, when they have blown from it on the teſt a due proportion of lead, they may be therein ſometimes miſtaken: for though *Saturn* may carry away with him all the copper, that did imbaſe the gold, yet he does not likewiſe free it from the ſilver (for which purpoſe aqua fortis is therefore wont to be uſed) nay, the ſkilfulleſt refiner, that I ever yet knew, hath ſeveral times affirmed to me, that cupelling fine gold with lead, the gold has after retained and protected from the fire a proportion of ſilver, that lay lurking in the lead, and was afterwards ſeparated from the gold by aqua fortis, but in ſo ſmall a quantity, that the experiment (the coſt and pains conſidered) was not lucriferous. And of this ſort of inſtances, *Pyrophilus*, more might be preſented, if we did not think prolixity might be unwelcome to you.

BUT as many experiments ſucceed not according to expectation, becauſe the menſtruums employed about them were not pure enough; ſo ſome miſcarry, becauſe ſuch menſtruums are but too exactly depurated: for it is not ſo much the purity of liquors in their kind, as their fitneſs for the particular purpoſe, to which they are deſigned, that is in experiments to be principally regarded. For inſtance, we have ſometimes, for recreation ſake, and to affright and amaze ladies, made pieces of white paper and linen appear all on a flame, without either burning, ſinging, or as much as diſcolouring them. This is performed by plunging the paper very thoroughly in weak ſpirit of wine, and then approaching it to the flame of a candle; by which the ſpirituous parts of the liquor will be fired, and burn a pretty while without harming the paper. But if this experiment be tried with exquiſitely rectified ſpirit of wine, it will not ſucceed. Of this phænomenon this plauſible reaſon has been aſſigned, that the flame of the ſpirit of wine is ſo pure and ſubtile, that, like an *ignis lambens*, it will not faſten upon the paper. But experience has informed us, that this conjecture is but a miſtake, for the flame of ſpirit of wine is ſo hot, that I have in lamp-furnaces employed ſpirit of wine inſtead of oil, and with the ſame flame I have not only lighted paper, but candles, and even melted foliated gold. The true reaſon therefore, why that paper is not burned by the flame, that plays about it, ſeems to be, that the aqueous part of the ſpirit of wine being imbibed by the paper, keeps it ſo moiſt, that the flame of the ſulphureous parts of the ſame ſpirit cannot faſten on it. And therefore when the deflagration is over, you ſhall always find the paper moiſt; and ſometimes we have found it ſo moiſt, that the flame of a candle would not readily light it. And on the other ſide, having purpoſely made trials of plunging paper into ſufficiently dephlegmated ſpirit of wine, the paper not having aqueous moiſture to defend it, was very readily kindled and burned by the flaming ſpirit. And one of our beſt ways to try the pureneſs of ſpirit of wine is grounded on this very ſuppoſition: for dipping it in a cotton-wiek like that of a candle, and ſetting it on fire, if the flame faſten on the wiek, it is a ſign of the goodneſs of the ſpirit; but if it do not, we conclude it to be weak, and not ſufficiently dephlegmed. It hath been likewiſe obſerved, that aqua fortis will work more readily on lead, if it be allayed with water, than if it be purely rectified. I otherwhere alſo mention an aqua fortis I have uſed, which was ſo ſtrong, that it would not well diſſolve ſilver itſelf, unleſs I firſt diluted it with fair water. And within this very week, wherein I write theſe things, I have had an unwelcome proof, that liquors may by too exquiſite a depuration be made unfit for our purpoſes. For having, to gratify ſome ingenious friends, made a certain menſtruum, wherewith we had formerly done ſome things upon gold, which were (not altogether without cauſe) thought ſtrange enough, we took care at this time to ſeparate it from whatever was either of an aqueous or an earthy nature, more exactly than ever we had formerly done. But coming to make uſe of this ſort of menſtruum, we found to our trouble and loſs, that inſtead of performing its wonted operations upon gold better than before, we could do nothing at all with it: for it will not now by heat itſelf be brought to touch gold, though that metal were wont to be diſſoluble in it even *in frigido*. And to ſatisfy you, that it was the too exquiſite depuration of this liquor, eſpecially from its terreſtrial parts, that thus unfitted it to touch a metal, which is otherwiſe wont to melt as it were naturally in it without ebullition (almoſt like ice in luke-warm water;) we will ſubjoin, that not only we in vain tried to make it ſerviceable by weakening it with fair water; but having, for trial-ſake, taken a little of this numerical parcel of liquor before it was ſo carefully rectified, we found, that it diſſolved crude gold as well as we had reaſon to expect. And it would be conſidered, whether or no in the extraction of the tinctures of ſeveral bodies, chymiſts do not only put themſelves to a needleſs, but to a prejudicial trouble, when they refuſe to employ any other ſpirit of wine, than that which is highly rectified. For, though in many bodies the parts deſired by the artiſts being the ſulphureous ones, the menſtruum is the better for an exquiſite dephlegmation; yet in divers other concretes the uſeful and efficacious parts have in them ſomething of ſaline, which makes them more free to impregnate copiouſly ſuch liquors, as have ſome aqueous mixed with their ſulphureous parts.

But becauſe there is nothing more eaſy than by diluting ſpirit of wine, though never ſo ſtrong, to make it as weak as one pleaſes; and becauſe pure ſpirit of wine is that of all other menſtruums, that chymiſts generally make moſt uſe of, and which coſts them moſt of charge and trouble (inſomuch that here in *London* that, which is perfectly dephlegmed, is valued, in their ſhops that ſell both, at ten times the price of common ſpirit of wine;) I preſume you will not take it ill, that without being obliged to it by the title of this diſcourſe, I take this occaſion to acquaint you with the way I employ to obtain dephlegmed ſpirit of wine; eſpecially ſince the practice of the common way of frequent rectifications is (not to mention other inconveniences) wont to prove either exceeding tedious, or inſufficient. Put then about an inch thick of tartar calcined to whiteneſs (for I find it not neceſſary to reduce it to a ſalt) and very dry, into the bottom of a tall and ſlender glaſs body, and pour on it as much ſpirit of wine, that has been but once rectified, as will, when they have been ſhaked together, ſwim above the tartar a finger's breadth (more or leſs in proportion to the tartar you put in) and then the head and receiver being carefully faſtened on again, in a gentle heat draw off the ſpirit of wine, ſhifting if you pleaſe the receiver, when about half is come over, and if need be, rectifying once more all that you diſtil upon dry calx of tartar as before. Whether or no you may meet with this method in ſome chymical books, I know not: but it ſeems, that either it has not been clearly taught, or has been propoſed by ſuſpected authors, or elſe among other proceſſes, by being found in whoſe company it has been diſcredited. For the moſt antient and experienced diſtillers I have met with, have either contented themſelves to follow the common way of repeated rectifications, though thereby they loſe much time, and much ſpirit of wine; or elſe have had recourſe to peculiar veſſels of ſuch a height, as beſides that they are neither eaſily nor cheaply to be procured, do not, as far as I have hitherto ſeen, excuſe the need of reiterated rectifications. Whereas, when we conſidered, that the fixed ſalt of tartar readily imbibes aqueous bodies, and that yet it will not at all mix with pure ſpirit of wine, it was eaſy to conclude, that the phlegmatic part of the ſpirit of wine would be ſoaked up by the alcalizate ſalt, whereby the inflammable part would be freed from it. And accordingly when we proceeded after the manner above preſcribed, we found, that the liquor, that was produced upon the firſt rectification from the ſalt, being fired in a warm ſilver-ſpoon, did not leave behind it one drop of phlegm, or ſo much as the leaſt moiſture upon the ſpoon; nay, and indeed did endure a ſeverer examen, to which for curioſity's ſake we thought fit to put it. And when the diſtillation was carefully made, we found by frequently (for trial-ſake) ſhifting the receiver, that all the ſpirit that aſcended was (to ſenſe) equally pure, ſince that which came up laſt of all, even till the calx ſeemed to begin to grow dry, by beginning to cleave at the top, did burn all away, as well as that which came over firſt. And having for further trial taken out the calcined tartar, and diſtilled it with a good fire, it yielded us pretty ſtore of a nauſeous and ſtrongly ſcented liquor, which ſeemed to be but phlegm, both to the taſte, and by its not being at all inflammable, though carefully tried. The ſame calx of tartar being kept in ſome earthen veſſel upon the fire till it be well dried, which will require a good heat, may be employed more than once in this operation. And it was not needleſly that we preſcribed bodies tall and ſlender; for we found not the experiment to ſucceed in large and low ones, and much leſs in retorts, in which the phlegm is wont to riſe together with the ſpirit; yet we found, that provided the diſtillation were made with a ſufficiently mild heat, a glaſs, though very broad, and but moderately high, would ſerve the turn ſo far, as that the firſt half that aſcended (the other being very weak) proved a ſpirit, that in a ſilver-ſpoon would burn perfectly all away. And becauſe white calx of tartar is ſometimes not ſo eaſy to be procured, we will add, that we have for trial-ſake ſometimes ſubſtituted quick-lime, or ſalt of pot-aſhes (made by a ſingle ſolution, filtration, and coagulation) with no bad ſucceſs, eſpecially in caſe of removing the receiver before the aſcenſion of the laſt part of the liquor, though even that itſelf has ſometimes from quick-lime come up inflammable enough. And therefore this alcohol of wine we peculiarly call the alcalizate ſpirit of wine; and the rather, becauſe *ſpiritus vini tartarizatus*, which perhaps may be thought the propereſt name for it, is employed by eminent chymical writers to ſignify a different thing. And a practicable way of making ſuch an alcalized and pure ſpirit of wine we thought not unfit to teach you here once for all, in regard the menſtruum is ſo highly uſeful, not only for tinctures, extracts, and many other chymical operations, but in the making of divers philoſophical experiments, and particularly ſome of thoſe, which you may meet with in our writings. And an eminently ingenious perſon (but to me a ſtranger) chancing to get a ſight of this eſſay, was pleaſed to give me thanks for this laſt part of it; becauſe, though he had very often made uſe of ſalt of tartar to improve ſpirit of wine, yet he did it before, not to dephlegm the weaker liquor, but to acuate the ſtrong with the alcali: which though I deny not to be a thing feaſible, yet (as I told him) unleſs it be ſkilfully attempted, the highly rectified liquor, that is poured on, will rather leave ſome of its moſt ſpiritous parts behind, than carry up ſo fixt a ſalt.

The Second ESSAY,

Of Unſucceeding EXPERIMENTS.

WHAT has been already ſaid, *Pyrophilus*, may, I hope, ſuffice to ſhew you, how experiments may miſcarry upon the account of the materials employed in trying them. And therefore we ſhall now paſs on to conſider the contingencies, to which experiments are obnoxious, upon the account of circumſtances, which are either conſtantly unobvious, or at leaſt are ſcarce diſcernible till the trial be paſt. And becauſe theſe circumſtances can hardly be diſcourſed of in an accurate method (which their nature will ſcarce admit of) I ſhall not tie myſelf to any other order in ſetting down the inſtances, which occur to me on this occaſion, than that wherein they offer themſelves to my memory.

AND firſt I muſt acquaint you with what was not long ſince ſeriouſly related to me by doctor *K.* a perſon exceeding far both from the cuſtom, and, in this particular, from the temptation of telling untruths. He then aſſured me, that lending his laboratory in *Holland* to a friend of his during his own abſence, and leaving in that laboratory among other things great ſtore of aqua fortis of ſeveral compoſitions, which he had made, to employ about his famous ſcarlet-dye, this friend of his ſent him word a while after his departure, that by digeſting gold with an aqua fortis, he had ſeparated the tincture or yellow ſulphur from it, and made it volatile (the remaining body growing white) and that with this golden tincture he had, not without gain, turned ſilver (as to part of it) into very perfect gold. Upon which advertiſement the doctor ſpeedily returning to his laboratory, did himſelf with the ſame aqua fortis divers times draw a volatile tincture of gold, which did turn ſilver into true gold: and (that I may add that upon the bye, to gratify your curioſity) when I demanded, whether or no the tincture was capable to tranſmute or graduate as much ſilver, as equalled in weight that gold, from whence the tincture was drawn, he aſſured me, that out of an ounce of gold he drew as much ſulphur or tincture, as ſufficed to turn an ounce and a half of ſilver into that nobleſt metal. Which I am the more diſpoſed to believe, partly becauſe my trials permit me not to doubt of the ſeparableneſs of a yellow ſubſtance or tincture from gold; and partly becauſe I am tempted to think, that ſilver may have in it a ſulphur (to ſpeak in the chymiſts language) which maturation is capable to graduate into a golden one, by having been certified by the obſervations of men very experienced in metalline affairs (and perhaps too by my own) that ſometimes by corroſive liquors (which Sir *Francis Bacon* alſo, if I miſtake not, ſomewhere obſerves) and ſometimes by the operation of common ſulphur (eſpecially well opened and aſſociated with fit ſalts) ſilver has afforded ſome grains of very pure gold. But our doctor found himſelf much miſtaken in the hopes of growing rich by this experiment; for a while after endeavouring to make it again, his hopes were fruſtrated, which he aſcribes to the aqua fortis, and therefore has attempted the ſame work afreſh. But ſince all his trials have been hitherto fruitleſs, it is not improbable, that the diſappointment proceeded from ſome other more abſtruſe cauſe; for we find ſuch adventures to have ſometimes befallen artiſts irreparably. And *Glauber* alone, if you will therein credit him, tells us of ſeveral ways, by which he made gold once, and could not do it again. Upon which ſubject I muſt not omit thoſe very illuſtrious teſtimonies and inſtances of this nature, that I find recorded by that ornament of his age and quality, the prince of *Mirandola*, in his treatiſe *de Auro.* *Novi* (ſays he) *qui mihi aſſeruerit ſemel ſe ex mobili argento, quod vivum dicitur, ſtabile verumque argentum confeciſſe ſuccis & foliis herbarum, idque vendidiſſe peritis explorandæ metallicæ veritatis*; *eiſdem mox uſum ſe foliis fruſtra, & quod ſemel perfecerat, nunquam alias, quanquam id ſæpe tentaverit, perficere potuiſſe.*

Lib. 3. cap. 6.

Alium novi (ſays he further) *qui adhuc apud vivos moratur, cui cum aurum & argentum circiter quindecies per artem effectum eſſet, amiſit artem eam, accepitque oraculo ſocii per quietem habito, id ingratæ mentis vitio contigiſſe. Ut hinc etiam veritatem apoſtolici dicti condiſcamus, Neque qui plantat, neque qui rigat, eſt aliquid, ſed incrementum dat Deus.* And to both theſe narratives our learned prince does in the ſame book add divers others. *Retulit quidam mihi* (ſubjoins he) *ſeſe aurum ex argento feciſſe ſemel magnâ copiâ*; *ſecundo ſe uſum eiſdem rebus, feciſſe quidem, ſed minimâ ſemper quantitate, ſic ut detrimentum lucro majus eſſe ſupputaverit. Veniſſe in mentem, uti detrimentum effugere poſſit, ſi non ex argento, ſed ex ære melioris conditione metalli, ſeſe conſequi experiretur, idque ſe conjecturis firmis nixum tentaviſſe: cumque in eo fuiſſet, ut rem ſeſe adepturum ſperaret, miris modis eveniſſe, ut nihil omnino conſequeretur.*

Idem (continues the prince) *affirmavit ab amico, qui expertus hoc ipſum fuerat accepiſſe, qui cum ex cinnabari argentum feciſſet optimum, ſæpenumero ſeſe poſtea inſiſtentem operi majore cum diligentia ſemper eventu rei fuiſſe fruſtratum.* And to theſe relations of this famous prince I could add others of ſome acquaintances of mine, who having either once or twice made *luna fixa* (as artiſts call that ſilver, which wanting but the tincture of gold abides the trial of aqua fortis, *&c.*) or ſome other lucriferous experiment, have ſince in vain attempted to do the like again; and yet have their eyes ſo dazzled by the gold and ſilver they have (either ſeparated or) made, that they are not to be prevailed with to deſiſt from proſecuting their uncertain hopes.

THAT divers experiments ſucceed, when tried in ſmall quantities of matter, which hold not in the great, it may ſave you ſomething to be advertiſed of; divers projectors, eſpecially chymiſts, having already very dearly bought the knowledge of that truth, for oftentimes a greater and unwieldly quantity of matter cannot be expoſed in all its parts to a juſt degree of fire, or otherwiſe ſo well managed, as a leſs quantity of matter may be ordered. But this is ſo manifeſt a truth to thoſe, that have dealt much in experiments, that whereas many chymiſts would be vaſtly rich, if they could ſtill do in great quantities what they have ſometimes done in little ones, many have undone themſelves by obſtinately attempting to make even real experiments more gainful.

I HAVE not been very ſolicitous to ſubjoin particulars to the foregoing obſervations, becauſe that by reaſon of the contingency of ſuch experiments, as would be the moſt for my preſent purpoſe, you might poſſibly be tempted to loſe toil and charges upon trials, very likely not only to delude your hopes, but perhaps to make you diſtruſt the fidelity of our relations. Yet for illuſtration-ſake of what we have delivered, I am willing to mention ſome few contingent experiments, that occur to my thoughts.

AND firſt, it is delivered by the Lord *Verulam* himſelf, as I remember, and other naturaliſts, that if a roſe-buſh be carefully cut as ſoon as it has done bearing, it will again bear roſes in the autumn. Of this many have made unſucceſsful trials, and thereupon report the affirmation to be falſe; and yet I am very apt to think, that the Lord *Verulam* was emboldened by experience to write as he did. To clear up which difficulty, let me tell you, that having been particularly ſolicitous about the experiment, I find by the relation both of my own and other experienced gardeners, that this way of procuring autumnal roſes will in moſt roſe-buſhes moſt commonly fail, but in ſome, that are good bearers, it will ſucceed; and accordingly having this ſummer made trial of it, I find, that of many buſhes, that were cut in *June* in the ſame row, the greater number by far promiſe no autumnal roſes, but one, that hath manifeſted itſelf to be of a vigorous and prolific nature, is at this preſent indifferently well ſtored with damaſk-roſes. And there may be alſo a miſtake in the kind of roſes; for experienced gardeners inform me, that the muſk-roſe will, if it be a luſty plant, bear flowers in autumn without the help of cutting. And therefore that may be miſaſcribed to art, which is the bare production of nature. And cinnamon roſe-buſhes do ſo much better thrive by cutting than ſeveral other ſorts, that I remember, this laſt ſpring, my gardener having (as he told me) about mid *April* (which was as ſoon as that kind of roſe-buſh had done bearing) cut many of them in my garden, I ſaw about the middle of *June* ſtore of the ſame buſhes plentifully adorned both with buds and with blown flowers.

An uncertainty not unlike that, which we have newly taken notice of in the experiment of producing autumnal roſes, has been likewiſe obſerved in the attempts, that have been made to make divers ſorts of fruit grow upon the ſame tree. And as for differing ſorts of fruits of the ſame denomination, as apples, pears, *&c.* though ſome ſevere naturaliſts are unwilling to believe, that they can be made to grow upon the ſame tree; yet we dare not imitate their ſeverity, having lately ſeen various ſorts of pears fed by the ſame tree, and elſewhere three and twenty ſorts of apple-grafts flouriſhing upon the ſame old plant, and moſt of them adorned with fruit. Nay, and though the fruits be not of the ſame denomination, yet if they be of kin in nature, they may very poſſibly be brought to grow on the ſame tree: for we lately gathered ripe apricocks and ripe plums upon one tree, from which we likewiſe expect ſome other ſorts of ſtone-fruit. But to make fruits of very differing natures be nouriſhed proſperouſly by the ſame ſtock, is ſo difficult a thing, that we can at moſt but reckon it among contingent experiments. For though *Pliny* and *Baptiſta Porta* relate their having ſeen each of them an example of the poſſibility of producing on one tree great variety of differing fruits; and though ſuch a perſon as the deſervedly famous aſtronomer Dr. *Ward* aſſures me, that he has particularly taken notice of pears growing upon an apple-tree; and I elſewhere add a reſembling obſervation of ours; yet certainly this experiment has been for the moſt part but very improſperouſly attempted; nor have I yet ever ſeen it ſucceed above once, though tried with very much care and induſtry. And I remember, that this very year, in the ſame garden where I gathered the apricocks and plums above mentioned, I ſaw the cions of a pear-tree ſo ſkilfully grafted upon an apple-ſtock, that it flouriſhed very much with bloſſoms in the ſpring, and gave me great hopes, that it would bear fruit this newly-paſt ſummer, but has deceived my expectation; as divers other plants ſo grafted in the ſame garden have for many years deluded the hopes of the ſkilful maſter of it, who aſſures me, that though divers of them did for ſome years ſucceſſively afford promiſing bloſſoms, yet they all decayed away without bearing any of them any fruit. Which yet may ſeem ſomewhat ſtrange, ſince not only we have this ſummer gathered pears upon a graft, which a divine, to whom the garden belongs, affirmed to have been grafted upon a quince-tree; and the induſtrious *Kirchir* tells us, that *Experientia docet Perſicum moro inſitum fructus proferre, &c. de quo nullum eſt dubium utpote vulgare penè*: ‘but experience tells ‘us, that as little as a white-thorn and a pear-tree ſeem of kin, a cion of the latter ‘will ſometimes proſper will, being grafted upon a ſtock of the former.’

Artis Mag. Lucis & Umbræ, lib. 1 *p* 3 *cap.* 6.

To contingent experiments, *Pyrophilus*, you may, if you pleaſe, refer what is delivered by thoſe learned writers, who affirm, that if a lixivium made of the aſhes or fixed ſalt of a burned plant be frozen, there will appear in the ice the idea of the ſame plant; for we have divers times purpoſely made trial of this experiment without the promiſed ſucceſs. And I remember, that in the laſt cold ſeaſon, proper for ſuch trials, I purpoſely made a lixivium of fair water and ſalt of wormwood; and having frozen it with ſnow and ſalt after the manner of congelation elſewhere declared, I could not diſcern in the ice any thing more like to wormwood than to ſeveral other plants. And having about the ſame time, and after the ſame manner, expoſed to congelation a thin phial full of a ſtrong decoction of wormwood (from which an idea of the plant may be more probably expected) thoſe, to whom I ſhewed it, after it was frozen, could diſcern as little like wormwood in it as myſelf. It is true, that in both theſe phials the ice ſeemed ſomewhat oddly figured; but it is true alſo, not only that we have obſerved that water, wherein a ſaline body, as ſalt-petre, or ſea-ſalt, or ſugar, *&c.* has been diſſolved, has afforded us ice, which ſeemed to ſhoot into ſeveral figures, but even in ordinary water congealed we have often ſeen ice figured, as if the water had been no elementary body; which needs not be admired ſince (to omit other cauſes, which may concur to the production of this effect) many waters gliding through earths abounding in ſaline particles of this or that nature, may be eaſily, in their paſſage, impregnated with them; whence perhaps it comes to paſs, that dyers find ſome waters very fit, and others very unfit for the dying of ſcarlet and ſome other colours. And therefore we cannot but think, that the figures, that are oftentimes to be met with in the frozen lixivium or decoction of a plant, will afford but uncertain proofs, that the idea of each, or ſo much as of any determinate plant, diſplays itſelf conſtantly in that frozen liquor. And I much fear, that moſt of thoſe, that tell us, that they have ſeen ſuch plants in ice, have in that diſcovery made as well uſe of their imagination as of their eyes. And it is ſtrange to obſerve what things ſome men will fancy, rather than be thought to diſcern leſs than other men pretend to ſee. As I remember Mr. *R.* the juſtly famous maker of dioptrical glaſſes, for merriment telling one, that came to look upon a great tube of his of thirty foot long, that he ſaw through it in a mill ſix miles off a great ſpider in the midſt of her web; the credulous man, though at firſt he ſaid he diſcerned no ſuch thing, at length confeſſed he ſaw it very plainly, and wondered he had diſcovered her no ſooner. But yet, *Pyrophilus*, becauſe two or three ſober writers do ſeriouſly relate ſome ſtories of that nature upon their own obſervation, I am content for their ſakes to reckon their experiments rather among the contingent than the abſolutely falſe ones: for it is not impoſſible, but that among the many figures, which frozen liquors do ſometimes put on, there may appear ſomething ſo like this or that plant, that being looked upon with the favourable eye of a prepoſſeſſed beholder, it may ſeem to exhibit the picture of the calcined vegetable: and we ourſelves, not very long ſince, ſetting to freeze in ſnow and ſalt a fine green ſolution of good verdegreaſe which contains much of the ſaline parts of the grapes coagulated upon the copper by them corroded) obtained an ice of the ſame colour, wherein appeared divers little figures, which were indeed ſo like to vines, that we were ſomewhat ſurprized at the experiment; and that which increaſed our wonder was, that another part of the ſame ſolution being frozen in another phial by the bare cold of the air, afforded us an ice angularly figured as we have obſerved the ice of ſaline liquors oftentimes to be) but not at all like that made by the application of ſnow and ſalt. And having, for further trial ſake, ſuffered that ice, wherein the vines appeared to thaw of itſelf, and having then frozen the liquor a ſecond time in the ſame phial, and after the ſame manner as formerly, we could not diſcern, in the ſecond ice, any thing like that, which we had admired in the firſt. And in wine and vinegar, as much as thoſe liquors partake of the nature of the vine, we have not, after congelation, obſerved any peculiar reſemblance of it in figure.

THE mention we have been making of ice brings into my memory another experiment, which may perhaps be reckoned likewiſe among contingent ones, and that is the experiment of burning with ice as with a glaſs lens; which though ſome eminent modern writers preſcribe to be done, without taking notice of any difficulty in it, yet both we and others, that have induſtriouſly enough tried it, have met with ſuch defeating circumſtances in it, eſpecially from the uniform texture wont to be met with in moſt ice, that the making of ſuch burning-glaſſes may be well enough referred to thoſe experiments, whoſe conſtant ſucceſs is not to be relied on, as we elſewhere more particularly declare.

IN the trade of dying there is ſcarce any tinging ingredient, that is of ſo great and general uſe amongſt us as woad or glaſtum; for though of itſelf it dye but a blue, yet it is uſed to prepare the cloath for green and many other of the ſadder colours, when the dyers have a mind to make them permanent, and laſt without fading: but yet in the decocting of woad to make it yield or ſtrike its colour, there are ſome critical times and other circumſtances to be obſerved; the eaſy miſtake of which oftentimes defeats the dyer's expectation to his very great loſs, which ſometimes he knows not to what to impute, of which I have heard ſeveral of them complain: and therefore divers of our leſs-expert dyers, to avoid thoſe hazards, leave off the uſe of woad, though growing plentifully enough here in *England*, and inſtead of it employ indico, though it coſt them dearer, as being brought hither ſometimes from *Spain*, ſometimes from the *Barbadoes*, and oftentimes even from the *Eaſt-Indies*.

OUR *London* refiners, when, to part ſilver and copper, they diſſolve thoſe mixed metals in aqua fortis, are wont afterwards to dilute the glutted menſtruum with ſtore of fair water, and then with copper-plates to ſtrike down the diſſolved ſilver. But becauſe by this manner of proceeding much copper is wont, after the ſeparation of the ſilver, to remain in the menſtruum, as may appear by its high tincture, that this thus impregnated liquor may be improved to the beſt advantage, they are wont to pour it upon what they call whiting which is ſaid to be a white chalk or clay finely powdered, cleanſed, and made up into balls) wherewith the tincted parts incorporating themſelves, will, in ſome hours, conſtitute one ſort of verditer fit for the uſe of painters, and ſuch other artificers as deal in colours, leaving the remaining part of the menſtruum an indifferently-clear liquor; whence they afterward, by boiling, reduce a kind of ſalt-petre fit, with the addition of vitriol (and ſome freſh nitre) to yield them a new aqua fortis.

AND theſe things I mention, *Pyrophilus*, that you may know what I mean, when I tell you, that ſometimes the refiners cannot make this verditer for a great while together, and yet cannot tell, whence their diſability to make it proceeds. Of which contingency I remember I lately heard one of the eminenteſt and richeſt of them ſadly complain, affirming, that neither he, nor divers others of his profeſſion, were able, not long ſince, to make verditer for divers months together; and that ſeveral others were yet at a loſs in reference to that particular: though for his part he had, without knowing the cauſe of this contingency, found a remedy for it, namely, to warm the menſtruum well before it be poured on the whiting; on which, when the liquor was warm, the tincted parts would faſten, though they would not, whilſt (according to the cuſtom of refiners) it was poured on cold.

MAKING likewiſe the other day a viſit to the chief copperas work we have in *England*, one of the overſeers of it, who went along with me to ſhew me the contrivance of it, aſſured me, that divers times, by the miſtake or neglect of a circumſtance in point of time, they had loſt, and are yet ſubject to loſe, ſome thouſands of pounds of vitriol at a time, which in ſpite of their wonted, but not ſufficiently attentive and ſkilful care, would degenerate into an unctuous ſubſtance, not to be reduced into good vitriol again; unleſs by the tedious way of throwing it abroad, and expoſing it with the unprepared ſtones, from which they draw their vitriol, to the rain and ſun to be opened anew, and fitted for the yielding of vitriol after the ſame manner with thoſe crude minerals.

UPON this occaſion I muſt not omit, becauſe much conducing to the ſcope of our preſent diſcourſe, a memorable relation, that I have met with in the Indian Hiſtory of the learned *Joſephus Acoſta*, who diligently ſurveyed the famous and almoſt ineſtimable mines of *Peru*, and (for one that was not a chymiſt) has delivered divers conſiderable and judicious obſervations about them. That which I am now to mention, is in that chapter, where he treats of the ſilver of the *Indies*, ſet down in theſe words: ‘ It is ſtrange to ſee not only the difference betwixt the refining of metal ‘ by fire, and without it by quick-ſilver, but alſo that ſome of theſe metals, ‘ which are refined by the fire, cannot well be molten with any artificial wind, as ‘ with bellows, but when it is kindled, and blown with the natural air or natural ‘ wind. The metal of the mines of *Porco* is eaſily refined with bellows; and that of ‘ the mines of *Potoſi* cannot be molten with bellows, but only by the breath of their ‘ guayars, which are ſmall furnaces upon the ſides of the mountains, built expreſsly ‘ where the wind lies, within the which they melt this metal: and though it be hard ‘ to yield a reaſon of this difference, yet it is moſt certain and approved by long ‘ experience.’

Joſephus Acoſta. lib. 4. cap. 5.

IF there be any trade that obliges the artificers to be aſſiduouſly converſant with the materials they employ, it is that of the glaſs men; and yet even to them, and in their moſt ordinary operations, there happen now and then little accidents, which, though they know not well to what to aſcribe, are not yet capable of hindering them from doing ſometimes what they have done a thouſand times. And I remember, that among the laſt times I have been at a glaſs-houſe, an eminently-ſkilful workman, whom I had purpoſely engaged to make ſome veſſels for me, that required more than ordinary dexterity, was not able, when I came thither, to make metal (as they call that colliquated mixture of ſand and fixt ſalt, whereof they blow their glaſſes) tolerably fit to be employed: wherefore he deſired me to take the pains to come again another day, and he would try to repair his unluckineſs. But the next time I came, though it were upon appointment, his metal proved again unſerviceable, and inſtead of being colourleſs, when it was cold, looked as if it had been ſtained with blue and yellow, and was beſides brittler than it ought to have been. So that it need be no ſuch wonder, if philoſophers and chymiſts do ſometimes miſs of the expected event of an experiment but once, or at leaſt but ſeldom tried, ſince we ſee tradeſmen themſelves cannot do *always*, what, if they were not able to do *ordinarily*, they could not earn their bread.

IT is affirmed by *Helmont* and others, that treat of the *Lapides Cancrorum*, that they grow within the ſkulls of thoſe craw-fiſhes, from whence they have their name: but I have known good anatomiſts complain, that they have ſought them in vain in the heads of thoſe fiſhes, which may well make them diſtruſt the veracity of thoſe, that aſcribe them to that ſort of animals; yet we have often taken thoſe ſtony concretions out of the heads of craw-fiſhes. But paſſing lately through *Hungerford*, a town famous for the plenty of ſuch kind of fiſh, we made diligent enquiry concerning their nature, and were there informed by thoſe that looked to them, that the concretions above-mentioned are to be found in their heads but about that ſeaſon of the year, wherein they ſhift their ſhells, and that at other times of the year, ſeveral perſons had in vain endeavoured to ſtore themſelves with crabs eyes at *Hungerford*. And indeed, having at the laſt time of my being there (which was about the latter end of *June*) cauſed divers large ones to be taken out of the water, we found theſe little ſtones but in the head of one of them; whereas about a fortnight before, which was near the ſummer ſolſtice, paſſing by that place, we found in the wonted parts of the heads ſeveral ſuch concretions, as to bigneſs and ſhape, but ſo ſoft, that we could eaſily cruſh and diſcind them betwixt our fingers. And certainly the miſtake of the circumſtance of time has much prejudiced the reputation of many truths: and I remember, that *Aſellius*, to whoſe anatomical fortune the world is ſo much beholden, ingenuouſly acknowledges, that he had like to have loſt the diſcovery of the milky veins, becauſe having at firſt ſuſpected thoſe unlooked-for white veſſels, which he took notice of in the meſentery of a dog diſſected alive, to be ſome irregular ramifications of nerves, he was much confirmed in his conjecture by the next dog he opened; for having diſſected him at an inconvenient diſtance of time from the dog's repaſt, the ſlender veſſels he looked for being deſtitute of the chyle, which is it, that makes them conſpicuous, did not appear. So that he had loſt the benefit of his firſt lucky obſervation, had not his ſagacity inclined him to ſuſpect, that if a dog was plentifully fed at a convenient diſtance of time before his being diſſected, the veſſel ſwelled with alimental juices would be the better diſcernible: whereupon, having feaſted another dog ſome hours before he opened him, he manifeſtly detected thoſe milky veſſels, whoſe diſcovery has ſince ſet anatomiſts ſo uſefully on work.

BUT, *Pyrophilus*, not to exceed the limits of an eſſay, I muſt not multiply inſtances of the contingencies of experiments, but content myſelf to tell you in general, that in divers caſes ſuch circumſtances as are very difficult to be obſerved, or ſeem to be of no concernment to an experiment, may yet have a great influence on the event of it. If on either of the extremes or poles of a good armed load-ſtone, you leiſurely enough, or divers times, draw the back of a knife, which has not before received any magick influence, you may obſerve, that if the point of the blade have in this affriction been drawn from the middle of the æquator of the load-ſtone towards the pole of it, it will attract one of the extremes of an equilibrated magnetick needle; but if you take another knife, that has not been invigorated, and upon the ſelf-ſame extremity or pole of the load-ſtone thruſt the back of the knife from the pole towards the æquator, or middle of the load-ſtone, you ſhall find, that the point of the knife has, by this bare difference of poſition in the blade, whilſt it paſt upon the extreme of the load-ſtone, acquired ſo different a magnetick property, or polarity, from that, which was given to the former knife by the ſame pole of the load-ſtone, that it will not attract, but rather ſeem to repel or drive away that end of the magnetick needle, which was drawn by the point of the other knife. And this improbable experiment not only we have made trial of, by paſſing ſlender irons upon the extremities of armed load-ſtones, the breadth of whoſe ſteel caps may make the experiment ſomewhat leſs ſtrange; but we have likewiſe tried it by affrictions of ſuch irons upon the pole of a naked terella, and we found it to ſucceed there likewiſe: how ſtrange ſoever it may ſeem, that the ſame point or part of the load-ſtone ſhould imbue iron with contrary properties, barely as they are, during their paſſing over it, drawn from the æquator of the load-ſtone, or thruſt towards it. But whether, or how far this obſervation inſinuates the operation of the load-ſtone to be chiefly performed by ſtreams of ſmall particles, which perpetually iſſuing out of one of its poles, do wheel about and re-enter at the other; we ſhall not now examine, (though this ſeem one of the moſt likely phænomena we have met with, to hint a probable magnetical hypotheſis) contenting ourſelves to have manifeſted, by what plainly appears, how much influence a circumſtance, which none but a magnetick philoſopher would take notice of, may have on an experiment. We have alſo, with pleaſure, obſerved, how artificers in the tempering of ſteel, by holding it but a minute or two longer or leſſer in the flame (or other competent heat) do give it very differing tempers, as to brittleneſs or toughneſs, hardneſs or ſoftneſs: for as when it is taken out of the flame to be extinguiſhed, it looks either red, yellow or blue; ſo they eſteem and find it fit to make knives, engraving tools, or ſprings for watches, *&c.* and yet it paſſes from one colour to another ſo ſwiftly, that none but an artiſt expert in tempering of iron would ſuſpect, that ſo ſmall a difference of time of its ſtay in the flame could produce ſo great a difference in its tempers. On which occaſion, *Pyrophilus*, I call to mind, that making a while ſince ſome trials concerning gravers in the ſhop of a famous artificer, he preſented me, as a great rarity, a graver (which I yet keep) that would make the uſual experiments about tempering of gravers appear falſe to him, that ſhould never try them but upon it; for with all the care, wherewith I tried upon it the known ways of ſoftening gravers, I could not ſoften this: which men eminently ſkilled in theſe matters (together with the perſon that made it) affirmed to have been made of *Damaſco*-ſteel, the ſtrength whereof in cutting iron I have (not without ſome wonder) made trial of. But whether this ſingularity, which we have mentioned in this graver, proceeded from the nature of the ſteel, or from the temper, that it had afterward given it, is not yet agreed upon by thoſe ſkilful men, to whom I have ſhewed it: but one of them, who by making inſtruments for navigators, has had the opportunity of making more than ordinary inquiry into matters of this nature, aſſures me, that he can eaſily ſoften this kind of ſteel, by only taking it off the fire at a certain nick of time, differing from that, which is wont to be obſerved in order to the ſoftening of common gravers. And who knows, but that in many other experiments, ſeemingly deſpicable and unheeded circumſtances may be of great concernment, though by reaſon of want of ſuch particular obſervations, as the frequent dealing with the ſame body has given magnetick philoſophers and artificers occaſion to make, men have not yet taken notice of their importance?

To give you one inſtance to this purpoſe, *Pyrophilus*, let me take notice to you, that divers planters of fruit-trees have with wonder obſerved, that ſome grafts of cherry-trees, for example, have borne fruit the ſame year that they were grafted, (nay, I have obſerved ſome plants to bear fruit the ſame quarter of the year) and others not till the year after their inſition, though neither in the goodneſs of the graft, nor in that of the ſtock, they had obſerved any diſparity, to which the difference abovementioned could be aſcribed; and therefore the bearing or not bearing of the cions of a cherry-tree the firſt year of its inſition is by many gardeners looked upon as a thing meerly contingent. And yet indeed it ſcarce deſerves to be reckoned among ſuch contingent experiments, as we have been hitherto treating of; for I am informed by the trials of more than one of the moſt ſkilful and experienced grafters of theſe parts, that a man ſhall ſeldom fail of having cherries borne by his graft the ſame year, in which the inſition is made, if he take care, that his graft, which muſt be of a good kind, have bloſſom-buds, as they are wont to be called, upon it:

whereas if it were only leaf-buds, as they may be termed, it will not bear fruit till the ſecond ſeaſon. And this not being taken notice of by vulgar gardeners, makes them, as we have ſaid, impute a needleſs contingency to the fruitfulneſs of ſuch kind of grafts. Now to diſcern ſuch buds as are fit to produce bloſſoms, from ſuch as will diſplay themſelves but in leaves, is no difficult matter, the former ſort being more full, and big, and round than the latter, which are wont alſo to lie more flat and cloſe to the graft. And it was, *Pyrophilus*, ſuch obſervations as this, that induced us, after the beginning of the former eſſays, to diſcriminate from ſuch contingent experiments as thoſe, wherein the cauſe of the contingency is very abſtruſe and difficult to be diſcerned, ſuch other experiments, whoſe ſeeming contingency proceeds from more eaſily diſcoverable cauſes; for ſuch, by diligent obſervation of circumſtances, may be reduced to a greater certainty than the others ſeem capable of. Though I dare not deny, that even divers of thoſe contingent experiments, which to us yet ſeem to belong to the firſt ſort, by men's future ſkill and diligence in obſervation, may be made fit to be reduced to the ſecond ſort.

BEFORE I leave this ſubject, *Pyrophilus*, I dare not omit to ſay ſomething to you of the *Virgula Divina*, or rather *Divinatoria*, by which many mineraliſts pretend to diſcover the latent veins of metals. Some uſe a forked hazel, whoſe horns they hold by the ends one in each hand; and others content themſelves to chuſe a hazel rod (which ſome will have to be all of the ſame year's ſhoot) and this they bind on to another ſtreight ſtick of any other wood, and walking ſoftly with it over thoſe places, where they ſuſpect the bowels of the earth to be enriched with metals, they ſay, that if they paſs over a metaline vein, the wand will, by bowing towards, diſcover it. And ſome dealers in metals I know, who affirm, that by holding the metals ſucceſſively in that hand, wherein a man holds the rod, he may diſcover what determinate metal is predominant in the vein: for when he puts into his hand that metal, wherewith the mine chiefly abounds, the wand will manifeſtly bow more ſtrongly; that when it is held in the hand with any other metal. What to determine concerning the truth of this perplexing experiment, I confeſs I know not. For *Agricola* himſelf, after a
De re metellica, lib. 20. p. 28. long debate concerning it, gives us this account of his ſenſe; *Metallicus igitur* (ſays he) *quia eum virum bonum & gravem eſſe volumus, virgulâ incantatâ non utetur, quia*
Lib. 3. *part* 4. *cap.* 3. *rerum naturæ peritum & prudentem, furcatam ſibi uſui non eſſe, ſed, ut ſupra dixi, habet naturalia venarum ſigna, quæ obſervat*. The diligent *Kircherus* informs us in his *Arte Magneticâ*, that having exactly tried the experiment with metals, (for he mentions not his having tried it with mines) he could not find it in any meaſure ſucceed; and we ourſelves having ſeveral times made trial of it in the preſence of the confidenteſt aſſertors of the truth of it, could not ſatisfy ourſelves, that the wand did really ſtand either to the metals, when placed under it, or to the metalline veins, when we carried it over mines, whence metalline ore was at that very time digging out. But on the other ſide, divers good authors, and even our diligent country-man *Gabriel Plat*, though wont to be ſomewhat too ſevere to chymiſts, does aſcribe very much to this detecting wand; and divers perſons, in other things very far from credulous, have as eye-witneſſes with great aſſeverations aſſerted the truth of the experiment before us: and one gentleman, who lives near the lead-mines in *Somerſetſhire*, leading me over thoſe parts of the mines, where we knew that metalline veins did run, made me take notice of the ſtooping of the wand, when he paſſed over a vein of ore, and proteſted, that the motion of his hand did not at all contribute to the inclination of the rod, but that ſometimes, when he held it very faſt, it would bend ſo ſtrongly as to break in his hand. And to convince me, that he believed himſelf, he did, upon the promiſes

made him by his ſtooping wand, put himſelf to the great charge of digging in untried places for mines, (but with what ſucceſs he has not yet informed me.) Among the miners themſelves I found ſome made uſe of this wand, and others laughed at it. And this I muſt take notice of, as peculiar to this experiment, that the moſt knowing patrons of it confeſs, that in ſome men's hands it will not at all ſucceed, ſome hidden property in him that uſes the wand being able, as they ſay, to overpower and hinder its inclinatory virtue. To which I muſt add what a very famous chymiſt, who affirms himſelf to have tried many other things with it beſides thoſe that are commonly known, very ſolemnly profeſſed to me upon his own knowledge; namely, that in the hands of thoſe very perſons, in whoſe hands the rod will (as they ſpeak) work, there are certain unlucky hours, governed by ſuch planets and conſtellations, (which I confeſs I believed not enough to remember their names) during which it will not work, even in thoſe hands, wherein at other times it manifeſtly will. But of this experiment I muſt content myſelf to ſay, what I am wont to do, when my opinion is aſked of thoſe things, which I dare not peremptorily reject, and yet am not convinced of; namely, that they that have ſeen them can much more reaſonably believe them, than they that have not.

Nor is it only in experiments, *Pyrophilus*, but in obſervations alſo, that much of contingency may be: witneſs the great variety in the number, magnitude, poſition, figure, *&c.* of the parts taken notice of by anatomical writers in their diſſections of that one ſubject the human body, about which many errors would have been delivered by anatomiſts, if the frequency of diſſections had not enabled them to diſcern betwixt thoſe things, that are generally and uniformly found in diſſected bodies, and thoſe which are but rarely, and (if I may ſo ſpeak) through ſome wantonneſs, or other deviation of nature, to be met with I remember, that a while ſince being preſent at the diſſection of a luſty young thief, we had opportunity to obſerve, among other things, that the interval betwixt two of his ribs was near the back-bone filled up with a thick bony ſubſtance, which ſeemed to be but an expanſion of the ribs, and appeared not to have grown there upon occaſion of any fracture, or other miſchance. About the ſame time being at a private diſſection of a large and young human body with ſome learned men, an ingenious perſon, profeſſor of anatomy, there preſent, chancing to cut a great nerve, ſpied in the ſubſtance of it a little of a very red liquor, which he immediately ſhewed me, as wondering what it might be: but I concluding it to be blood, preſently ſuſpected that it might have proceeded from ſome ſmall unheeded drop of blood wiped off by the bruſhy ſubſtance of the nerve from the knife wherewith it was cut. Wherefore carefully wiping a diſſecting knife, I did in another place cut the nerve aſunder, and found another very little drop of pure blood in the ſubſtance of it as before. This I did again elſewhere with like ſucceſs, ſhewing it to the by-ſtanders, who admired to ſee a veſſel carrying blood (for ſuch they concluded it to be) in the body of a nerve, in regard they remembered not to have ever met with ſuch an accident; though I the leſs admire it, becauſe I have in an ox's eye or two obſerved in that coat, which the moderns commonly call the retina, and which ſeems to be but an expanſion of the pith of the optick nerve, little turgent veins manifeſtly full of blood.

We further obſerved in that lately mentioned body, in which we took notice of the irregular conjunction of two ribs, that the lungs, which were very ſound, had a ſupernumerary lobe on one ſide, which did ſo little differ from its companions, that we did not, till we had diſplayed the lungs, take notice of it. And I remember, that a while before, being invited by a company of phyſicians to a private diſſection, and the lungs, which otherwiſe ſeemed not unſound, appearing in divers places faſtened to the ribs, two ingenious anatomiſts, that were there preſent, did ſo little agree in their obſervations concerning ſuch caſes, that the one affirmed, that he had never ſeen any lungs (which had not been exceſſively morbid) tied to the thorax; and the other proteſted, that he had ſcarce ever opened a diſeaſed body, wherein the lungs did not ſo adhere. But if it were not improper to mind a young gentleman of venereal obſervations, I could eaſily give you an eminent proof of the diſagreement of anatomical obſervators, by inſiſting on the controverſy betwixt the famous writers on that ſubject, concerning the anatomical notes or tokens of virginity; many eminent authors affirming, that they have ſeldom failed of finding them in one amongſt very many diſſected maids; and many other artiſts, both conſpicuous and experienced, preremptorily profeſſing, that they have ſeldom or never met with the pretended marks in perſons even of the moſt undoubted virginity. And certainly it is very ſtrange, that about a matter, which ſeems ſo eaſily determinable by ſenſe, accurate and ſober men ſhould ſo widely diſagree; as that the one ſhould profeſs he has very rarely, if ever, met with in a human body, what another affirmeth himſelf to have as ſeldom, if ever, miſſed. But becauſe, *Pyrophilus*, this ſubject is, perhaps, ſomewhat improper to be inſiſted on either to, or by, a young man, I ſhall paſs on to tell you, that amongſt the accurateſt of our modern writers, I ſuppoſe you will readily allow me to reckon Dr. *Harvey* and Dr. *Highmore*; and that though in their excellent treatiſes of generation they both inſiſt on the production and changes obſervable in hens eggs, as the patterns, whereunto the generation of other annimals may be referred; yet have we divers times, in the progreſs of nature in her formation of a chick, obſerved conſiderable variations in point of time and other circumſtances (though in the main our obſervations commonly agreed) from what is by them delivered: which diverſity may eaſily proceed from the different conſtitution of hens, their differing aſſiduity in ſitting on their eggs, the differing qualifications of the eggs themſelves, and ſeveral other particulars of the like nature. And I remember, that the other day taking notice of this to my learned friend Dr. *Highmore*, he readily acknowledged to me, that he himſelf had likewiſe obſerved divers circumſtances in eggs whilſt they were hatching, which varied from thoſe ſet down by him in his book; though he had there accurately expreſſed the changes he diſcerned in thoſe eggs, which at the ſame time afforded him his obſervations. And indeed there are certain things of ſuch a nature, that ſcarce any ſingle man's accurateneſs in making a ſingle obſervation about them can ſecure him from appearing unſkilful or unfaithful in his obſervations, unleſs thoſe, that ſhall afterwards examine them, chance to be endowed with a ſomewhat more than ordinary either equity, or ſagacity, or both. For inſtance, he that firſt affirmed, that a needle animated by a loadſtone did conſtantly convert its extremes to the oppoſite poles of the earth, could ſcarce ſuſpect himſelf of having delivered any thing, which he had not carefully tried. And yet of thoſe pilots, *Gonzales Oviedo* and *Sebaſtian Cabot*, (who are ſaid to have in *America* firſt taken notice of the declination of the mariner's needle) he that did firſt in thoſe far diſtant parts of the world compare the meridian line afforded by magnetical needles with one mathematically drawn, (which may be readily found by accurate ſun-dials) and thereby obſerve the variation of the needle, or its declination from the true meridian line, might eaſily conclude the obſerver formerly mentioned to have been faulty, by reaſon of his finding the needle's variation differing (perhaps by divers degrees) from that delivered by the firſt obſerver. And this ſecond man's obſervation might appear to have been as careleſly made to a hundred other obſervers, if the obſervations of navigators had not made it apparent, that the declination of the needle is far from being the ſame in all places: for though *Cardan* (as *Kircher* and *Fracaſtorius*, as another informs us) be pleaſed to affirm, that the loadſtone declines as many degrees, as the pole-ſtar is diſtant from the pole of the world; yet beſides divers reaſons, common experience ſufficiently manifeſts the inconſiderableneſs (not to ſpeak more harſhly) of that aſſertion. *Fournier Hydr. l.* 11. *c.* 11. For about the iſlands of the *Azores*, eſpecially that of *Corvo*, over which the firſt meridian is by many ſuppoſed to paſs, the magnetick needle hath been obſerved directly to reſpect the poles, without any ſenſible declination from them; but in other places it is wont to vary ſometimes eaſtward, ſometimes weſtward, more or leſs. Inſomuch that not only our venturous countryman Captain *Thomas James* obſerved it in 63 degrees north-latitude to be no leſs than 27 degrees, 48 minutes; *In the Table annexed to his voyage.* but a learned mathematical writer, that is lately come forth, makes the declination at the *Fretum Davis* to amount to, what is almoſt incredible, 50 degrees. And this deflexion of the needle ſometimes to one ſide of the meridian, ſometime to the other, happens with ſo much ſeeming irregularity, as has made both the diligent *Kircher* himſelf, and divers other magnetick writers, almoſt deſpair of reducing theſe kind of obſervations to any general hypotheſis.

To which we may add, that perhaps very few even of the exacteſt obſervations of this nature made an age ſince, would now appear accurate to them, that ſhould try them in the ſelf-ſame places wherein, and the ſelf-ſame manner after which they were formerly made. So that the diligenteſt of thoſe obſervers would appear to us to have been negligent, if the ſagacity of ſome of their ſucceeders had not prompted them to ſuſpect, that even in the ſame place the needle's variation may vary. And I remember, that having not long ſince enquired of an Engliſh contriver of mathematical inſtruments for the uſe of ſeamen, what he had obſerved concerning this alteration of the needle's variation, he told me, that by comparing of ancient and modern obſervations made by himſelf and other accurate mathematicians at *London*, he had found the declination conſtantly to decreaſe, and, as he conjectured, about 12 or 13 minutes (though that methinks be much) in a year. And it will be yet more difficult to ſet down any obſervation of this nature, which will appear exact to poſterity, if that ſtrange thing be true (as it may well be) which was related to *Kircher* by a friend of his, who affirms himſelf to have obſerved a notable change of the needle's variation at *Naples*, after a great *incendium* of the neighbouring mountain *Veſuvius*; which alteration he not abſurdly ſuſpects to have proceeded from the very great change made in the neighbouring ſubterraneal parts by that great conflagration. And it ſeems the ſame obſervation has been taken notice of by mathematicians elſewhere. For the learned Jeſuit *Fournier* in his French hydrography tells us in more general terms, that ſince the *incendiums* of *Veſuvius* the declination (of the needle) has notably changed in the kingdom of *Naples*. *Livre* 11. *chap.* 10. The ſame author ſomewhere delivers what (if it be true) is remarkable to our preſent purpoſe, in theſe words: 'There are perſons, who have obſerved, that the ſame needle, that declined 5 'degrees upon the ſurface of the earth, being carried down very low into certain 'caves, declined quite otherwiſe.' I added thoſe words, *if it be true*, not to queſtion the veracity of the author, but becauſe it is very poſſible the makers of the obſervation (though learned men) may have been miſtaken in it, without ſuſpecting themſelves in danger of being ſo. For I ſhould ſcarce have imagined, unleſs my own particular obſervation had informed me, in how great a variety of ſtones and other foſſiles the ore of iron may lurk diſguiſed: ſo that it is no way incredible, that knowing chymiſts themſelves, and much more mathematicians and others, not being aware of the obſervation of what I have newly delivered, may preſume, becauſe they ſaw not in the deep caves abovementioned any minerals like the vulgar iron ore, that there is nothing of that metal there, when indeed there may be enough to occaſion that deflexion of the needle; which (eſpecially if it be ſtrongly excited) may be often drawn aſide by iron or other magnetick bodies, at a greater diſtance than thoſe, that have not tried, will be apt to ſuſpect. Which may perhaps be the reaſon, why in the little iſland of *Ilva* (upon the coaſt of *Italy)* where they dig up iron and ſtore of loadſtones, of which I have ſeen in *Tuſcany* of a prodigious bigneſs, there is indifferent, but neighbouring places, ſuch a ſtrange diſparity of the needle's variation as curious men have recorded.

NOR are magnetical and anatomical obſervations the only ones, which are ſubject to diſagree now and then, without the negligence of thoſe that make them: but I want time, and I fear you would want patience, to conſider at preſent as many of them, as might be eaſily enumerated to you.

I SUPPOSE, *Pyrophilus*, you may have obſerved, how I in the paſt diſcourſe have forborn to inſiſt on medicinal experiments; which I have purpoſely done, becauſe they are ſo many, and almoſt all of them ſubject to ſuch uncertainties, that to inſiſt on them would require much more time, than my occaſions will allow me to ſpend upon this eſſay. And indeed in phyſick it is much more difficult than moſt men can imagine, to make an accurate experiment: for oftentimes the ſame diſeaſe proceeding in ſeveral perſons from quite differing cauſes, will be increaſed in one by the ſame remedy by which it has been cured in another. And not only the conſtitutions of patients may as much alter the effects of remedies, as the cauſes of diſeaſes; but even in the ſame patient, and the ſame diſeaſe, the ſingle circumſtance of time may have almoſt as great an operation upon the ſucceſs of a medicine, as either of the two former particulars; as we may elſewhere have occaſion by ſundry inſtances to manifeſt. But beſides the general uncertainty, to which moſt remedies are ſubject, there are ſome few, that ſeem obnoxious to contingencies of a peculiar nature: ſuch is the ſympathetick powder, of which not only divers phyſicians and other ſober perſons have aſſured me they had ſucceſsfully made trial, but we ourſelves have thought, that we were eye-witneſſes of the operation of it; and yet not only many, that have tried it, have not found it anſwer expectation, but we ourſelves trying ſome of our own preparing on ourſelves, have found it ineffectual, and unable to ſtop ſo much as a bleeding at the noſe; though upon application of it a little before, we had ſeen ſuch a bleeding, though violent, ſuddenly ſtopped in a perſon, who was ſo far from contributing by his imagination to the effect of the powder, that he derided thoſe that he ſaw apply it to ſome of the drops of his blood. Wherefore that the ſympathetick powder and the weapon-ſalve are never of any efficacy at all, I dare not affirm; but that they conſtantly perform what is promiſed of them, I muſt leave others to believe. But making mention of remedies of this nature, though I am willing, *Pyrophilus*, to put a period both to your trouble and my own, yet I muſt not omit to tell you, that whereas the pæony-root has been much commended both by ancient and modern phyſicians of no mean account, as an amulet againſt the falling ſickneſs, and yet has been by many found ineffectual; we have been apt to ſuſpect, that its inefficacy, if it be but infrequent, might poſſibly proceed from its having been unſeaſonably gathered: and when I was laſt in the weſt of *Ireland*, acquainting the eminenteſt of the Galeniſts there with my conjecture, he confirmed me in it, by aſſuring me, that he had often tried the pæony root unſeaſonably gathered without ſucceſs; but having lately gathered it under its proper conſtellation, as they ſpeak, (which is when the decreaſing moon paſſes under *Aries)* and tied the ſlit root about the necks and arms of his patients, he had freed more than one, whom he named to me, from epileptical fits. Agreeable whereunto I find, that a famous phyſician of *Grenoble*, Monſieur *des Grands Prez*, in the laſt of his obſervations communicated to the famous practical phyſician *Riverius*, ſolemnly profeſſes his having divers times freed his patients from the falling-ſickneſs by the ſingle outward application of pæony-roots, collected and applied as is above mentioned. But though he thence infers the uſefulneſs of obſerving ſtars in the practice of phyſic, yet before much weight be laid upon ſuch improbable notions, as moſt of thoſe of judiciary aſtrologers, the influence of conſtellations upon ſimples, *&c.* ought by ſevere and competent experiments to be better made out than hitherto it has been.

BUT to ſay no more of the contingent obſervations to be taken notice of in trials medical, I could tell you, that I have obſerved even mathematical writers themſelves to deliver ſuch obſervations as do not regularly hold true. For though it hath been looked upon as their privilege and glory to affirm nothing, but what they can prove by no leſs than demonſtration; and though they uſed to be more attentive and exact, than moſt other men in making almoſt any kind of philoſophical obſervation: yet the certainty and accurateneſs, which is attributed to what they deliver, muſt be reſtrained to what they teach concerning thoſe purely-mathematical diſciplines, arithmetic and geometry, where the affections of quantity are abſtractedly conſidered: but we muſt not expect from mathematicians the ſame accurateneſs, when they deliver obſervations concerning ſuch things, wherein it is not only quantity and figure, but matter, and its other affections, that muſt be conſidered. And yet leſs muſt this be expected, when they deliver ſuch obſervations, as, being made by the help of material inſtruments framed by the hands and tools of men, cannot but in divers caſes be ſubject to ſome, if not many, imperfections upon their account. Divers of the modern aſtronomers have ſo written of the ſpots and more ſhining parts or (as they call them) *Faculæ*, that appear upon or about the ſurface of the ſun, as to make their readers preſume, that at leaſt ſome of them are almoſt always to be ſeen there. And I am willing to think, that it was their having ſo often met with ſuch phænomena in the ſun, that made them write as they did. And yet when I firſt applied myſelf to the contemplation of theſe late diſcoveries, though I wanted neither good teleſcopes, nor a dark room to bring the ſpecies of the ſun into, yet it was not till after a great while, and a multitude of fruitleſs obſervations made at ſeveral times, that I could detect any of theſe ſolary ſpots, which having during many months at leaſt appeared ſo much ſeldomer than it ſeems they did before, that I remember a moſt ingenious profeſſor of aſtronomy, excellently well furniſhed with dioptrical glaſſes, did about that time complain to me, that for I know not how long he had not been able to ſee the ſun ſpotted. And as for the *Faculæ*, that are written of, as ſuch ordinary phænomena, I muſt profeſs to you, *Pyrophilus*, that a multitude of obſervations made with good teleſcopes, at ſeveral places and times whilſt the ſun was ſpotted, has ſcarce made me ſee above once any of the looked for brightneſſes.

AND as the nature of the material objects, wherewith the mathematician is converſant, may thus deceive the expectations grounded on what he delivers; ſo may the like happen by reaſon of the imperfection of the inſtruments, which he muſt make uſe of in the ſenſible obſervations, whereon the mixed mathematics (as aſtronomy, geography, optics, *&c.*) are in great part built. This is but too manifeſt in the diſagreeing ſupputations, that famous writers, as well modern as antient, have given us of the circuit of the terreſtrial globe, of the diſtance and bigneſs of the fixed ſtars and ſome of the planets, nay, and of the height of mountains: which diſagreement, as it may oftentimes proceed from the differing method and unequal ſkill of the ſeveral obſervers, ſo it may in divers caſes be imputed to the greater or leſs exactneſs and manageableneſs of the inſtruments employed by them. And on this occaſion I cannot omit that ſober confeſſion and advertiſement, that I met with in the noble *Tycho*, who having laid out, beſides his time and induſtry, much greater ſums of money on inſtruments than any man we have heard of in latter times, deſerves to be liſtened to on this theme, concerning which he has (among other things) the following paſſage: *Facilè* (ſays he) *lapſus aliquis penè inſenſibilis in inſtrumentis etiam majoribus conficiendis ſubrepit, qui inter obſervandum aliquot ſcrupulorum primorum jacturam faciat; inſuper ſi ipſe ſitus & tractandi modus non tam abſoluta norma perficiatur, ut nihil prorſus deſideretur, intolerabilis nec facilè animadvertenda deviatio ſeſe inſinuat. Adde quod inſtrumenta uſu & ætate à prima perfectione degenerent. Nihil enim, quod hominum manibus paratur, ab omni mutatione undiquaque exiſtit. Organa enim ejuſcemodi, niſi è ſolido metallo affabre elaborentur, mutationi aëreæ obnoxia ſunt; & ſi id quoque detur, ut è metallica materia conſtent, niſi ingentia fuerint, diviſiones minutiſſimas graduum non ſufficientur exhibent; dumque hoc præſtant, ſua magnitudine & pondere ſe ipſa ita aggravant, ut facile tum extra planum debitum aut figuram competentem, dum circumducuntur, declinent, tum etiam ſua mole intractabilia redduntur. Quare magis requiritur in inſtrumentis aſtronomicis, quæ omni vitio careant conſtruendis, artificium pari judicio conjunctum, quam hactenus à quamplurimis animadverſum eſt. Id quod nos ipſe uſus longaque docuit experientia, non parvo labore nec mediocribus ſumptibus comparata.*

Tycho Brahe, *l.* 2. *de Cometâ An.* 1577. *p.* 153.

HITHERTO our noble author. And as for the obſervations made at ſea, the diligent *Fournier* advertiſes, that however many ſea-captains and others may brag of their mathematical obſervations made on ſhip-board, yet he, upon trial of many inſtruments both at ſea and aſhore, makes bold to affirm, that no aſtronomer in the world can be ſure to make his obſervation at ſea within ten minutes of the preciſe truth, no not (ſays he) upon the ſand itſelf, within one minute of it.

BUT inſtead of acquainting you with what may be drawn from the writings of our hydrographer, to prove, that his aſſertion is rather modeſt than too bold, I ſhall obſerve, that the obſervations even of ſkilful mathematicians may hold ſo little, or diſagree ſo much, when they pretend to give us the determinate meaſures of things, that I remember of three very eminent modern mathematicians, who have taken upon them, by their experiments, to determine the proportion betwixt air and water, the one makes not the weight of water to exceed above 150 times that of air; the other reckons water to be between 13 and 14 hundred times; and the third no leſs than 10,000 times the heavier. Not to mention a modern and famous writer or two, who have been ſo miſtaken as to think, that the weight of the water in compariſon of the air is I know not how much under-reckoned, even by this laſt (overbold) eſtimate. And, if I had leiſure, I could annex an experiment partly ſtatical, and relating to the weight of the air, which though we made divers times in an hour, yet we miſſed of the like ſucceſs twice as often in the ſame hour, without being able to know beforehand, whether the experiment would ſucceed within ſome pounds weight. But of this more perhaps elſewhere.

THE ends, *Pyrophilus*, which we have propoſed to ourſelves in ſetting down the things by us delivered in this and the former eſſay, are principally two.

AND firſt, we deſire, that the inſtances we have given you of the contingency of experiments, may make you think yourſelf obliged to try thoſe experiments very carefully, and more than once, upon which you mean to build conſiderable ſuperſtructures either theorical or practical; and to think it unſafe to rely too much upon ſingle experiments, eſpecially when you have to deal in minerals: for many to their ruin have found, that what they at firſt looked upon as a happy mineral experiment, has proved in the iſſue the moſt unfortunate they ever made. And I remember, that the moſt experienced mineraliſt I have hitherto been acquainted with, though his ſkill has been rather gainful then prejudicial to him, has very ſeriouſly told me, that he could quickly grow an extraordinary rich man, if he could but do conſtantly whatſoever he has done, not only two or three, but many times.

THE other end, *Pyrophilus*, to which I had an eye in writing the paſt diſcourſes, was, that they may ſerve for a kind of apology for ſober and experimental writers, in caſe you ſhould not always upon trial find the experiments or obſervations by them delivered anſwer your expectations. And indeed it would prove a great diſcouragement to wary and conſiderate naturaliſts from enriching the world with their obſervations, if they ſhould find, that their faithfulneſs in ſetting down what they obſerved is not able to protect them from blaſting imputations of falſhood, but that by publiſhing any thing for the good of others, they muſt expoſe their reputation to all the uncertainties, to which any of their experiments may chance to prove obnoxious. It is true indeed, that if a writer be wont to be fabulous or tranſcriptive, and to deliver things confidently by hear-ſay, without telling his readers when he does ſo; if his experiments upon trial ſucceed not, we may be allowed to impute their unſucceſsfulneſs rather to him, than to ourſelves, or to chance, and need not think ourſelves obliged to have ſo much a greater care of his reputation, than he had of his own, as for his ſake to try more than once, what he for our ſakes never tried ſo much as once. But if an author, that is wont to deliver things upon his own knowledge, and ſhews himſelf careful not to be deceived, and unwilling to deceive his readers, ſhall deliver any thing, as having tried or ſeen it, which yet agrees not with our trials of it; I think it but a piece of equity, becoming both a Chriſtian and a philoſopher, to think (unleſs we have ſome manifeſt reaſon to the contrary) that he ſet down his experiment or obſervation as he made it, though for ſome latent reaſon it does not conſtantly hold; and that therefore though his experiment be not to be relied upon, yet his ſincerity is not to be rejected. Nay, if the author be ſuch an one, as has intentionally and really deſerved well of mankind, for my part I can be ſo grateful to him, as not only to forbear to diſtruſt his veracity, as if he had not done or ſeen what he ſays he did or ſaw, but to forbear to reject his experiments, till I have tried, whether or no by ſome change of circumſtances they may not be brought to ſucceed. Thus a while ſince finding in Sir *Francis Bacon*, that he delivers as a ſomewhat unlikely truth, that ſpirit of wine will ſwim upon oil (of almonds) we forthwith made trial of it, but found the oil ſwim upon the ſpirit of wine, and this upon ſeveral trials before witneſſes: but our tenderneſs of the reputation of ſo great and ſo candid a philoſopher made us to bethink ourſelves, that (though he mentions it not, nor perhaps thought of any ſuch thing, yet) poſſibly he may have uſed ſpirit of wine more pure than ordinary; and thereupon having provided ſome that was well rectified, we found, that the oil, that was wont to ſwim upon ſpirit of wine, not freed from its aqueous parts, did readily ſink, and quietly lie in the bottom of that, which was carefully dephlegmed. And ſo having been informed, that the learned Dr. *Brown* ſomewhere delivers, that aqua fortis will quickly coagulate common oil, we poured ſome of thoſe liquors together, and let them ſtand for a conſiderable ſpace of time in an open veſſel, without finding in the oil the change by him promiſed (though we have more than once with another liquor preſently thickened common oil). Whereupon being unwilling, that ſo faithful and candid a naturaliſt ſhould appear fit to be diſtruſted, we did again make the trial with freſh oil and aqua fortis in a long-necked phial left open at the top, which we kept both in a cool place, and after in a digeſting furnace; but after ſome weeks we found no other alteration in the oil, than that it had acquired a high and lovely tincture: notwithſtanding which, being ſtill concerned for the reputation of a perſon, that ſo well deſerves a good one, the like contingencies we have formerly met with in other experiments, made us willing to try, whether or no the unſucceſsfulneſs we have related might not proceed from ſome peculiar though latent quality, either in the aqua fortis or the oil by us formerly employed: whereupon changing thoſe liquors, and repeating the experiment, we found after ſome hours the oil coagulated almoſt into the form of a whitiſh butter. Nor dare I allow myſelf to be confident, that I ſhall not need to be dealt with by you upon ſome occaſions, with the like equity, that I have been careful to expreſs towards others. And ſince the writing of thus much of this very eſſay, having deſired a a very ſkilful and candid chymiſt to do me the favour to provide me ſome of the pureſt and ſtrongeſt ſpirit of ſalt, that could be made; he kept ſome ſalt in a vehement fire for divers days and nights together, and freed the extracted liquor ſo carefully and ſo ſkilfully both from its phlegm and its terreſtrial fæces, that after all I have written in the former eſſay concerning that menſtruum, I muſt freely confeſs to you, that I am now ſatisfied, that a ſpirit of ſea-ſalt may without any unſincerity be ſo prepared, as to diſſolve the body of crude gold, though I could not find, that the ſolutions I made of that metal were red, but rather of a yellow or golden colour, much like thoſe made with common aqua regis. But neither this artiſt nor I have been ſince able to make another ſpirit of ſalt capable of diſſolving gold, notwithſtanding all the induſtry we have employed about it; which makes me refer this to contingent experiments; unleſs the proſperous event of our former trial may be aſcribed to the quality of the ſalt, that was diſtilled, which was brought from the iſland of *Mayo*, where the ſcorching ſun makes out of the ſea-water a ſalt, that is accounted much ſtronger and more ſpirituous than that, which is wont to be made in *France* and other more temperate climates. And let me, *Pyrophilus*, take this opportunity to add, that if I had not very cautiouſly ſet down the obſervation I related in another eſſay * concerning the little fiſhes or worms I there teach you to diſcover in vinegar, I ſhould perhaps need much of your equity, to keep me from being thought to have impoſed upon you in what I there delivered. For I have ſince met with divers parcels of vinegar, wherein the obſervation could not be made, for one wherein it held; ſo that I am glad to keep by me ſome vinegar ſtocked with thoſe ſcarce viſible animals, to ſatisfy ingenious men, among whom ſome have been fain, after their own fruitleſs trials, to come to me to ſhow them the things delivered in that obſervation. What I mentioned a little above to have been tried upon ſallet-oil, puts me in mind of telling you, that among our experiments concerning the changes of colours, we were about to acquaint you with one, which we had formerly made upon common oil-olive, it ſeeming to us a not inconſiderable one; ſince it was a way, that we deviſed of inſtantly changing the colour of the oil from a pale yellow to a deep red, with a few drops of a liquor, that was not red, but almoſt colourleſs. This experiment, as we were ſaying, *Pyrophilus*, we were about to ſet down among others concerning colours: but becauſe we do not willingly rely on a ſingle trial of ſuch things, as we know not to have been ever tried before, we thought it not amiſs for greater ſecurity to make the experiment the ſecond time, but could not then find it to ſucceed, nor even ſince upon a new trial (probably by reaſon of ſome peculiar quality in that particular parcel of liquor we firſt made uſe of) which made us think fit to omit the intended mention of it: but if I had upon my firſt trial acquainted you with it without any further ſcruple, you might upon trial have ſuſpected, if not concluded, that I had miſinformed you, though I had really delivered nothing but what I had tried. And indeed, *Pyrophilus*, though I have not the vanity to pretend to have deſerved ſo much of you, as ſuch naturaliſts as Sir *Francis Bacon* have deſerved from every ingenious reader of their books; yet perhaps you will do me but right to believe, that though ſome of the experiments I have delivered may prove contingent, yet I have not delivered them unfaithfully, in reference to what I thought I obſerved in them, and remembered of them. And though I deſire you ſhould ſo read my writings, as to give no farther aſſent to my opinions, than the reaſons or experiments produced on their behalf require; yet in matters of fact, which I deliver as having tried or ſeen them, I am very willing you ſhould think, that I may have had the weakneſs to be miſtaken, but not an intention to deceive you.

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* This is one of thoſe, that make up the book of the uſefulneſs of *Experimental Philoſophy*.

THERE is yet one thing more, that I ſhall venture to acquaint you with before I conclude this eſſay, though you may think it reliſhes of a paradox, and it is this: that when I am ſatisfied of the abilities and circumſpection of a writer, delivering a matter of fact as upon his own knowledge; I do not preſently reject his obſervation as untrue, much leſs condemn the perſon himſelf as a lyar, whenſoever I find, that it ſeems to be contradicted by a contrary and more undoubted obſervation, or to contradict a received and plauſible either hypotheſis or tradition; but rather try, if by fit diſtinction or limitation I can reconcile them; unleſs I can imagine ſomething or other, which might probably lead him to miſtake. And of this indulgence to an intelligent writer I have this reaſon to give, that ſometimes there happen irregularities contrary to the uſual courſe of things, as is evident in monſters; and ſometimes the received hypotheſis, though perhaps not to be rejected as to the main, will not hold ſo univerſally as men preſume; and ſometimes too the contradiction betwixt the obſervations may be but ſeeming (by reaſon of the want of ſome unheeded circumſtance neceſſary to make them inconſiſtent) and ſo they may both be true.

WE might dilucidate and confirm what we have newly delivered by ſeveral inſtances, were it not, that this eſſay is already but too prolix. Wherefore we ſhall only recommend to your conſideration theſe few particulars.

THAT the Iriſh ſpiders (of which, whatever is vulgarly believed to the contrary, myſelf have in *Ireland* ſeen divers) are not poiſonous, is not doubted by the inhabitants, who have had many ages experience of their harmleſſneſs: and yet I dare not deny what the learned *Scaliger* ſomewhere affirms, that in (his country, if I miſ-remember not) *Gaſcony* their venom is ſo pernicious, that they ſometimes poiſon thoſe that tread upon them through the very ſoles of their ſhoes. And that even here in *England* (though a country ſo near to *Ireland*) ſome ſpiders (at leaſt) are venomous even without biting, I may elſewhere have occaſion to give you an experimental proof.

It is ſo much taken for granted by divers authors, who pretend likewiſe to give reaſons of it, and by the generality of their readers, that under the ſame meridian the magnetic needle keeps every where the ſame variation, without changing it by being carried northwards or ſouthwards, that it is like, if many perſons better acquainted with magnetic ſpeculations than trials ſhould read in the relations of the Hollanders, that under the meridian, that paſſes by the iſland of *Corvo*, where the needle points directly at the poles, and which is therefore wont to be reckoned the firſt meridian, they found at two places, the one about 46, the other about 55 degrees of northern latitude, a declination in the former of thoſe elevations of no leſs than 7 or 8 degrees, and in the latter of a far greater number; and alſo that they found under the twentieth parallel of ſouthern latitude under the ſame meridian of the *Azores* 10 or 11 degrees of declination; many, I ſay, if they ſhould meet with theſe particulars, probably would ſuppoſe the Dutch to have been very bad obſervers, becauſe theſe obſervations do not (as we intimated above) agree with the theory of the needle's declination. And yet if we confer theſe obſervations with others of the like nature, made by good navigators and other ſkilful men along other meridians, we may, I ſuppoſe, find cauſe rather to rectify the general opinion, than reject the Dutch obſervations for their diſagreeing with it; eſpecially if we take into conſideration what is affirmed by the Jeſuit *Jules Alenis* (whom *Fournier*, amply treating of longitudes, extols for the accurateſt obſerver of the needle's variation that ever was) ſailing into *China* in a great Portugal carraque, and accompanied by the famous pilot *Vincent Rodrique*, who had then made twenty-eight voyages to the *Indies*. For out of one of this father's letters *Fournier* has preſerved this memorable paſſage: 'You muſt '(ſays he) take notice of one thing very conſiderable, namely, that the further you 'go from the æquator in the ſame meridian, the greater you will find the magnetical 'variation.' There are ſome eminent modern naturaliſts, who affirm, that they have aſſuredly tried by weather-glaſſes, that cellars and other ſubterraneal places are colder in winter than in ſummer: and yet not to oppoſe to this experiment the common tradition to the contrary, I remember, that the bold and induſtrious Capt. *James* (formerly mentioned) in the relation of his ſtrange voyage publiſhed by his late Majeſty's command, has this notable obſervation, where he relates the exceſſive coldneſs of the water they met with in ſummer in that icy region, where they were forced to winter in the year 1632. 'Moreover our well (ſays he) out of which we had 'water in *December*, had none in *July*.'

De la Longitude, c. 34.

LASTLY, though in the weſtern parts it have been obſerved, that generally the inſide, or heart, as they call it, of trees, is harder than the outward parts; yet an author, very well verſed in ſuch matters, treating of the building of ſhips, gives it us for a very important advertiſement touching that matter, that they have obſerved at *Marſeilles*. and all along the Levantine ſhores, that that part of the wood, that is next the bark, is ſtronger than that, which makes the heart of the tree. But to draw at length to a concluſion of this already too tedious eſſay; the ends above mentioned, *Pyrophilus*, being thoſe, which I propoſed to myſelf in writing the paſt diſcourſe, you will make an uſe of it, which I was very far from intending you ſhould, if you ſuffer it to diſcourage you from the vigorous proſecution of your inquiries into experimental knowledge. Nor indeed is any thing, that hath been ſaid, fit to perſuade you to other than watchfulneſs in obſerving experiments, and warineſs in relying on them; but not at all to ſuch a deſpondency of mind, as may make you forbear the proſecution of them: for neither doth the phyſician renounce his profeſſion, becauſe divers of the patients he ſtrives to cure are not freed from their diſeaſes by his medicines, but by death; nor doth the painful huſbandman forſake his cultivating of the ground, though ſometimes an unſeaſonable ſtorm or flood ſpoils his harveſt, and deprives him of the expected fruit of his long toils. For as in phyſic and huſbandry, thoſe, that exerciſe them, are kept from deſerting their profeſſions, by finding, that though they ſometimes miſs of their ends, yet they oftentimes attain them, and are by their ſucceſſes requited not only for thoſe endeavours that ſucceed,

Fourn. Archeteĉture Navale, c. 22.

but for thoſe that were loſt; ſo ought we not by the contingencies incident to experimental attempts, to be deterred from making them, becauſe not only there are many experiments ſcarce ever obnoxious to caſualties, but even among thoſe, whoſe event is not ſo certain, you may very probably make an experiment very often, without meeting with any of thoſe unlucky accidents, which have the power to make ſuch experiments miſcarry. And ſure the proſperous ſucceſs of many ſucceeding attempts is well able to make amends for the fruitleſs pains employed on thoſe few, that ſucceed not; eſpecially ſince in experiments it not frequently happens, that even when we find not what we ſeek, we find ſomething as well worth ſeeking as what we miſſed. Of this laſt mentioned truth we may elſewhere have occaſion to diſcourſe more largely; and therefore ſhall now conclude with barely minding you, that even merchants themſelves are not wont to quit their profeſſion, becauſe now and then they loſe a veſſel at ſea, and oft-times their ſhips are by contrary winds and other accidents forced to put in at other ports than thoſe they were bound for. Which example I the rather make uſe of, becauſe that as the American navigators employed by the European merchants, having been by ſtorms forced from their intended courſe, have been ſometimes thereby driven upon unknown coaſts, and have made diſcovery of new regions much more advantageous to them, than the faireſt and conſtanteſt winds and weather could have been; ſo in philoſophical trials, thoſe unexpected accidents, that defeat our endeavours, do ſometimes caſt us upon new diſcoveries of much greater advantage, than the wonted and expected ſucceſs of the attempted experiment would have proved to us.

SOME
SPECIMENS
OF
An Attempt to make CHYMICAL EXPERIMENTS uſeful to illuſtrate the notions of the CORPUSCULAR PHILOSOPHY.

The PREFACE,

Giving an account of the two following treatiſes, and propoſing the deſirableneſs of a good intelligence betwixt the Corpuſcularian Philoſophers and the Chymiſts.

THERE are many learned men, who being acquainted with chymiſtry but by report, have from the illiterateneſs, the arrogance and the impoſtures of two many of thoſe, that pretend ſkill in it, taken occaſion to entertain ſo ill an opinion, as well of the art, as of thoſe that profeſs it, that they are apt to repine, when they ſee any perſon, capable of ſucceeding in the ſtudy of ſolid philoſophy, addict himſelf to an art they judge ſo much below a philoſopher, and ſo unſerviceable to him: nay, there are ſome, that are troubled when they ſee a man, acquainted with other learning, countenance by his example ſooty empirics, and a ſtudy, which they ſcarce think fit for any but ſuch, as are unfit for the rational and uſeful parts of phyſiology. I now take notice of theſe things, becauſe they gave occaſion to the two following treatiſes. For perceiving divers years ago, that ſome learned men of the temper above deſcribed thought it ſtrange (if not amiſs alſo) that one, of whoſe ſtudies they were pleaſed to have too favourable an expectation, ſhould ſpend upon chymical trials (to which I then happened to be invited by the opportunity of ſome furnaces and leiſure) much of thoſe endeavours, which they ſeemed to think might be far more uſefully employed, than upon ſuch an empty and deceitful ſtudy: perceiving this, I ſay, I thought it not amiſs to endeavour to manifeſt, that without ſeeking after the elixir, that alchymiſts generally hope and toil for (but which they, that knew me, knew to be not at all in my aim) I did not in the proſecution of chymical trials do any thing either without an end, or unſuitable to the deſign I had of attempting to promote men's knowledge of the works of nature, as well as their power over them. In order to this, I did not think it enough to ſhew, that by an inſight into chymiſtry one may be enabled to make ſome meliorations (I ſpeak not of * tranſmutations) of mineral and metalline bodies, and many excellent medicines for the health of men, beſides divers other preparations of good uſe in particular trades, and in ſeveral occurrences of human life; I did not, I ſay, think it enough to do this, becauſe, that though this might ſuffice to evince that a rational man might without loſing his time employ ſome of it to underſtand and promote chymiſtry; yet this would ſcarce ſuffice to manifeſt it to be uſeful to philoſophy. And therefore there ſeemed requiſite ſome ſpecimens, which might ſhew that chymical experiments might be very aſſiſtant even to the ſpeculative naturaliſt in his contemplations and inquiries.

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* See the *Eſſay of the Uſefulneſs of Chymiſtry* in Sect. II. Part I. *Of the Uſefulneſs of E. P.*

But againſt my attempting any thing of this nature, three difficulties oppoſed themſelves. The firſt was the want of leiſure, in regard I was already pre-engaged to write of other ſubjects, and to proſecute ſome experiments, whoſe event I was concerned to know. Another impediment was, that for other reaſons elſewhere mentioned, and chiefly to keep my judgment as unprepoſſeſſed as might be with any of the modern theories of philoſophy, till I were provided of experiments to help me to judge of them; I had purpoſely refrained from acquainting myſelf thoroughly with the intire ſyſtem of either the Atomical, or the Carteſian, or any other whether new or revived philoſophy; and therefore I could ſcarce be fit to ſhew, how chymical experiments might illuſtrate their doctrines. And thirdly, ſome of thoſe learned men, for whom I was to write, more favouring the Epicurean, and others (though but a few) being more inclinable to the Carteſian opinions, it ſeemed very difficult to gratify by the ſame compoſures perſons of differing perſuaſions.

But as to the firſt of theſe diſcouragements, ſince my pre-engagements to other themes were not unknown to thoſe, for whom I was to write, it might reaſonably be preſumed they would over-look ſuch unaccurateneſs as ſhould appear imputable to haſte: and beſides, ſome ſuch ſubject might be choſen to write of, as would conveniently admit enlargements and additions, according as my leiſure ſhould afterwards ſerve me to annex them.

On occaſion of the ſecond impediment, I remembered, that having divers years before read the lives of the Atomical, among other philoſophers, in *Diogenes Laertius*; and having ſometimes occaſionally heard mention made of divers Epicurean and Carteſian notions, and having hence framed to myſelf ſome general, though but imperfect, idea of the way of philoſophizing my friends eſteemed; I thought I might, without a more particular explicit inquiry into it, ſay ſomething to illuſtrate ſome notions of it, by making choice of ſuch, as, being of the more ſimple and obvious, did not require ſkill in the more myſterious points of the hypotheſis they belonged to.

And as for the laſt of the three diſcouragements abovementioned, I conſidered, that the Atomical and Carteſian hypotheſes, though they differed in ſome material points from one another, yet in oppoſition to the Peripatetic and other vulgar doctrines they might be looked upon as one philoſophy: for they agree with one another, and differ from the ſchools in this grand and fundamental point, that not only they take care to explicate things intelligibly; but that whereas thoſe other philoſophers give only a general and ſuperficial account of the phænomena of nature from certain ſubſtantial forms, which the moſt ingenious among themſelves confeſs to be incomprehenſible, and certain real qualities, which knowing men of other perſuaſions think to be likewiſe unintelligible; both the Carteſians and the Atomiſts explicate the ſame phænomena by little bodies variouſly figured and moved. I know, that theſe two ſects of modern naturaliſts diſagree about the notion of body in general, and conſequently about the poſſibility of a true vacuum; as alſo about the origin of motion, the indefinite diviſibleneſs of matter, and ſome other points of leſs importance than theſe: but in regard that ſome of them ſeem to be rather metaphyſical than phyſiological notions, and that ſome others ſeem rather to be requiſite to the explication of the firſt origin of the univerſe, than of the phænomena of it, in the ſtate wherein we now find it; in regard of theſe, I ſay, and ſome other conſiderations, and eſpecially for this reaſon, that both parties agree in deducing all the phænomena of nature from matter and local motion; I eſteemed that, notwithſtanding theſe things, wherein the Atomiſts and the Carteſians differed, they might be thought to agree in the main, and their hypotheſes might by a perſon of a reconciling diſpoſition be looked on as, upon the matter, one philoſophy. Which becauſe it explicates things by corpuſcles, or minute bodies, may (not very unfitly) be called corpuſcular; though I ſometimes ſtyle it the Phænician philoſophy, becauſe ſome antient writers inform us, that not only before *Epicurus* and *Democritus*, but even before *Leucippus* taught in *Greece*, a Phænician naturaliſt was wont to give an account of the phænomena of nature by the motion and other affections of the minute particles of matter. Which becauſe they are obvious and very powerful in mechanical engines, I ſometimes alſo term it the mechanical hypotheſis or philoſophy.

By ſuch conſiderations then, and by this occaſion, I was invited to try, whether, without pretending to determine the abovementioned controverted points, I could, by the help of the corpuſcular philoſophy, in the ſenſe newly given of that appellation, aſſociated with chymical experiments, explicate ſome particular ſubjects more intelligibly, than they are wont to be accounted for, either by the ſchools or the chymiſts. And however ſince the vulgar philoſophy is yet ſo vulgar, that it is ſtill in great requeſt with the generality of ſcholars; and ſince the mechanical philoſophers have brought ſo few experiments to verify their aſſertions: and the chymiſts are thought to have brought ſo many on the behalf of theirs, that of thoſe, that have quitted the unſatisfactory philoſophy of the ſchools, the greater number, dazzled as it were by the experiments of Spagyriſts, have imbraced their doctrines inſtead of thoſe they deſerted: for theſe reaſons, I ſay, I hoped I might at leaſt do no unſeaſonable piece of ſervice to the corpuſcular philoſophers, by illuſtrating ſome of their notions with ſenſible experiments, and manifeſting, that the things by me treated of may be at leaſt plauſibly explicated without having recourſe to inexplicable forms, real qualities, the four peripatetic elements, or ſo much as the three chymical principles.

Being once reſolved to write ſome ſuch *ſpecimina* as I formerly judged requiſite, I ſoon bethought myſelf of the experiment hereafter delivered concerning ſalt-petre, divers of whoſe phænomena I did alſo, as time would permit, caſt into one of the eſſays I was then engaged to write to a friend. And having diſpatched that little treatiſe, it found ſo favourable a reception among thoſe learned men, into whoſe hands it came, that I was much encouraged to illuſtrate ſome more of the doctrines of the corpuſcular philoſophy, by ſome of the experiments, wherewith my furnaces had ſupplied me; which alſo, as occaſion ſerved, I did, partly by writing ſome phyſico-chymical treatiſes, and partly by making ſuch large notes on the eſſay concerning ſalt-petre, as might plentifully contribute to the hiſtory of qualities, of which I had ſometimes thoughts. And this continued, till in the year before the laſt the public confuſions in this (then unhappy) kingdom reducing me to quit my former deſign, together with the place, where my furnaces, my books, and my other accommodations were, I fell afterwards upon the making of pneumatical trials, whereof I lately ventured to give the public an account in a book of new experiments phyſico-mechanical about the air.

Viz. A. D. 1659.

I should not trouble the reader with ſo prolix a preface to ſuch ſmall treatiſes as thoſe whereto this is prefixed, but for theſe two reaſons: the one, that I hope the foregoing narrative will make me be the more favourably judged by the philoſophers I deſire to ſerve, if ſometimes I write leſs ſkilfully of their opinions, than perhaps I ſhould have done, had I allowed myſelf to ſearch into them: and the other, that I am earneſtly ſolicited to publiſh ſome other tracts, tending to the ſame purpoſe that theſe do; to which alſo ſhould I ever be induced, by the reception theſe may meet with, to trouble the world with them, the ſame preface, as it is now penned, may ſerve for an introduction. I had almoſt forgot to take notice, that whereas at the end of the eſſay concerning ſalt-petre I mentioned a then newly publiſhed treatiſe of the laborious *Glauber*'s, which I had not then peruſed, I found it to contain ſome obſervations concerning the hiſtory of ſalt-petre, which, if they be true, are conſiderable enough; I muſt again recommend the examination of them to the reader's curioſity, having been hindered by divers avocations from ſaving him that labour myſelf. And whereas alſo ſome years after I was informed of another little book he had put out ſince the former, wherein he teaches us a way of purifying ſalt-petre, to make a conjunction of the ſpirituous and fixter parts of it, and then to ſuffer the mixture to evaporate and ſo cryſtallize into nitre; this would, I confeſs, have made me apprehenſive of paſſing for a plagiary with thoſe, that did not know me, but that it was eaſy for me to clear myſelf by the teſtimony of very learned men, who had ſome years before peruſed my treatiſe, and eſpecially of one perſon (well known by his writings) who was pleaſed to like it ſo well, as to deſire he might tranſlate it, and had accordingly long before turned it into very elegant Latin. I might perhaps venture to add, that though I could not juſtify myſelf by ſo convincing a proof of my innocence, yet he, that ſhall take the pains to conſider, that I could not borrow of *Glauber* the various phænomena I have particularly ſet down, and much leſs the reflections on them, and ſhall compare in what differing manners, and to what differing purpoſes, we two propoſe the making of ſalt-petre out of its own ſpirit, and fixed ſalt (he but preſcribing as a bare chymical purification of nitre, what I teach as a philoſophical redintegration of it) he, I ſay, who ſhall compare theſe things together, will, perchance, think, that I was as likely to find this laſt named experiment as another. Which things I ſay not, as if I ſcrupled to make uſe of the induſtrious *Glauber*'s, or any other man's experiments, eſpecially, when I borrow not with them any of the doctrines I build on them; but becauſe ſince I neither did nor could take any notice of *Glauber*'s book in mine, I judged it requiſite to ſay ſomething, to prevent my being thought to have unthankfully taken one of the chief paſſages of my diſcourſe from a book, to which I was utterly a ſtranger.

The reaſons of my thus conſenting to publiſh the following *Hiſtory of fluidity and firmneſs*, without the reſt of thoſe annotations, which I writ upon the ſame eſſay touching ſalt-petre, are partly, that theſe are my recenteſt compoſures of this nature (having been written but the laſt year ſave one) and were ſet down, when I allowed myſelf to be leſs unacquainted with writers addicted to the modern philoſophy; partly alſo, becauſe the conſiderableneſs of the ſubject invited me to make theſe annotations much more copious, and ſomewhat leſs unaccurate, than my notes upon almoſt any other part of the eſſay; and partly (and indeed principally) becauſe mention being ſometimes made of this hiſtory in my freſhly publiſhed phyſico-mechanical experiments, both the printer, and ſome learned gentlemen, who were pleaſed to think that book not unworthy the tranſlating, have ſollicited me to let this treatiſe be annexed to the ſeveral verſions they are about of that pneumatical piece, and to the Engliſh edition of the three foregoing diſcourſes, which the printer fears would, without the company of theſe or ſome others, make but too thin a book.

And I thought fit to premiſe to this hiſtory the eſſay concerning ſalt-petre, not only becauſe it might appear very improper to publiſh annotations without the text itſelf, whereunto they relate; but indeed becauſe I find, that there are ſtill many learned men, of the ſame diſpoſition with thoſe I have mentioned in the beginning of this preface; whence I am invited to divulge this eſſay by the ſame conſiderations, that at firſt induced me to write it. Eſpecially ſince I remember not, that among the new philoſophers, I have met with any one experiment, that does ſo fairly and ſenſibly accommodate ſo many of their opinions. And indeed I freely confeſs, that I ſhall think myſelf to have done no uſeleſs ſervice to the common-wealth of learning, if I prove ſo fortunate, as by theſe, or any other writings of mine to the like purpoſe, to beget a good underſtanding betwixt the chymiſts and the mechanical philoſophers, who have hitherto been too little acquainted with one another's learning; there being to this very day a great and almoſt general miſunderſtanding betwixt the corpuſcular philoſophers and the chymiſts; moſt of thoſe (on the one hand) looking upon the Spagyriſts as a company of mere and irrational operators, whoſe experiments may indeed be ſerviceable to apothecaries, and perhaps to phyſicians, but are uſeleſs to a philoſopher, that aims at curing no diſeaſe but that of ignorance; and moſt of the Spagyriſts (on the other hand) looking upon the Corpuſcularians (if I may ſo call them) as a ſort of empty and extravagant ſpeculators, who pretend to explicate the great book of nature, without having ſo much as looked upon the chiefeſt and the difficulteſt part of it; namely, the phænomena, that their art has added to the former edition of this vaſt and obſcure volume. But that ſome of the principal of the hermetic opinions may be more handſomely accommodated by the notions of the Phœnician hypotheſes, than by the common philoſophy of elements and ſubſtantial forms (which yet their writers ſo frequently allude to and otherwiſe employ) may appear from hence, that whereas the ſchools generally declare the tranſmutation of one ſpecies into another, and particularly that of baſer metals into gold, to be againſt nature, and phyſically impoſſible; the corpuſcular doctrine rejecting the ſubſtantial forms of the ſchools, and making bodies to differ but in the magnitude, figure, motion or reſt, and ſituation of their component particles, which may be always infinitely varied, ſeems much more favourable to the chymical doctrine of the poſſibility of working wonderful changes, and even tranſmutations in mixt bodies. And on the other ſide, there are ſcarce any experiments, that may better accommodate the Phænician principle, than thoſe, that may be borrowed from the laboratories of chymiſts. For firſt, chymiſtry enabling us to depurate bodies, and in ſome meaſure to analyze them, and take aſunder their heterogeneous parts, in many chymical experiments we may better than in others know what manner of bodies we employ, art having made them more ſimple or uncompounded, than nature alone is wont to preſent them us. And next, many chymical operations being performed in cloſe, and yet in tranſparent veſſels, we may better know what concurs to the effects produced, becauſe adventitious bodies (or at leaſt all groſſer ones) are kept from intruding upon thoſe, whoſe operations we have a mind to conſider. And laſtly, the bodies employed by the chymiſts being for the moſt part active ones, the progreſs of nature in an experiment, and the ſeries of ſucceſſive alterations, through which the matter paſſes from firſt to laſt, is wont to be made more nimbly, and conſequently becomes the more eaſy to be taken notice of and comprehended. So that all this conſidered, I hope it may conduce to the advancement of natural philoſophy, if, as I ſaid, I be ſo happy, as, by any endeavours of mine, to poſſeſs both chymiſts and Corpuſcularians of the advantages, that may redound to each party by the confederacy I am meditating between

tween them, and excite them both to enquire more into one another's philoſophy, by manifeſting, that as many chymical experiments may be happily explicated by corpuſcularian notions, ſo many of the corpuſcularian notions may be commodiouſly either illuſtrated, or confirmed by chymical experiments.

A
PHYSICO-CHYMICAL ESSAY,

Containing an EXPERIMENT, with ſome Conſiderations touching the differing parts and redintegration of SALT-PETRE.

SECTION I.

SALT-PETRE, *Pyrophilus*, though in that form, wherein it is ſold in ſhops, it be no very obvious concrete; yet either in its rudiments, or under ſeveral diſguiſes, it is not to be found in ſo great number of compound bodies, vegetable, animal, and even mineral, that it ſeems to us to be not only one of the moſt catholick of ſalts, but ſo conſiderable an ingredient of many ſublunary concretes, that we may juſtly ſuppoſe it may well deſerve our ſerious enquiries, ſince the knowledge of it may be very conduſive to the diſcovery of the nature of ſeveral other bodies, and to the improvement of divers parts of natural philoſophy.

SECT. II.

BUT not having at preſent much leiſure allowed me by ſeveral avocations, to make accurate enquiries into the nature of ſalt-petre in general; and, which is more conſiderable, being not yet furniſhed with a competent number of experiments requiſite to ſuch a purpoſe; I muſt content myſelf for this time to tender you ſome aſſiſtance, towards the diſcovery of how differing ſubſtances may be obtained from nitre, and compound it again, by preſenting you ſome reflections on an experiment, which my deſire to haſten to another ſubject obliges me to ſet down nakedly, as I firſt tried it, by way of narrative.

SECT.

SECT. III.

WE took then common nitre (as we bought it at the druggiſt's) and having by the uſual way of ſolution, filtration, and coagulation, reduced it into cryſtals, we put four ounces of this purified nitre into a ſtrong new crucible, * in which (the veſſel being firſt well nealed to prevent cracking, and covered to prevent the falling in of any thing, that might unſeaſonably kindle the petre) we melted it into a limpid liquor, and whilſt it was in fuſion, caſt into it a ſmall live coal, which preſently kindled it, and made it boil and hiſs, and flaſh for a pretty while. After which, we caſt in another glowing coal, which made it fulminate afreſh; and after that we caſt in a third and a fourth, and ſo continued the operation, till the nitre would neither fulminate nor be kindled any more. After which we continued to keep it in a ſtrong fire for above a quarter of an hour, that if any volatile part ſhould yet remain, it might be forced off.

SECT. IV.

THEN taking out the crucible, and breaking it whilſt it was hot, we took out, as carefully as we could, the remaining fixed nitre, before it had imbibed much of the moiſture of the air; and dividing it into two equal parts, we diſſolved one of thoſe portions in as much fair water as would juſt ſuffice for the ſolution of it, and then dropped on it ſpirit of ſalt-petre, till the ebullition occaſioned by the mutual action of thoſe contrary liquors did perfectly ceaſe; and forthwith filtrating this mixture, we expoſed it in a new open phial to the air in a window; and returning to the other portion of fixed nitre, which we had ſet apart and not diſſolved, we dropped on that likewiſe of the ſame ſpirit, till the hiſſing and ebullition were altogether ceaſed, and then we expoſe this mixture alſo in an open glaſs-jar to the air in the ſame window with the former.

SECT. V.

THE event of theſe trials was, that the mixture, wherein fair water was employed, did in a few hours faſten to the lower part of the ſides of the glaſs, wherein it was put, ſome ſaline particles, which ſeemed by their form (and partly too by their ſhoot-

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* If it be here demanded, Why the experiment was not made with a greater quantity of ſalt-petre? we may anſwer, that the mentioned quantity was moſt proportinate to the beſt crucible we then had. And if it be further aſked, Whether it were not the better way of obtaining the ſeveral ſubſtances ſeparable from nitre, to diſtil it in cloſe veſſels without addition of any foreign body? we ſhall reply only by repreſenting, that the propoſed way is not ſo practicable as one would imagine: for not acquieſcing in the common practice of chymiſts, who are wont to mingle with the ſalt-petre they diſtil three or four times its weight of brick, earth, or ſome other additament, which (eſpecially in ſo great a proportion) may much alter the nature of the fixed ſalt remaining behind with it; we have had the curioſity to try more than once, whether we could diſtil ſalt-petre *per ſe* in glaſs retorts, and found, that though to avoid giving too ſtrong a fire, we once (at leaſt) placed the retort only in a panful of ſand; yet when the heat was grown ſtrong enough to melt the ſalt, it cracked the retort, and did partly run out at the crack: only we obtained ſome ſmall quantity of a liquor, which by its ſourneſs and operation taught us what we might have expected of the reſt of the volatile part of the nitre, in caſe the veſſel would have held till it had paſſed over into the receiver.

ing about the lower parts of the veſſel) to be ſalt-petre; amongſt whoſe little cryſtals nevertheleſs there appeared to ſwim very little grains (much ſmaller than muſtard-ſeeds) of ſome other kind of ſalt, environed with a downy matter, not unlike that, which is oftentimes to be obſerved in roſe-water, and ſeveral other diſtilled waters, when they begin to decay. The cryſtals were the next day taken out, being by that time grown ſomewhat greater, and more numerous; and diſcloſed themſelves, upon trial, to be indeed nitrous, as well by their manner of burning, as their ſhape. Concerning the latter of which, ſince learned modern writers have miſ-repreſented it, ſome making nitre to be cylindrical, and others of a figure leſs approaching to the true one; I think myſelf obliged in this place to obſerve to you by the way, that having purpoſely conſidered ſome large cryſtals of refined and unanalyzed nitre, the figure being in ſuch beſt diſcerned, they appeared to have each of them ſix flat ſides (not always of equal breadth in reſpect of one another) whereof any two that were oppoſite were commonly parallel. But to return to our augmented cryſtals of nitre; what the other matter, that adhered to them, was, there was ſo very little of it, that we could not well diſcern, though we then ſuſpected it to proceed from the want of a juſt or exact proportion betwixt the volatile and fixed parts of the nitre, that were to be reunited.

SECT. VI.

THE remaining liquor being poured into an open glaſs-jar, and left in the ſame window, continued five or ſix days without manifeſting any conſiderable alteration; but at the end of that time there began to appear in it very fine cryſtalline ſtriæ of petre, which grew more and more numerous during a fortnight longer: at which time, being wearied with attending the ſo ſlow conſumption of the liquor, we poured it from the cryſtals, and ſet it in a digeſting furnace to evaporate more nimbly.

SECT. VII.

THE other mixture, wherein no water was employed, did preſently, for a great part of it, ſubſide in the form of ſalt; over which there ſwam a little liquor, which alſo ſeemed to keep the ſubſiding particles of ſalt from congealing into one coherent maſs, or ſo much as greater lumps: and a part of this drenched ſalt being taken out, and permitted to dry in the air, did not appear very regularly figured, but yet ſeemed here and there to recede very little from the ſhape of ſalt-petre; and being caſt on a quick coal, it burned partly after a manner not peculiar (that we have obſerved) to any diſtinct kind of ſalt; and yet it partly ſeemed to imitate the flaſhing way of deflagration proper to nitre. The remaining part of this ſalt, together with the liquor ſwimming upon it, we kept for about a month in the open air, without diſcerning any obſervable change in the liquor, till towards the latter end of that time, and then we found it partly coagulated into ſmall ſaline maſſes, whoſe figure we were not able to diſcern. And therefore diſſolving the whole mixture in a little fair water, and filtrating it, we found (after evaporation in a digeſting-furnace) about one half of the ſalt ſhot into fine ſmall icicles of the ſhape of cryſtals of petre, but ſomewhat differing from them in taſte upon their firſt being put upon the tongue; but upon a live coal they burned not unlike petre. And the remaining half of this diſſolution, being ſomewhat haſtily preſſed to exhale, let fall its ſalt in a figure, which we could not reduce either to that of ſalt-petre, or of any other determinate kind of ſalt.

For the clear comprehending of this experiment, you may be pleaſed, *Pyrophilus*, to take notice,

SECT. VIII.

1. That a new coal is not to be caſt on the nitre, till the detonation occaſioned by the former be either quite or almoſt altogether ended; unleſs it chance, that the puffing matter do blow the coal too ſoon out of the crucible, (which often enough happens towards the end of the operation:) which ſeems to happen chiefly, becauſe the firſt part of the nitre growing to be predominant, the inflammable and halituous particles cannot break through the matter, now grown more ſtiff, but by ſuch impetuous eruptions, as make them oftentimes toſs back the coals as ſoon as ever they are caſt into the crucible: and in this way of proceeding we have been forced to ſpend much more time, than the opinion of the already deflagrability (if I may ſo ſpeak) of ſalt-petre did beforehand permit us to imagine.

SECT. IX.

2. That we diſcerned by our ſcales, that the weight of the ſpirit of nitre requiſite to be dropped on, till all the ebullition made betwixt that liquor and the ſolution of fixed nitre were ceaſed, did not amount to ſo great a weight, as the ſalt-petre loſt in its detonation, and yet fell not much ſhort of it.

SECT. X.

3. That the fixed nitre this way made differed but very little from vulgar ſalt of tartar in its lixiviate taſte, in its aptneſs to attract the air, or to relent by the moiſture of it, and in its other more obvious qualities; only whereas ſalt of tartar is wont to be white (which nevertheleſs being fluxed has been by others, as us, obſerved to become of a kind of ſky-colour) this fixed nitre was of a deep colour betwixt blue and green: which colour, upon the affuſion of the ſpirit of nitre, vaniſhed; whereas otherwiſe (to obſerve that to you upon the by) ſome ſort of calcined nitre will ſo obſtinately retain that colour, that I keep by me a bluiſh green liquor made of fixed-petre, I know not how oftentimes ſucceſſively reſolved *per deliquium* and coagulated again, till it would no longer be reduced to a dry ſalt, but to an unctuous body eaſily flowing in heat like wax: my deſign in which trial it were here ſomewhat improper to inſiſt on *.

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* That our friends might not be obliged to wait ſo long for the redintegration of nitre, but might ſee the experiment made in as little time as is poſſible, we deviſed a more expeditious way of uniting the divorced parts of our ſalt, and it was only by ſuffering ſuch fixed nitre, as is mentioned by our author, to run *per deliquium* into a liquor, which being ſeparated from its *fæces* by filtration through cap-paper was very clear and limpid: for, when we had a mind to ſhew the experiment, we did only upon this liquor drop ſome ſpirit of nitre, and that, after ſuch a noiſe, ſparkling and efferveſcence, as our author ſpeaks of,

SECT. XI.

AND becauſe, *Pyrophilus*, it may be ſuſpected, that the ſalt-petre mentioned to have been produced by the reunion of the volatile and fixed part of that concrete, may have been only ſome aſſociated particles of ſalt-petre, that by lurking undiſcernedly in the fixed nitre had eſcaped the analizing violence of the fire, and by the affuſion of fair water were ſet at liberty to aſſemble together, and thereby diſcloſe themſelves in their true ſhape:

To remove this ſcruple, and to let you ſee that much of the enſuing diſcourſe will not need your ſuppoſing, that the experiment of the redintegration of petre was accurately made, and did accordingly ſucceed; I muſt here annex, that though by divers other chymical experiments, which I have had occaſion to make with ſalt-petre, I ſometimes diſcovered, that now and then ſome undiſcerned particles of the ſalt-petre may poſſibly eſcape our diligence when we make fixed nitre; yet thoſe particles are too few to amount to ſuch cryſtals of petre, as the affuſion of the acid ſpirit upon the lixiviate ſalt are capable of affording. And that we have, to ſatisfy ourſelves farther in this particular, purpoſely ſatiated, according to the former manner, a ſolution of common pot-aſhes, bought of them, that are wont to ſell it in ſhops, (who are not ſo fooliſhly knaviſh, as to adulterate them with ſalt-petre, which is much dearer than pot-aſhes) and filtrating the ſolution from its copious *fæces*, found after evaporation, in the remaining liquor, within about two or three days, and ſometimes much ſooner, pretty ſtore of cryſtalline ſalt in a nitrous figure, which though at firſt it taſted ſomewhat corroſively, (perhaps becauſe the proportion betwixt the nitrous ſpirit and the pot-aſhes was not duly obſerved) yet after it had a while remained upon the tongue, the taſte of it much emulated that of ſalt-petre; and part of it being caſt upon a live coal, did by its blue and halituous flame diſcover itſelf to be of the nature of that ſalt. To which we may add, that we likewiſe tried the experiment with aqua fortis and ſalt of tartar, and thereby produced ſalt-petre, though but in ſmall quantity, and a long time. And thoſe two additional experiments I the rather

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of, (all which haſtily vaniſhed) did immediately aſſociate itſelf with a competent proportion of the fixed ſalt ſwimming in the ſolution, and therewith fall down in little icicles of a nitrous ſhape and nature; and when we pleaſed to continue the affuſion of the acid ſpirit, this emergency of ſalt petre would be obſervable from time to time, till either all, or almoſt all the fixed ſalt had united itſelf with the other. And theſe little icicles being dried in lumps, did as well upon the tongue and upon a quick coal, as they had done to the eye, diſcloſe themſelves to be ſo truly nitrous, that our friends were not wont without ſome wonder, as well as much pleaſure, to behold ſalt-petre thus ſuddenly generated in leſs than a minute of an hour. Theſe ſmall icicles being in ſufficient plenty diſſolved in fair water, we did for trial's ſake reduce by congelation to fairer cryſtals.

But though this be the perfecteſt and readieſt way of reproducing nitre, yet becauſe it often requires, eſpecially in dry weather, a long time to reduce fixed nitre *per deliquium* into a liquor, we have ſometimes ſubſtituted the following way: We diſſolved in fair water as much fixed nitre as we cou'd, and filtrating the ſolution through cap-paper, we ſatiated it with ſpirit of nitre, after the manner above deſcribed; and then ſetting it to evaporate very ſlowly, and afterwards ſuffering it to cool, we obtained, within ſome hours after the firſt mixture of the liquors, ſtore of fine little cryſtals of petre, which ſhot in the liquor; the remaining part of which being evaporated afforded more of them. And though the evaporation and cryſtallization coſt us divers hours, yet it ſeemed, that the ſalt-petre was produced preſently upon the ceaſing of the conflict betwixt the two liquors. For the mixture, before evaporation, taſted very like a ſolution of common nitre, and the little drops, that upon the efferveſcence ſkipped out of the glaſs, and fell back upon the ſides of it, did there many of them preſently coagulate into little grains of nitrous ſalt.

mention, becauſe many of the enſuing reflections may be juſtified by them, although the main experiment made on ſalt-petre alone ſhould in divers particulars be ſuppoſed (for we have uſed our endeavours that it may not be found) to have been miſtaken.

SECT. XII.

The reflections, which may be made on this experiment, are more than I have either the ſkill or leiſure to proſecute; and therefore I ſhall content myſelf to preſent you very ſuccinctly with a few of thoſe, that do the moſt readily occur to my preſent thoughts.

And firſt this experiment ſeems to afford us an inſtance, by which we may diſcern, that motion, figure, and diſpoſition of parts, and ſuch like primary and mechanical affections (if I may ſo call them) of matter, may ſuffice to produce thoſe more ſecondary affections of bodies, which are wont to be called ſenſible qualities.

SECT. XIII.

And to begin with the tangible qualities, as heat and cold; it is commonly held, that ſalt-petre is in operation a cold body, if not one of the coldeſt in the world; and accordingly phyſicians and chymiſts are wont to give it in fevers, to allay the inward exæſtuations of the blood and humours: and that profound naturaliſt the Lord *Verulam* highly commends a little of it, and did for many years himſelf make uſe of it, to condenſe the ſpirits. But whatever it be in inward operation, certainly to the outward ſenſe it appears very cold: and yet the parts of this ſo cold body (its ſpirit and alkali, by the latter of which chymiſts are wont to mean any fixed ſalt produced by burning) put together, do immediately agitate each other with great vehemency; and did in our experiment produce ſuch an heat, that I could ſcarcely endure to hold in my hand the phial, wherein much leſs than an ounce of each was mixed, though but leiſurely and almoſt by drops; as if heat were nothing but a various and nimble motion of the minute particles of bodies. For in our experiment, as long as that confuſed agitation laſted, ſo long the heat endured, and with that agitation it increaſed and abated; and at length, when the motion ceaſed, the heat alſo vaniſhed.

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Upon the mixture of the two fore-mentioned liquors, there was alſo produced a very audible ſound, not unlike the hiſſing produced by the quenching of a live coal in water; and this hiſſing was, as that other is wont to be, accompanied with an effervеſcence and boiling up of the liquor, with ſtore of bubbles, till it was ready to run over the veſſel. This ſound ſeemed to proceed from the nimble and ſmart percuſſions of the ambient air, made by the ſwift and irregular motions of the particles of the liquors: and ſuch a kind of ſound, but much louder, was produced by the impetuous eruptions of the halituous flames of the ſalt-petre upon the caſting of a live coal upon it. What intereſt ſuch a ſmartneſs in ſtriking the air hath in the production of ſound, may in ſome meaſure appear by the motion of a bullet, and that of a ſwitch or other wand, which produce no ſound, if they do but ſlowly paſs through the air; whereas if the one do ſmartly ſtrike the air, and the other be ſhot out of a gun, the celerity of their percuſſions on the air puts it into an undulating motion, which reaching the ear, produces an audible noiſe, even at a good diſtance from the body, whoſe ſwift paſſage cauſes thoſe nimble vibrations in the air, as we may elſewhere have occaſion to declare. And that in the ſound obſervable in our experiment, the contiguous air receives many ſtrokes from the particles of the liquor, ſeems probable, by the ſudden and eager tumultuation of the parts of the liquors; and by this, that the noiſe increaſed and decayed proportionably to the ebullition of the liquors, and ceaſed altogether as ſoon as the ſaline particles floating in them had by their conflict tired themſelves into quietneſs. And it is to be obſerved, that the noiſe ended long before the heat. To the latter of which ſuch an inteſtine tumult of the parts of many bodies is ſufficient, as is yet uncapable to produce a ſound. As we ſee in amber or good hard wax heated by rubbing; and in many liquors, which retain a conſiderable degree of heat a good while after the expiration of the noiſe they made in boiling.

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liquor

mention, becauſe many of the enſuing reflections may be juſtified by them, although the main experiment made on ſalt-petre alone ſhould in divers particulars be ſuppoſed (for we have uſed our endeavours that it may not be found) to have been miſtaken.

SECT. XII.

The reflections, which may be made on this experiment, are more than I have either the ſkill or leiſure to proſecute; and therefore I ſhall content myſelf to preſent you very ſuccinctly with a few of thoſe, that do the moſt readily occur to my preſent thoughts.

And firſt this experiment ſeems to afford us an inſtance, by which we may diſcern, that motion, figure, and diſpoſition of parts, and ſuch like primary and mechanical affections (if I may ſo call them) of matter, may ſuffice to produce thoſe more ſecondary affections of bodies, which are wont to be called ſenſible qualities.

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SECT. XVI.

Upon the mixture of theſe two liquors there alſo obtrudes itſelf upon the ſenſe a very ſtrong and offenſive ſmell, proceeding from the ſpirit of petre; which perhaps occaſioned ſome chymiſts to call a menſtruum (wherein that nitrous ſpirit and ſmell is predominant) the Stygian water. But though the nitrous ſpirit have a very ſtrong and unwelcome odour of itſelf, yet is it made much more offenſive by being poured on its own fixed ſalt; for upon their own conflict, the matter, being vehemently agitated, doth more copiouſly emit ſuch ſtinking exhalations than before, and ſendeth forth fumes manifeſtly diſcernible as well to the eye as noſtrils. The odour of the fixed nitre is very languid; but that, which it diſcovers, being diſſolved in a little hot water, is altogether differing from the ſtink of the other, being of kin to that of other alkalizate ſalts. And yet the ſalt-petre, from which ſuch differingly-ſcented bodies ſpring, and which may again emerge from the coalition of them, has not been obſerved, as I remember, to have any ſmell at all.

SECT. XVII.

The taſtes of theſe two bodies are as differing as any of their other qualities: for the ſpirit is exceedingly acid, and may be called a ſtrong and ſour *Acetum Minerale*; whereas the fixt nitre has as ſtrong a taſte of ſalt of tartar as the ſpirit has of diſtilled vinegar: and yet theſe two bodies, whoſe ſapours are ſo pungent, and ſo differing, do both ſpring from and unite into ſalt-petre, which betrays upon the tongue no heat nor corroſiveneſs at all, but coldneſs mixt with a ſomewhat languid reliſh retaining to bitterneſs. And though we muſt not conceal from you, that in our trial the redintegrated ſalt-petre had upon its firſt impreſſion upon the tongue a taſte more ſharp and perforating (if I may ſo ſpeak) than ordinary nitre; yet that pungency may not improbably be ſuppoſed to have proceeded from ſome acid particles of the ſpirit, that were not yet duly incorporated with, but only looſely adherent to, the more perfectly nitrous parts, which afterwards diſcovered itſelf upon the tongue. And however, the difference betwixt the taſte of this new ſalt, and thoſe of the acid and alkalizate ſalts whereof it conſiſted, and (unqueſtionably) the taſte of theſe compared with that of the crude petre, which was diſſipated into them, were ſufficient to warrant this reflection.

SECT. XVIII.

Of the other obſervables preſented us by our experiment, we muſt, *Pyrophilus*, content ourſelves to mention but a few; our haſte being ſuch, that it will not permit us either to enumerate them all, or to inſiſt long on any of them.

SECT. XIX.

SECONDLY then, the propoſed experiment ſeems to make it ſomewhat queſtionable, whether or no inflammability doth ſtrictly in all mixt bodies require a diſtinct ſulphureous ingredient; and whether or no in ſome concretes it may not reſult from ſuch a contrivance of parts, as that thereby the particles of the concrete are diſpoſed to be ſet a moving by the adventitious, whether fiery or calorific, corpuſcles of another body, in ſuch numbers, and with ſuch celerity, as may put them into that ſcheme of matter, which we call flame. How violent an heat may be produced upon ſuch an account as this, may in ſome meaſure appear by an experiment, wherein our preſent theme ſalt-petre is the main agent. For if into a phial filled with good ſpirit of nitre you caſt a piece of iron, you may perceive, that the liquor, whoſe parts moved placidly and uniformly before, manifeſted no heat to the touch, meeting with pores and particles in the iron capable of very much altering the motion of its parts (and perhaps alſo that of ſome very ſubtile intercurrent matter) thoſe active parts do preſently begin to penetrate, ſever, and ſcatter abroad the particles of the iron (almoſt as gunpowder doth the pieces of breaking granadoes) with ſuch rapidity, and in ſuch plenty and throngs, that being themſelves alſo put into a very ſwift and irregular motion (whenceſoever it proceeds) there is hereby produced a heat capable (if the quantity of the liquor and metal be great enough) to burn his hand, that holds the veſſel, and perhaps break the veſſel (if it be not very open) all to pieces: whereas by caſting into the ſame ſpirit of nitre little lumps of camphire, whoſe particles were indiſpoſed to occaſion the like diſturbance and agitation in the nitrous ſpirit, we obſerved the agitation made of the particles of the white gum to change it only into a yellowiſh and fluid ſeeming oil.

SECT. XX.

BUT not to wander any further, our own experiment informs us, that ſalt-petre (which not only is inflammable, but burns very fiercely and violently) may be produced by the coalition of two bodies, which are neither of them inflammable; the one being a fixed ſalt, that to become ſuch has already ſuffered the loſs of all that the fire could deprive it of, and the other being a ſpirit abounding with acid particles, which kind of ſalts have been obſerved to be more apt to quench than foment fire.

SECT. XXI.

AND becauſe we may elſewhere, God aſſiſting, treat more particularly of the inflammableneſs of bodies, we will now add but a few lines concerning that of nitre, that this circumſtance of it might not eſcape our obſervation; namely, that upon caſting ſalt-petre on a glowing coal, or upon the caſting of a glowing coal into melted ſalt-petre, the nitre will immediately take fire, and flaſh out into bluiſh and halituous flames; whereas if the ſame nitre be placed in a crucible, though that crucible be by degrees made glowing hot, and do immediately with its concave ſurface in innumerable places touch the particles of nitre, yet the ſtrange ſalt will be thereby melted, but not kindled. The reaſon of which phænomenon I muſt not now (but may on another occaſion) ſpend time to inquire after.

SECT. XXII.

IT may alſo, *Pyrophilus*, deſerve the inquiry, whence it proceeds, that whereas the body of ſalt-petre, when committed to diſtillation, is oftentimes very well dried, and conſiſts of ſaline parts, which are generally accounted to be of a very dry nature, yet the ſpirits of petre forced by the fire into the receiver ſhould not, like ſal-armoniac, and ſome other bodies diſtilled with the like heat and veſſels, adhere in the form of ſublimate to the receiver, but fall into a liquor, which does not, for aught we have ſeen or heard of, either totally or in part coagulate again the cold, as we have ſeen ſpirit of urine and other volatile liquors (afforded by animal ſubſtances) often do; and as we have obſerved, though rarely, even in the corroſive liquor, that is wont to be called butter of antimony. And the like inquiry may be made concerning the liquidneſs of the diſtilled ſpirits of depreciated ſalt, calcined vitriol, and divers other bodies, which ſeem to have been deſtitute of moiſture, when committed to diſtillation.

SECT. XXIII.

BUT this not being preciſely a phænomenon of our experiment, we ſhall not here proſecute it (though perhaps we elſewhere may) but rather obſerve to you, *Pyrophilus*, that whereas good ſpirit of nitre being left in an open veſſel, is wont to ſmoak and waſte itſelf in an exhalation ſenſible, eſpecially if it be excited by a little heat, not only in the noſe but to the eye; this fugitive ſpirit, when it is once re-united to its own fixed ſalt, emits no ſuch ſteam, though kept a good while near a conſiderable fire: which inſtance may ſomewhat aſſiſt us to make out, that the moſt fugitive parts of concretes may in ſpight of their natural mobility be detained in bodies by their union and texture with the more ſluggiſh parts of them, among which thoſe lighter and more active ingredients may be ſo entangled, as to be reſtrained from avolation.

SECT. XXIV.

ANOTHER thing worth conſidering in our experiment is this, that upon the dropping of the acid ſpirit into the alkalizate liquor, if you place the open-mouthed glaſs, wherein the experiment is performed, betwixt the light and your eye, you may plainly diſcern, that the ſaline particles of theſe liquors toſs one another (or are toſſed by ſome briſk inviſible ſubſtance) to the height of divers fingers breadth up into the air, whence moſt of them fall back into the veſſel like a thick ſhower of little drops of rain. And it were worth inquiring, whence this ſparkling of the parts of theſe mixed liquors ariſes; and whether the ſaline corpuſcles may be conceived rapidly to move differing ways, and ſo, thwarting each other in their courſes, and rudely joſtling at their occurſions, ſome of them are forced to bound or fly upwards (almoſt like ivory balls meeting each other on a billiard-table). And to aſſiſt you in this inquiry, give me leave to inform you, that the particles thus thrown into the air appear to be moſt of them ſaline by this obſervation; that ſoon after the fall of the fore-mentioned ſhowers, you ſhall find the ſides of the glaſs, wherein the affuſion of the nitrous ſpirit has been made, all embroidered with little grains of ſalt, left there by thoſe wandering drops, that fell beſides the liquor.

SECT. XXV.

AND let me farther obſerve to you, that there ſeems to be a very nimble agitation in the particles of the ſpirit of nitre, by this, that upon the pouring of aqua fortis (whoſe active part is little elſe than ſpirit of nitre) upon a ſolution of ſalt of tartar in fair water, in which divers ſmall lumps of the ſalt remained yet undiſſolved, we have obſerved the acid ſpirit to ſever the particles of the ſalt with ſuch impetuoſity, that the numberleſs little bubbles produced upon their conflict, and haſtily aſcending in ſwarms from ſome of the little lumps, made them emulate ſo many little, but rapidly riſing, ſprings. And to make it yet appear more probable, that there may be ſuch croſſing motions in the parts of theſe liquors, we obſerved, that after the two contrary ſalts had by their mutual conflict tired each other (or rather had been upon their occurſions faſtened to one another) there would follow no farther ebullition or ſkipping up and down of little drops of the liquors, upon the putting in of more ſpirit of nitre, unleſs there were added likewiſe more of the alkalizate liquor.

SECT. XXVI.

AND, before we paſs on from this reflection, it may not be uſeleſs to take notice of the difference, that there may be betwixt thoſe active parts of a body, which are of differing natures, when they are as it were ſheathed up, or wedged in amongſt others in the texture of a concrete; and the ſame particles when (extricated from theſe impediments) they are ſet at liberty to flock together, and by the exerciſe of their nimble motions diſplay their proper, but formerly clogged activity, or acquire a diſpoſition to be briſkly agitated by ſome fine interfluent matter. For though in the intire body of ſalt-petre the ingredients it conſiſts of, or the differing ſubſtances, into which the fire diſſipates it, do ſo mutually implicate and hinder each other, that the concrete, whilſt ſuch, acts but very languidly; yet when the parts come to be diſlocated, and the halituous and alkalizate particles are enabled or made to diſband from the concrete, and aſſociate themſelves with thoſe of their own nature, we ſee with how great an activity both the acid ſpirit and the fixed ſalt are endowed.

SECT. XXVII.

AND we may yet farther obſerve, that it is not barely an indefinite nimbleneſs of motion, and activity of the particles of ſaline liquors, that enables them to perform each of their particular effects: for to the production of ſome of theſe there ſeems requiſite, beſides perhaps a modification of their motion, a determinate figure of the corpuſcles, anſwerable to that of the pores of the body by them to be diſſolved; as ſpirit of nitre corrodes ſilver, but not gold; which nevertheleſs its particles aſſociated with thoſe of ſal-armoniac, and thereby acquiring a new figure, and perhaps a differing motion, will readily diſſolve: and the liquor of fixed nitre will, for the ſame reaſon, diſſolve ſuch ſulphureous and unctuous bodies as the acid ſpirit will not corrode: nay, and I have carefully obſerved, that there may be liquors, that will not diſſolve ſome bodies, unleſs the motion or activity of their particles be allayed or modified by the mixture of fair water, or ſuch unactive vehicles.

SECT. XXVIII.

ANOTHER particular, which in our experiment we may take notice of, is, the unwarineſs of thoſe vulgar chymiſts, who preſume confidently (and indiſcriminately enough) to aſcribe to each of the heterogeneous ingredients, or (in their language) principles of a concrete analyſis by the fire, the virtues and properties (perhaps too in an exalted degree) of the entire body. But though this be an error of very ill conſequence in reference to divers chymical preparations of medicines; yet having elſewhere diſcourſed purpoſely of it, we ſhall here content ourſelves to allege againſt it the inſtances afforded us by the experiment under conſideration: for in that we may obſerve, that when ſalt-petre is diſtilled, the volatile liquor and fixed ſalt, into which it is reduced by the fire, are endowed with properties exceeding different both from each other, and from thoſe of the undiſſipated concrete. For the ſpirit of nitre is (as we formerly have obſerved) a kind of *Acetum Minerale*, and poſſeſſes the common qualities to be met with in acid ſpirits as ſuch; whereas the fixed nitre is of an alkalizate nature, and participates the qualities belonging generally to lixiviate ſalts; and ſalt-petre itſelf is a peculiar ſort of ſalt, diſcriminated by diſtinct properties both from thoſe ſalts, that are eminently acid, as allum, vitriol, ſal-gemmæ, *&c.* and from thoſe, that are properly alkalizate, as ſalt of tartar and pot-aſhes; and accordingly, we may eaſily obſerve a vaſt diſparity in the effects and operations of theſe three bodies. For ſeveral, if not all of thoſe mineral ones, which aqua fortis will by corroding diſſolve, the ſolution of fixed nitre will precipitate; and divers, if not all of thoſe ſulphureous and unctuous bodies, which the ſolution of fixed nitre will diſſolve, the acid ſpirit of petre will precipitate. And we have in a trice re-diſſolved with the ſpirit a ſolution of ſublimate precipitated with the other liquor: thus, if into a ſcarlet tincture made by an infuſion of Braſil in fair water, we pour a little ſpirit of nitre, the ſhaken liquor will in a moment change its redneſs for a kind of yellow, which by pouring on it a little of the ſolution of fixed nitre, may be again graduated into a ſomewhat ſanguine colour, ſometimes paler, and ſometimes perhaps deeper than the firſt; whereas a ſolution of ſalt-petre itſelf poured on either of the former tinctures, the red or the yellow, has not been by us diſcerned to have produced any ſenſible alteration. And whereas ſalt-petre itſelf is partly fixed, and partly volatile, the acid ingredients of it are altogether volatile, the alkalizate fixed. But having elſewhere occaſion to ſpeak to this ſubject, we ſhall now proceed to tell you, that

SECT. XXIX.

IT may paſs to another obſervable preſented us by our experiment, that it gives us occaſion to inquire, whether the air doth not contribute ſomething to the artificial production of ſalt-petre, or at leaſt to the figuration of it according to the perfecter ſhape belonging to that kind of ſalt: for we formerly obſerved, that the ſalt, which was leiſurely permitted to ſhoot of itſelf in the liquor expoſed to the open air, did ſhoot into more fair and large cryſtalline ſtriæ, than thoſe, that were gained out of the remaining part of the ſame liquor by a more haſty evaporation, though made but in a digeſting furnace. And we have alſo obſerved, that when once we poured aqua fortis on a ſtrong ſolution of ſalt of tartar, till no further effervеſcence was diſcernable betwixt them, though the mixture by a ſomewhat quick heat afforded a ſalt, that ſeemed to be very nitrous, yet it would not be brought to ſhoot in ſo fair and conſpicuouſly figured cryſtals of petre, till it had been a good while expoſed to the open air: but whether the air itſelf impregnated with the promiſcuous ſteams of moſt of the bodies of the terreſtrial globe (and perhaps with ſeminal effluvia from ſome of them) do really contribute any thing either to the production or figuration of ſalt-petre in our experiment, I dare not yet determine, for two chief reaſons.

SECT. XXX.

WHEREOF the firſt is, becauſe the figuration ſeems not improbably aſcribable, not ſo much to the proper efficiency of the air, as to the conveniency, which by quietneſs, and a competent vehicle to move in, was afforded to the ſaline particles, to conform themſelves (or be conformed by a concourſe of agents and circumſtances) to that figure, which is moſt natural to them. For we have obſerved already, that the fixed nitre, which was not diſſolved in water, before the affuſion of the acid ſpirit, did not ſhoot into the wonted form of cryſtals of petre, but remained a kind of nitrous powder, the acid and alcalizate ſaline particles not having a convenient vehicle to expand themſelves in; but being neceſſitated, for want of room, to make an unſeaſonable and over-haſty coalition, upon which their own weight made them ſubſide in the figures reſulting from their caſual concourſe, and therefore probably differing from thoſe, into which the ſaline corpuſcles would have been diſpoſed, had they been allowed a competency of vehicle and time.

SECT. XXXI.

THE other reaſon of my heſitancy about the uſe of the air in our experiment, is, that I inconſiderately forgot to try, whether part of that liquor, which ſhot into cryſtals in an open-mouthed-glaſs expoſed to the air, would not have done the like, if it had been left quiet as long as the other was, though in a veſſel accurately ſtopped *: but whatever the air hath to do in this experiment, I dare invite you to believe, that it is ſo enriched with variety of ſteams from terreſtrial (not here to determine whether it receive not ſome alſo from celeſtial) bodies, that the inquiring into the further uſes of it (for I mean not its own uſes in reſpiration, ſailing, pneumatical engines, *&c.*) may very well deſerve your curioſity. To encourage which, I dare at preſent only tell you, that though I cannot yet pretend to much experience in this particular, yet we have known ſuch changes (ſeemingly chymical) made in ſome ſaline concretes, by the help chiefly of the volatilizing operations of the open air, as very few, ſave thoſe, that have attentively conſidered what *Helmont*, and one or two other artiſts, have hinted on that ſubject, or have made trials of that nature themſelves, will be apt to imagine.

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* Whether the air have any great intereſt in the figuration or in the re-production of nitre, the author hath ſince examined by particular trials; but in veſſels and by ways not to be eaſily deſcribed in few words, and therefore the further mention of them is reſerved for another diſcourſe.

SECT. XXXII.

And if upon further and exacter trial it appears, that the whole body of the ſalt-petre, after its having been ſevered into very differing parts, by diſtillation, may be adequately re-united into ſalt-petre equiponderant to its firſt ſelf; this experiment will afford us a noble and (for aught we have hitherto met with) ſingle inſtance to make it probable, that that, which is commonly called the form of a concrete, which gives it its being and denomination, and from whence all its qualities are in the vulgar philoſophy, by I know not what inexplicable ways, ſuppoſed to flow, may be in ſome bodies but a modification of the matter they conſiſt of; whoſe parts, by being ſo and ſo diſpoſed in relation to each other, conſtitute ſuch a determinate kind of body, endowed with ſuch and ſuch properties. Whereas if the ſame parts were otherwiſe diſpoſed, they would conſtitute other bodies of very differing natures from that of the concrete, whoſe parts they formerly were, and which may again reſult or be produced after its diſſipation and ſeeming deſtruction, by the re-union of the ſame component particles, aſſociated according to their former diſpoſition.

SECT. XXXIII.

The redintegration (or re-production) of an analyzed body, if can be accurately and really performed, may give much light to many particulars in philoſophy, and would certainly be very welcome both to the embracers of the atomical hypotheſis, and generally to thoſe other modern naturaliſts, who aſpire to ſuch explications of nature's phænomena as may at leaſt be underſtood: all whom I wiſh, that though men cannot perhaps in all things, yet at leaſt as far as they can, they would accuſtom themſelves to ſpeak and think, as nature does really and ſenſibly appear to work; and not to acquieſce in notions and explications of things, which, ſtrictly examined, are not intelligible.

Wherefore I am about to attempt a reproduction in vitriol, turpentine, and ſome other concretes, in which it ſeems not unlikely to be performable: and perhaps you may ſee cauſe to think, that the experiment of ſalt-petre, even as we have already made and propoſed it, though it be not an exact and adequate redintegration, is yet not far from being a real one; the diſſipated parts of the concrete truly re-uniting into a body of the ſame nature with the former, though not altogether of the ſame bulk.

SECT. XXXIV.

And yet I think it requiſite to repreſent to you, *Pyrophilus*, that ſalt-petre is a body, whoſe parts are not organical, and which is not ſo much as very compounded; and that therefore bodies, that conſiſt of more numerous ingredients, and much more thoſe, whoſe organical parts require a much more artificial and elaborate diſpoſition or contrivance of their component particles, cannot be ſafely judged of, by what is poſſible to be performed on a body of ſo ſimple and ſlight a contexture as is ſalt-petre: for poundee, that even wine, though no organical body, nor ſo much as the moſt com-drawned of inanimate concretes, when its ſpirit is, though by the gentleſt diſtillation, priſti from it, will not, by the re-union of its conſtituent liquors, be reduced to its too ene nature; becauſe the workmanſhip of nature in the diſpoſition of the parts was thoſ laborate to be imitable, or repairable by the bare and inartificial appoſition of e divided parts to each other: beſides that in the diſſociating action, even of the gentleſt fire, upon a concrete, there does perhaps vaniſh, though undiſcernedly, ſome active and fugitive particles, whoſe preſence was requiſite to contain the concrete under ſuch a determinate form; as we ſee in wine degenerating into vinegar, where the change ſeems to proceed from this, that upon the avolation, or (if I may ſo ſpeak) depreſſion, of ſome ſubtile ſulphureous ſpirits, whoſe receſs or degeneration is not to be perceived by any ſenſible diminution of bulk in the liquor, the remaining parts fall into new leagues or diſpoſitions, and conſtitute an acid liquor ſomewhat fixed and corroſive, and conſequently of qualities very differing from thoſe of the wine, whoſe ſouring produced it: as we more fully declare in our experiments relating to fermentation.

SECT. XXXV.

AND certainly there is, as we formerly ſaid, ſo artificial a contrivance of particles requiſite to the conſtitution of the organical parts of living bodies, that it will be ſcarce poſſible for human art or induſtry to imitate, ſo as to equal thoſe exquiſite productions of nature. And therefore I wonder not, that the ſtory of the Phœnix's reſurrection out of her own aſhes ſhould, by the beſt naturaliſts, be thought a meer fiction. And if that relation, mentioned by the inquiſitive *Kircherus* as an eye-witneſs of the re-production (if I may ſo call it) of ſhell-fiſhes near the brink of a lake in the Sicilian promontory *Peloro*, by the watering of their broken bodies with ſalt-water in the ſpring, be ſtrictly true, it ſeems much more improbable, that ſuch changes and viciſſitudes ſhould be bare redintegration of the diſſociated parts of ſuch reſtored bodies; than that (according to what we * elſewhere teach) they ſhould be new productions made by ſome ſeminal particles undiſcernedly lurking in ſome part of the deſtroyed body, and afterwards excited and aſſiſted by a genial and cheriſhing heat ſo to act upon the fit and obſequious matter, wherein it was harboured, as to organize and faſhion that diſpoſed matter according to the exigencies of its own nature. For that in ſome bodies the ſeminal particles may a while ſurvive the ſeeming deſtruction of life, is not altogether without example, as we elſewhere profeſſedly manifeſt. And in *Kircher*'s ſtory it is to be obſerved, that the reſtored animals were but ſhell-fiſh; in whoſe ſlimy and viſcous ſubſtance the ſpirits and prolific parts are probably both more diffuſed and kept from being eaſily diſſipable; to which I know not whether it will be worth while to ſubjoin, that in ſuch fiſhes the mechanical contrivance is but very plain, and, as it were, ſlight and obvious, in compariſon of the exquiſitely elaborated parts of more perfect animals.

Lib. 3. de Arte Magnet. Par. 5. cap. 3.

SECT. XXXVI.

THE laſt obſervable, *Pyrophilus*, that we ſhall at preſent take notice of in our experiment, ſhall be this, that it may thereby ſeem probable, that ſome chymical remedies may be too raſhly rejected by phyſicians, becauſe oil or ſpirit of vitriol, aqua regis, or other corroſive liquors, have been employed in their preparation: for it is confidently affirmed by many phyſicians, and but faintly denied by ſome chymiſts, that the corroſive menſtruums made uſe of in the preparation of remedies, can never be ſo exquiſitely waſhed off from them, but that ſome of the ſalts will adhere to the medicines, and perniciouſly diſplay their corroſive nature in the body of him, that takes them. And it is not to be denied, but that many ignorant and venturous chymiſts do unſkilfully (and therefore dangerouſly enough) imploy corroſives ſometimes without any neceſſity or real advantage to invite them to it, and ſometimes without ſufficiently freeing their medicines from the corroding ſalts, by whoſe aſſiſtance they were prepared. For it is not always the frequency of ablution, though with warm water, that will ſuffice to carry off the ſalts from ſome bodies; and therefore thoſe great artiſts *Helmont* and *Paracelſus* preſcribe ſome things to be dulcified by the abſtraction of the water of whites of eggs (which, though it ſeemed inſipid, hath been found a great diſarmer of corroſive ſalts) and others by the frequent diſtillation of ſpirit of wine, which indeed (not to mention the balſamic parts it may leave behind) we have obſerved to have a faculty of carrying up with it the ſaline particles of ſpirit of vinegar adhering to ſome chymical remedies. But all this notwithſtanding, *Pyrophilus*, there may be ſeveral bodies (and perhaps more than are commonly taken notice of) which quite alter the nature of the acid ſalts employed to prepare them, by occaſioning thoſe ſalts to degenerate into another nature, upon the very act of corroding, or elſe by ſo aſſociating their own ſalts with one of thoſe of the diſſolving menſtruum, that from the coalition of both, there emerges a third body of differing in qualities from either. As in our experiment we find, that the ſpirit of petre, which is much more ſharp and corroſive than the ſtrongeſt diſtilled vinegar, and the fixed nitre, which is cauſtic like ſalt of tartar, and may, I ſuppoſe, well ſerve for a potential cautery (as ſurgeons ſpeak) do by their mutual action work themſelves into ſaltpetre, which is far enough from having any eminently fretting quality, and may be ſafely taken inwardly in a much greater doſe than either of its ingredients.

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* In ſome papers about the origin of living creatures ſuppoſed to ſpring of themſelves.

SECT. XXXVII.

How much corroſive ſalts may dulcify themſelves by corroding ſome bodies, you may eaſily try, by pouring diſtilled vinegar, or moderate ſpirit of vitriol, upon a competent proportion of corals, or crabs eyes, or pearls (or, as I ſuppoſe, almoſt any teſtaceous body). And for my part, though I am very ſhy of imploying corroſive liquors in the preparation of medicines; yet I have lately given a preparation of refined ſilver made with aqua fortis itſelf, or ſpirit of nitre, not only innocently, but with ſuch ſucceſs, that a couple of experienced phyſicians themſelves, that were troubled with a ſuperfluity of ſerous humours, ſent to requeſt it of me for their own uſe.

SECT. XXXVIII.

It were therefore worth while, in every preparation, where corroſive liquors are wont to be employed, or may ſeem requiſite to be ſo, to conſider the diſtinct nature of the particular bodies to be wrought upon, or conſult experience, whether or no the acid menſtruum do communicate to the concrete any particles capable of retaining their fretting quality after the end of the operation; or whether or no the ſalts do not ſo ſpend and tire themſelves in the act of corroding, that being as it were ſheathed, they become unable to corrode any further; or whether or no the menſtruum do not in the body to be corroded meet with ſome ſuch ſaline particles, as may with it conſtitute a new and inoffenſive ſubſtance, as when ſpirit of vinegar, by corroding calcined lead, is turned with it into a ſalt, not of an acid, but a ſaccharine taſte, ſuch as invited chymiſts to give it the name of ſugar of *Saturn*. In the former of theſe caſes the medicine may be dangerous, unleſs it be after the ſolution or corroſion ended exquiſitely dulcified from all remainder of the corroſive ſalts. But in the two latter caſes the remedies may, in ſpite of the corroſiveneſs of the menſtruums employed about them, be ſafe and innocent enough; for it matters not much, how ſharp and fretting the ſevered ingredients of a remedy were, provided the remedy itſelf reſulting from them be not ſo. And whereas it is objected, that in divers of theſe remedies the corroſive ſalts are not really deſtroyed, but only diſguiſed, becauſe by diſtillation it is poſſible to ſeparate from them the liquors uſed about them as corroſive as ever; it may eaſily be replied, that beſides that, in ſeveral medicines, the matter of fact will not hold in divers others, the objection built on it is much more ſpecious than ſolid: for it very little concerns us to be ſure, that out of the medicines we take or give, the violence of fire cannot ſeparate corroſive ſalts; provided we be duly ſatisfied, that no ſuch ſeparation can be made by the heat or juices of a human body. And therefore, though it have been affirmed, that tartarum vitriolatum would upon the urgent ſollicitation of a ſtrong fire, part with much of (that moſt fretting liquor upon animal ſubſtances) the oil of vitriol, that concurred to its production; yet our beſt and warieſt phyſicians, not only chymiſts but methodiſts, ſcruple not to give it inwardly in ſeveral conſtitutions and diſtempers. And to end this diſcourſe with the experiment, that began it, we clearly ſee; that ſalt-petre is frequently and innoxiouſly given inwardly, though the ſalt, that makes even aqua fortis ſo corroſive, be the principal ingredient of it, and may by diſtillation be driven from it.

SECT. XXXIX.

IT would not have been very uneaſy for me, *Pyrophilus*, to have added to divers particulars of the paſt diſcourſe, experiments and conſiderations tending to countenance or illuſtrate the reflections therein ſet down: but in the firſt place, I wanted leiſure to expatiate; in the ſecond place, I was unwilling to anticipate what I have to ſay to you in other eſſays, eſpecially expecting to have elſewhere occaſion to make mention of ſalt-petre. And beſides all this, I am (to tell you the truth) deſirous to impoſe on you a kind of neceſſity of proſecuting this experiment, further than, when I made it, I had opportunity to do. For as I am apt to think it may prove a noble one, ſo I am ſufficiently ſenſible of my having not yet been able to look into the bottom of it; and that very ſenſe of my own ignorance helped to keep me from lengthening your trouble in this eſſay, leſt by ſolemnly endeavouring to countenance my conjectures, I might be thought dogmatical in a haſty ſcribble, wherein it is much more my deſign to awaken and engage your curioſity. than acquaint you with my opinions. And yet I thought it not amiſs to mention the paſt conſiderations, ſuch as they are, partly becauſe this one inſtance ſeems ſo fairly to accommodate more than one notion of the intelligible philoſophy, which ſeems hitherto not to have ſo much as employed, much leſs produced, any ſtore of experiments; and partly, becauſe I would have you take notice, that more obſervables than one may ſometimes be very reaſonably ſought for in a ſingle experiment. And perhaps too, I was willing by my ſpending a whole eſſay upon one experiment, without allowing myſelf to wander often from it, to invite you to think with me, that experiments ought to be eſtimated by their value, not their number; and that a ſingle experiment, I ſay not ſuch as that the laſt eſſay treats of, but in general, ſuch as, it may be, may as well deſerve an entire treatiſe, as a great many leſs conſiderable ones. As one of thoſe large and orient pearls, that are fit to adorn a monarch's crown, may out-value a very great number of thoſe little (though true) pearls, that are to be bought by the ounce in goldſmiths and apothecaries ſhops.

SECT. XL.

Having newly met, *Pyrophilus*, with ſome ſmall treatiſes freſhly publiſhed by *Glauberus*, and not having now the leiſure to conſider, or indeed ſo much as to peruſe, much leſs the opportunity to make trial of divers particulars, which by turning over the leaves of the book, I find mentioned by him in relation to ſalt-petre, I muſt recommend to you the care of examining the particulars he delivers; and trying how far ſome of them may ſerve to correct, or to confirm, and how far others may be corrected by what has been in the paſt diſcourſe ſet down concerning ſalt-petre upon experiments; ſome of whoſe fruits I can yet ſhew you, which were made upon the account of the diviſibleneſs of nitre into fixed and volatile parts, long before the publication of *Glauber*'s treatiſes.

THE

HISTORY

OF

FLUIDITY and FIRMNESS.

Advertiſements concerning the following Treatiſe.

THAT the author in thoſe animadverſions upon the *Eſſay touching Salt-petre*, whereof the enſuing treatiſe makes a part, might with the more freedom and conveniency add, alter, and even retract as he ſhould ſee cauſe, he thought fit to write them, as if they were made on the work of another.

THE author hopes, that the equitable reader, conſidering that the following particulars touching fluidity and firmneſs were firſt written but by way of annotations upon the beginning of the above-mentioned eſſay, will excuſe the unaccurateneſs of the method, as a fault ſcarce evitable on that occaſion. It is alſo hoped, that if the reader will remember, that he was told in the preface to the newly-mentioned eſſay, that moſt of thoſe, whom to gratify that treatiſe and the enſuing notes on it were written, were addicted to the Epicurean philoſophy; the author's explicating things chiefly according to the atomical principles will not be thought ſtrange, nor be looked upon as a ſure argument of his being wedded to the particular opinions, wherein the Atomiſts differ from other modern Naturaliſts: eſpecially, ſince he has on ſome occaſions plainly enough intimated the contrary, by propoſing, together with the atomical ways of reſolving a thing, another explication more agreeable to the Carteſian, or ſome other modern hypotheſis.

THE following tract was entitled, *A Hiſtory of Fluidity and Firmneſs*, becauſe indeed the having ſet down experiments and other matters of fact relating to the ſubjects treated of is the main, though not the only thing the author dares pretend to have done in it. And he ſtyles the hiſtory, as it now comes abroad, *Begun*; partly, becauſe he would invite abler pens to contribute their obſervations towards the compleating of what he is ſenſible he has but begun; and partly becauſe he may hereafter if God permit, do ſomething of that kind himſelf.

AND laſtly, the author, though troubled that he can do it, dares not but advertiſe the reader, that ſome pages, partly a little after the beginning, and partly about the middle of the following treatiſe, having been loſt through the negligence or miſtake of him, to whoſe care the ſheets, whereon it was written, were committed; he fears he has not been able, otherwiſe than very lamely and imperfectly, to repair that loſs out of his memory.

12.29
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THE

Hiſtory of FLUIDITY and FIRMNESS.

The FIRST PART.

Of FLUIDITY.

SECTION I.

WHETHER philoſophers might not have done better in making fluidity and firmneſs rather ſtates than qualities of bodies, we will not now examine. But under which ſoever of the two notions we look upon them, it is manifeſt enough, that they are to be reckoned amongſt the moſt general affections of the conventions or aſſociations of ſeveral particles of matter into bodies of any certain denomination, there being ſcarce any diſtinct portion of matter in the world, that is not either fluid, or elſe ſtable or conſiſtent. And therefore, I preſume, it may be well worth while to conſider, what may be the general cauſes of theſe two ſtates, qualities, or affections of matter; and to try, whether by aſſociating chymical experiments to philoſophical notions, there may not be given at leaſt a more intelligible and more practical account of both theſe ſubjects, than has been hitherto afforded us by the doctrine of the ſchools, which is wont to appear very unſatisfactory to diſcerning men; many of whom look upon what is wont to be taught by the Peripatetics, concerning fluidity and firmneſs, as well as other qualities, to be partly too general to teach us much, and partly too obſcure to be underſtood. And that which at preſent invites us to this inquiry is, chiefly, that ſome circumſtances of our author's experiment, touching ſalt-petre, may afford us ſome uſeful aſſiſtance in our deſigned ſearch. For though the chief phænomena and circumſtances of the experiment may be thought principally to reſpect fluidity; yet ſince that and firmneſs are contrary qualities, and ſince it is truly, as well as commonly, ſaid, that contraries ſurveyed together ſerve to illuſtrate each other, it may reaſonably be hoped, that what the circumſtances juſt now related may give to the nature of fluidity, may facilitate the knowledge of that of compactneſs: nevertheleſs, we ſhall often be obliged to treat of theſe two qualities together, becauſe the experiments we are to produce do many of them relate to both.

SECT. II.

A BODY then ſeems to be fluid, chiefly upon this account, that it conſiſts of corpuſcles, that touching one another in ſome parts only of their ſurfaces (and ſo being incontiguous in the reſt) and ſeparately agitated to and fro, can by reaſon of the numerous pores or ſpaces neceſſarily left betwixt their contiguous parts, eaſily glide along each other's ſuperficies, and by reaſon of their motion diffuſe themſelves, till they meet with ſome hard or reſting body; to whoſe internal ſurface, by virtue of that motion, their ſmallneſs, and either their gravity, or ſomething analogous or equivalent to it, they exquiſitely, as to ſenſe, accommodate themſelves.

SECT. III.

WHAT notion *Epicurus*, and the ancient Atomiſts his followers, had of fluid bodies, may be learned from theſe verſes of his paraphraſt *Lucretius:*

Illa autem debent ex lævibus atque rotundis
Eſſe magis, fluido quæ corpore liquida conſtant.
Nec retinentur enim inter ſe glomeramina quæque,
Et procurſus item in proclive volubilis extat.

And indeed, it is probable enough, that in divers liquors the little ſurfaces of the component particles are ſmooth and ſlippery, and that their being ſo does much facilitate the gliding of the corpuſcles among themſelves; and conſequently, the fluidity of the body they compoſe. Nor is it to be denied, that the ſpherical figure of ſuch corpuſcles may alſo conduce to their eaſy rolling upon one another: but there are divers other figures, which may make the little bodies endowed with them voluble enough to conſtitute a fluid ſubſtance. And the other qualities to be met with in divers liquid ſubſtances, and even in water itſelf, and oil, ſeem to argue their parts to be otherwiſe ſhaped; and thoſe fluid bodies, which are not liquors, as air and fire, ſeem to be compoſed of particles not all or moſt of them round, but of very various, and ſometimes of very irregular figures; and yet that ſuch bodies deſerve to be called fluid ones, will be manifeſt anon. And that they make a much more conſiderable part of the univerſe, than thoſe that are wont to be called liquors, may be argued from hence, that except the earth, the planets, and perhaps too the fixed ſtars, the reſt of the world, as vaſt as it is, ſeems to conſiſt chiefly, if not only, of an ætherial, thin, and fluid ſubſtance, as may appear (to omit other arguments) by what later aſtronomers have obſerved concerning the free and unreſiſted motion of ſuch comets as have by a trajection through the æther, for a long time wandered through the celeſtial or interſtellar part of the univerſe.

SECT. IV.

AND here let us obſerve, that it is not neceſſary to the fluidity of a body, nay, nor to its appearing fluid to the eye itſelf, that the corpuſcles it conſiſts of be crouded as cloſe together as they are wont to be in water, and other bodies, that are commonly looked upon as the only liquors. For though a parcel of matter no bigger than a grain of corn, being rarified into ſmoke, will poſſeſs an incomparably greater ſpace than it did before; and though, if a body be further rarified into flame, its expanſion will be yet much greater: yet both ſmoke and flame may be ſo ordered, as to appear like liquors. We have practiſed divers ways, to make the fumes of bodies acquire a viſibly-level ſuperficies like water; but the eaſieſt, though not perhaps the beſt, is this, (part of which I remember I have ſeen performed as a kind of trick by a very ingenious perſon:) The mouth being filled with the ſmoke of roſemary (that happening to be at hand when I made the experiment) if this ſmoke be plentifully blown into a glaſs pipe of an indifferent ſize, and open at both ends; and if when it is well filled with ſmoke, the lower end be preſently ſtopped, and the glaſs be kept ſtill a while in an erected poſture, the fumes will ſettle by degrees to a level ſuperficies like water: ſo that, though you gently incline the pipe any way, the upper ſurface of the ſmoke will nevertheleſs quickly grow parallel to the horizon. And if the glaſs be further (but ſlowly) made to ſtoop, the ſmoke will ſeem to run down in a body like water, whilſt it continues in the pipe; though when it is come to the lower end of it, inſtead of dropping down like water, it will commonly rather fly upwards, and diſperſe itſelf into the air. And as for flame, I foreſee I ſhall ere long have occaſion to mention an experiment, whereby I have ſometimes endeavoured to ſhew, that even two contiguous flames, as expanded bodies as they are, and as open as their texture is, may like viſible fluids of a differing kind retain diſtinct ſurfaces.

SECT. V.

BUT inſtead of examining any further, how many bodies are or may be made viſibly to appear fluid ones; let us now reſume the conſideration of what it is that makes bodies fluid: eſpecially, ſince having intimated ſome of the reaſons, why we are unwilling to confine ourſelves to the Epicurean notion, we hope it will the leſs be diſliked, that we thought fit to make ſuch a deſcription of a fluid ſubſtance, as may intimate, that we conceive the conditions of it to be chiefly theſe three.

THE firſt is the littleneſs of the bodies that compoſe it: for in big parcels of matter, beſides the greater inequalities or roughneſſes, that are uſual upon their ſurfaces, and may hinder the eaſy ſliding of thoſe bodies along one another; and beſides that divers other affections of a fluid body cannot well belong to an aggregate of groſs lumps of matter; beſides theſe things, I ſay, the bulk itſelf is apt to make them ſo heavy, that they cannot be agitated by the power of thoſe cauſes (whatever they be) that make the minute parts of fluid bodies move ſo freely up and down among themſelves: whereas it would ſcarce be believed, how much the ſmallneſs of parts may facilitate their being eaſily put into motion, and kept in it, if we were not able to confirm it by chymical experiments. But we ſee that lead, quickſilver, and even gold itſelf, though whilſt they are of a ſenſible bulk, they will readily ſink to the bottom of aqua regis, or any other ſuch liquor; yet when the menſtruum has corroded them, or fretted them aſunder into very minute parts, thoſe minute corpuſcles grow then ſo much more capable of agitation than before, that quitting the bottom of the liquor, they are carried freely every way, and to the top, with the aſſociated parts of the liquor, without falling back again to the bottom, Nay, we ſee, that ponderous and mineral bodies divided into corpuſcles ſmall enough may be made ſo light and voluble, as to become ingredients even of diſtilled liquors; as we may learn by what ſome chymiſts call the butter, others (ſimply) the oil, and others the *oleum glaciale* of antimony; which, though it be after rectification a very limpid liquor, yet does in great part conſiſt of the very body of the antimony, as may appear (not to mention its weight) by this, that it is moſt eaſy to precipitate out of it with fair water ſtore of ponderous white calx, reducible by art to an antimonial glaſs. Nay, we make a menſtruum, with which we can eaſily at the firſt or ſecond diſtillation bring over gold enough, to make the diſtilled liquor appear and continue enobled with a golden colour.

AND to ſhow yet more particularly, that great bodies are too unwieldy to conſtitute fluid ones; we may further obſerve, how as well nature as art, when either of them makes bodies of conſiderable bulk fluid, is wont, in order thereunto, to make a comminution of them, as we may obſerve in divers examples.

SECT. VI.

THUS we ſee, that in the ſtomachs of dogs, nature, to reduce bones into thoſe fluid bodies, chyle and blood, does by ſome powerful and appropriated juice, whether belonging to the ſtomach itſelf, or thrown out of the arteries in the paſſage of the circulating blood) diſſolve them into parts ſo minute, that the acuteſt eye would not tempt a man to ſuſpect, that ſuch a liquor had ever been a bone. And that it may not be objected, that this diſſolution is chiefly performed, or at leaſt muſt always be aſſiſted by the liquor, which animals take into their ſtomachs by drinking; I ſhall repreſent not only, that we find by experience, how little common water, the only uſual drink of dogs, wolves, *&c.* is able to diſſolve bones, though they be very long not macerated, but boiled in it; but that (if we may believe natural hiſtorians and credible travellers) there are ſome ſorts of animals, as particularly camels, that may be brought not to drink once in many days, even when they travel in hot climates. And to make you think this the leſs improbable, I ſhall add, that I am familiarly acquainted with an ingenious gentleman, who, as himſelf and an ancient virtuoſo, in whoſe houſe he lives, have informed me, does uſually drink but once in ſeveral days, and then no exceſſive draught neither. And when I aſked him, how long he had actually abſtained not barely from drink, but from thirſting after it? he anſwered, that he had once (ſome few years before) continued about nine days without either taking or needing any drink; and he doubted not, but that he might have continued much longer in that ſtate, if by diſtempering himſelf one night with long and hard ſtudy he had not had ſome light inclination to take a ſmall draught, which ſerved him for about four days longer. And when I aſked him, whether that hot ſummer's day, that preceded the evening, wherein he happened to tell me this, he had not drunk at all? he anſwered negatively. And it adds to the ſtrangeneſs of this peculiarity, that this gentleman is in the flower of his youth, being but about twenty-two years of age, and of a ſanguine and florid complexion. And (to annex that alſo upon the by) I learned by inquiry from him, that he ſweats freely enough, as I remember I ſaw him do; that his diet is the ſame with other men's, without reſtraining him from the free uſe of ſalt meats; and that his urine is in quantity much like that of ordinary men of his age and temperament. But to return to what I was ſaying more generally of the ſtomachical menſtruum of animals; I ſhall add on this occaſion, that to make ſome kind of imitation of it, I prepared, and do elſewhere mention and teach a certain liquor, that I uſe, whereby I have in a ſhort time, and without fire, diſſociated the parts of roaſted or boiled fleſh, bread, fruit, *&c.* and pulled them aſunder into very minute bodies, whereby I have reduced ſometimes one, ſometimes another of them, together with the menſtruum (which needs not much exceed them in bulk) to the conſiſtence of a fluid body.

WE ſee likewiſe, that fuſion makes metals fluid, and in fuſion there is manifeſtly a comminution of the melted body; the heat alone of gold, ſilver, or iron, though increaſed even to ignition, being not able to make thoſe metals become fluid, whilſt they continue in maſſes of any ſenſible bulk. To which I ſhall add anon, that even melted metals may have their fluidity increaſed by a yet further comminution of their parts.

SECT. VII.

AND to reſume here the conſideration of that very difficult queſtion, which we have elſewhere mentioned, it ſeems well worth inquiry, whence it happens, that in the diſtillation of common ſalt and other ſaline bodies, which not only are not fluid, but are hard even to brittleneſs, there will yet be obtained a perfect and permanent liquor, and from ſome of them a very conſiderable proportion of it. In anſwer to this queſtion it may indeed be ſaid, that in divers dry bodies, ſuch as harts-horn, wood and bones, committed to diſtillation, the fire does no more than ſeparate the aqueous or other liquid parts from the others, wherewith they were blended in the concrete, and bring them together into the receiver, where they convene into a liquor. But beſides that this itſelf is perhaps more eaſily ſaid than proved, it does not reach the propounded difficulty: for with what probability can it be affirmed of bodies, that have been already calcined or melted? ſuch as are the red calx of vitriol, and fluxed ſea-ſalt, *&c.* which yet afford liquors, though their aqueous and other looſer parts have been already driven away by a ſtrong fire before their being expoſed to diſtillation. I have ſometimes then conſidered, whether it may not ſeem leſs improbable to conjecture, that the vehement agitation produced in ſuch bodies by the violence of heat does both divide them into minute corpuſcles, and drive over ſwarms of them into the cold receiver, where loſing their former vehemence of agitation, they are reduced into a liquor, chiefly (for I would not exclude concurrent cauſes) by reaſon that the fire happened to rend the concrete into parts, by their extreme littleneſs, or their ſhape, or both, ſo eaſy to be tumbled up and down, that the wonted agitation of the air, propagated by the interpoſed bodies or medium, or elſe that the ſame cauſe, whatever it be, that gives the air its wonted agitation, is able to give ſuch minute corpuſcles enough of it to keep them fluid.

SECT. VIII.

THAT there is conſtantly in the air a various motion of the ſmall parts, will be anon declared. That alſo ſome bodies will be kept fluid by a much leſs meaſure of agitation than is requiſite to others, ſeems probable from hence, that wine will continue a liquor in ſuch a languid warmth of the air, as will not keep the parts of water moving, but permit them to reſt in the form of ice. And in cold countries, where wine itſelf would congeal, (as I have by art made it do here in *England)* it is obſerved, that though the more aqueous parts will by the loſs of their motion be turned into ice, yet the more ſubtile and ſpirituous parts remain unfrozen; and ſo do divers other liquors (eſpecially chymical) of very ſubtle and voluble parts. And the corpuſcles, that chiefly compoſe that body, which is properly called the air, though it appears by weather-glaſſes, that cold may very much contribute to condenſe it, (that is, to occaſion the approach of its parts to one another, or, reduce them to a cloſer order) have not been obſerved to be frozen by any degree of cold whatſoever; which ſeems to proceed from hence, that by reaſon of their extreme littleneſs, (not excluding their figure) there cannot be ſo little of agitation about the earth, as not to be ſufficient to continue bodies, and conſequently to keep them fluid.

Now, that likewiſe it is poſſible, that a ſaline ſpirit ſhould conſiſt at leaſt in great part of very minute grains of ſalt, we elſewhere declare; where it is taught, that a ſal-armoniac may be made by ſpirit of urine and ſpirit of ſalt, as the common ſal-armoniac is made with crude ſalt: and there a way is alſo ſhewn, how theſe two ſalts, (the urinous and the other) as ſtrictly as they are united in the compound, may be readily divorced. And agreeably to this I obſerve, that as (according to what I elſewhere note) a common aqua fortis may be enabled to diſſolve gold, (on which of itſelf it will not faſten) by the addition of ſpirit of ſalt; ſo I find, that common crude ſalt barely diſſolved in it will give it the like power of working upon gold. Nay, I have tried, that crude nitre, diſſolved in good ſpirit of ſalt, may make it ſerve for an aqua regis. And I remember on this occaſion, that having enquired of the moſt noted perſon in *Holland* for the diſtilling of corroſive waters, what was the greateſt proportion of diſtilled liquors, that ever he was able to obtain from ſea-ſalt; he (though a man not given ſo much as to boaſting) affirmed to me, that by uſing inſtead of the ordinary *Caput mortuum*, as brick-duſt, ſand, *&c.* (that chymiſts are wont to mingle with ſalt before they diſtil it) a certain whitiſh clay, he had ſometimes brought over almoſt the whole body of ſalt into a liquor; inſomuch that from a pound of ſalt he could draw, and that without any extraordinary trouble or degree of fire, fourteen ounces of liquor. And when, becauſe I ſuſpected, that much of this might be water forced from the clay mingled with the ſalt, I enquired, whether he had ever dephlegmed this liquor; he anſwered me, that he had purpoſely done it; and ſometimes found no leſs than about twelve ounces of it to be ſtrong rectified ſpirit: which brought into my mind that almoſt incredible paſſage of *Beguinus*, who ſomewhere teaching the diſtillation of another ſalt, adds to the end of his directions, that if you have wrought well, you ſhall get from a pound of the matter a pound of ſpirit. But becauſe from all theſe liquors, diſtilled from ſuch kind of ſalts, it is poſſible either by rectification, or ſome more philoſophical way, to obtain a portion of phlegm or water, I leave it to further enquiry, whether or no the fluidity of theſe diſtilled liquors may not in divers caſes be in part furthered by the mixture of ſome particles of an aqueous nature, (ſuch being fit to make diſſolvers and vehicles for ſalts) which may not abſurdly be ſuſpected to have been produced by the action of the fire, upon the concrete committed to diſtillation; if we allow, with that famous chymiſt *Helmont*, that by the alkaheſt all groſs bodies may be totally, and that, without it, even oil and ſalt may, in great part (and that without additaments) be reduced into inſipid water.

SECT. IX.

We ſhall anon, (when we come to treat of firmneſs) mention our having made a certain ſubſtance ſo diſpoſed to fluidity, that it may be made to change the ſtable conſiſtence for a liquid one, by ſo ſmall an agitation, as only the ſurpluſage of that, which the ambient air is wont to have about the middle even of a winter's day, above what it hath in the firſt or latter part of it. Nay, we have made even a metalline ſalt or vitriol capable of this proclivity to liquefaction, of which we have unqueſtionable witneſſes. And therefore, it need not appear incredible, that other heaps or aggregates of corpuſcles much lighter than theſe, though heavier than thoſe of the air, may have all their parts ſo minute and fitted for motion, that the wonted agitation of the air may not only about noon, but at all other times of the day, keep them in motion, and thereby in the ſtate of fluidity.

SECT. X.

AND here I muſt add, that it was not altogether without cauſe, that I lately took notice of the ſhapes as well as the ſizes of bodies, in reference to their fitneſs to conſtitute fluid ones. For though I be not ſure, but that in thoſe bodies, as ſal-armoniac, antimony, *&c.* which are by the fire ſublimed into flowers rather than diſtilled into liquors, the magnitude of the component corpuſcles may not be a hinderance to the fluidity of the body they conſtitute; yet this ſeems as probably referable to their figure, unapt for the requiſite motion, as to their bulk. And I have ſometimes made to this purpoſe this experiment; that by ſlowly diſtilling oil-olive *per ſe* in a glaſs-retort, (placed in ſand) I found, as I expected, that about the third part of the oil, which was driven over into the receiver, did there coagulate into a whitiſh body almoſt like butter. So that although it ſeemed manifeſt by the ſtrong ſmell and very piercing taſte of this white ſubſtance, that the oil, which afforded it, had its particles, as it were, torn in pieces; and though diſtillation be wont to obtain liquors even from conſiſtent bodies; yet in our experiment of a concrete, that is naturally fluid, the diſtilled liquor itſelf proves not to be ſo: of which no cauſe ſeems more obvious, than that the newly-acquired ſhape of the diſſipated parts of the oily corpuſcles makes them unfit for motion; either abſolutely ſpeaking, or at leaſt in reſpect of one another, by making them leſs pliant than formerly, or giving them a figure more eaſy to be entangled with the neighbouring corpuſcles, or elſe by making their ſurfaces leſs ſmooth and ſlippery than before.

SECT. XI.

BUT to return thither, whence we have digreſſed, and mention ſome more familiar examples of the conduciveneſs of the ſmallneſs of a body's disjoined parts to its fluidity, we may take notice, that of bodies, that conſiſt of incoherent parts, and are made up, as it were, by aggregation, thoſe *de cæteris partibus*, in their being poured out, moſt reſemble liquors, that are the ſmalleſt; as would appear upon the emptying ſeveral ſacks, the one of apples, the other of walnuts, the third of filberts, the fourth of corn, the fifth of ſand, and the ſixth of flour.

CONFECTIONERS alſo, cooks, and others, that make much uſe of whites of eggs, will eaſily reduce thoſe clammy and vicious bodies into a thin and fluid ſubſtance, to which, for its affinity with water, many give the ſame name: and yet this difference of fluidity being effected only by long and ſkilfully beating the maſs with a whiſk, or even with a ſpoon, ſeems to be produced but by pulling aſunder the parts (which, perhaps, before were long and ſomewhat twined) and breaking them into a ſhorter or leſſer, and conſequently more voluble ones. And I remember, I have ſeen a good quantity of that jelly, that is ſometimes found on the ground, and by the vulgar called a ſtar-ſhoot, as if it remained upon the extinction of a falling ſtar, which being brought to an eminent phyſician of my acquaintance, he lightly digeſted it in a well-ſtopt glaſs for a long time, and by that alone reſolved it into a permanent liquor, which he extols as a ſpecifick to be outwardly applied againſt wens.

SECT.

SECT. XII.

AND here we will ſubjoin an obſervation afforded us by the art of caſting, which has ſometimes yielded us a not unpleaſant diverſion. It is obſerved then by goldſmiths well verſed in that art, (and has been recommended to me by an artificer eminently ſkilful in it) as one of the chief remarks belonging to it; that when any ſuch curious work of ſilver is to be caſt, as requires, that the impreſſion of hairs or very ſlender lines be taken off by the metal, it is not enough, that the ſilver be barely melted; but it muſt be kept a conſiderable while in a ſtrong fuſion: for if it be too ſoon poured out, the figure it will make will be but blunt; whereas if it be kept a competent time in fuſion, the matter becoming thereby more liquid as well as hotter, will be thin enough to run into the ſmalleſt cavities of the mould, and ſo receive a figuration even from the delicateſt of them. Whence it may probably be deduced, that ſome bodies already fluid may, by a further comminution of their parts, be made yet more fluid. The like increaſe of fluidity may be obſerved in ſome other fluid bodies, eſpecially unctuous ones, as turpentine, oil, *&c.* when heat begins to break as well as agitate their parts.

I MAY elſewhere have occaſion to mention, how, by the operation of the fire, the cryſtalline ſalt of urine may be reduced without additaments to a ſtrong and ponderous liquor: though in this, as perhaps alſo in ſome of the former inſtances, it is not unlikely, that (as we may hereafter more particularly declare) there may concur to the produced change of conſiſtence, ſome alteration in the figure of the corpuſcles, whereof the firm body conſiſted.

AND if that be true, which *Helmont* in ſeveral places affirms of his prodigious liquor, alkaheſt, it is poſſible to turn plants, animals, ſtones, minerals, metals, or whatever kind you pleaſe of conſiſtent body here below, into a liquor equiponderant to the reſolved concrete: which (if granted) ſeems to argue, that the moſt ſolid body, by being divided into parts ſmall enough to be put into motion by the cauſes, that keep thoſe of water and other liquors in agitation, may become fluid bodies. And this intimation I ſhall add for the ſake of philoſophers, that barely by long digeſtions, (and much more if they be helped by ſeaſonably-repeated diſtillations) in exactly ſtopped veſſels, and a due degree of heat, there may be made in the parts of many bodies, both vegetable and animal, ſo great a change from the ſtate of conſiſtence to that of fluidity, as thoſe, that contenting themſelves with ordinary courſes of chymiſtry, have not had a peculiar curioſity for trials of this nature, will not be forward to expect.

SECT. XIII.

THE ſecond of the abovementioned three conditions is, that there be ſtore of vacant ſpaces intercepted betwixt the component particles of the fluid body, or at leaſt about thoſe of them, that are ſuperficial: for without this there will not be room for each of the corpuſcles to continue its agitation upon the ſurfaces of the neighbouring ones; and there would be no ceſſion of any, becauſe there would be no place unpoſſeſſed for the impelled corpuſcle to be received in. But when I ſpeak of vacant ſpaces, ordinarily, (if not always) requiſite to be intercepted betwixt the particles of fluid

bodies;

bodies; I intend not to determine, whether or no ſuch ſpaces ſhould or may be vacuities properly ſo called; it being commonly ſufficient to this ſecond condition of a fluid body, that in the little ſpaces intercepted between thoſe that either are, or at leaſt are conſidered as ſolid parts, there be none but ſuch as will eaſily yield to them, and cannot conſiderably reſiſt the freedom of their motions.

WHICH being premiſed to keep this condition from being miſtaken, we may in confirmation of it take notice, how ſnow, which at its firſt falling is of a looſe and open texture, does eaſily yield to the impreſſions of the hand: but when by being ſtrongly compreſſed and formed into balls, the little icy bodies it conſiſts of are brought into a cloſer order, and many of them thruſt into the little ſpaces formerly poſſeſſed only by the yielding air, they become unable to give way to the motions of our hand as before, but compoſe a hard and reſiſting body. We ſee alſo, that when water is ſtrongly forced into and kept compreſſed in a bladder, ſo that its exterior particles have not about them as before the yielding air to give way to them, when they ſhould, according to their wont, ſwell about the ſides of the bodies that endeavour to preſs it inwards, it emulates a hard body, and reſiſts ſuch motions as otherwiſe it would readily yield to; unleſs a more eaſy ceſſion be occaſioned by the retching of the moiſtened bladder itſelf.

AND I chuſe to inſtance in a bladder diſtended with water, rather than in one full of air, becauſe, though this latter will alſo emulate a hard body, yet in this caſe the tenſion of the bladder would perhaps be aſcribed to a kind of ſpring, which divers experiments have taught us to belong to the air: whence it might be ſaid, that ſince the incloſed air will ſuffer itſelf to be thruſt inward a good way, though it will quickly when permitted fly out again; the hardneſs of a well-blown bladder proceeds not from want of the rooms requiſite to the ceſſion of the aërial corpuſcles, but to the motion of reſtitution natural to them; when like an innumerable company of little bows or ſprings, being bent by the force that compreſſes the ſides of the bladder, they do, as ſoon as it is taken off, ſtretch themſelves out again (ſome one way, ſome another) as far as is permitted them by the impriſoning bladder, which they thus every way keep ſtrongly diſtended.

BUT this having of vacant ſpaces, or ſome yielding matter about the corpuſcles of a fluid body, ſeems requiſite to its being ſo, but as what in a ſchool-term one may call a *Removens prohibens*; I mean, only as it obviates that impediment to their motion, which exquiſite fulneſs may be conceived to give to the various glidings amongſt themſelves of the parts of a body ſuppoſed to be perfectly of the ſame hardneſs or ſoftneſs, or, if you pleaſe, altogether equally diſpoſed or indiſpoſed to yield to one another. And although in ſuch bodies, as water, wine, oil, quickſilver, and the like, that are generally agreed upon to be fluid liquors, it will I preſume be granted, that this ſecond condition we have been ſpeaking of may take place; yet I will not ſay, that it were altogether abſurd to queſtion, whether there may not be a portion of matter conſiſting of parts ſo minute, and ſo agitated, and conſequently ſo eaſy to be either crumbled into yet ſmaller parts, or ſqueezed into any figure as occaſion requires, that they may inceſſantly change places among themſelves, and thereby conſtitute a moſt fluid body, without any vacuities, receptacles, or yielding matter about them, unleſs perhaps it be about the exterior parts of thoſe of them, that from time to time happen to be the ſuperficial corpuſcles of this thinneſt liquor. But though we have ſaid, that this may be queſtioned without abſurdity, yet it will not ſo much concern us in this place to examine, whether the affirmative may be rationally maintained, as to proceed to conſider what is farther requiſite to that ſtate of matter we are now treating of, eſpecially the qualification yet unmentioned ſeeming to be the principal of all.

SECT. XIV.

For the third and chief condition of a fluid body is, that the particles it conſiſts of be agitated variouſly and apart, whether by their own innate and inherent motion, or by ſome thinner ſubſtance, that tumbles them about in its paſſage through them. For this ſeems to be the main difference betwixt ſolid ice and fluid water, that in the one the parts (whether by any newly-acquired texture, or for want of ſufficient heat to keep them in motion) being at reſt againſt one another, reſiſt thoſe endeavours of our fingers to diſplace them, to which in the other, the parts being already in motion, eaſily give way. For whereas in the ice, every part actually at reſt muſt by the law of nature continue ſo, till it be put out of it by an external force capable to ſurmount its reſiſtance to a change of its preſent ſtate; in water each corpuſcle being actually (though but ſlowly) moved, we need not begin or produce a new motion in it, but only byaſs or direct that, which it has already, which many familiar inſtances maniſeſt to be a much eaſier taſk. From this agitation of the ſmall parts of liquors it comes to paſs, that theſe little bodies, to continue their motion, do almoſt inceſſantly change places, and glide ſometimes over, ſometime under, and ſometimes by the ſides of one another. Hence alſo may be rendered a reaſon of the ſoftneſs of fluid bodies, that is, their yielding to the touch: for the particles that compoſe them being ſmall, incoherent, and variouſly moved, it can be no difficult matter (as we lately intimated) to thruſt them out of thoſe places, which being already in motion they were diſpoſed to quit, eſpecially there being vacant rooms at hand, ready to admit them as ſoon as they are diſplaced. And hence it likewiſe happens, that theſe little bodies muſt be very eaſily moveable any way upon the motion of the maſs or liquor, which they compoſe; and that being very ſmall, and moving ſo many ways, they cannot but (according to *Ariſtotle*'s definition of things fluid) be very unfit to bound themſelves, but very eaſy to be bounded by any other firm body; for that hinders them from ſpreading any further: and yet to continue their various and diffuſive motion as much as they can, (eſpecially their gravity, at leaſt here about the earth, equally depreſſing and thereby levelling as to ſenſe their uppermoſt ſuperficies) they muſt neceſſarily move to and fro, till their progreſs be ſtopped by the internal ſurface of the veſſel, which by terminating their progreſs (or motion toward the ſame part) does conſequently neceſſitate the liquor thoſe little bodies compoſe, to accommodate itſelf exactly (for aught the eye is able to diſcern to the contrary) to its own figure.

SECT. XV.

This ſhort and general account of fluidity may, we hope, be as well further explicated and illuſtrated, as confirmed, by the following inſtances and experiments; and therefore we ſhall forthwith proceed to them.

And it will be fit to mention in the firſt place thoſe, that are afforded us by the body our author treats of, ſalt-petre; they having occaſioned our writing about this ſubject.

SALT-PETRE then may be made fluid two ſeveral ways, either by, or without a liquor.

BY the intervention of a liquor it puts on the form of a fluid body, when being diſſolved in water or aqueous juices, it is not by the eye diſtinguiſhable from the ſolvent body, and appears as fluid as it; which ſeems to proceed from hence, that the agitated particles of the water piercing into the joints or commiſſures of the corpuſcles of the ſalt, do disjoin them, and thereby divide the nitre into parts ſo ſmall, that it is eaſy for thoſe of the water, wherewith they are aſſociated, not only to ſupport them, but move them to and fro: whence it comes to paſs, that theſe particles being ſo ſmall, and ſwimming ſome one way, ſome another in the yielding body of water, make no ſuch reſiſtance againſt the motion either of a man's hand, or other external body, that ſtrives to diſplace them, as they did in their ſaline form.

BUT that with much leſs liquor a nitrous body may be rendered fluid, may appear to him, that ſhall expoſe ſuch fixed nitre, as our author teaches to make, to the moiſt air of a cellar: for there it will run *per deliquium*, (as chymiſts ſpeak) into a liquor, which conſiſts of no more aqueous particles, than are neceſſary to keep the ſaline ones (which ſeem to be much ſmaller than thoſe of unanalyzed nitre) in the agitation requiſite to fluidity.

SECT. XVI.

AND hence we may proceed to conſider, what fluidity ſalt-petre is capable of without the intercurrence of a liquor: and this may be two-fold. For firſt, if it be beaten into an impalpable powder, this powder, when it is poured out, will emulate a liquor, by reaſon that the ſmallneſs and incoherence of the parts do both make them eaſy to be put into motion, and make the pores they intercept ſo ſmall, that they ſeem not at a diſtance to interrupt the unity or continuity of the maſs or body. But this is but an imperfect fluidity, both becauſe the little grains or corpuſcles of ſalt, though eaſily enough moveable, are not always in actual motion; and becauſe they continue yet ſo big, that both they and the ſpaces intercepted betwixt them are, near at hand, perceivable by ſenſe. But if with a ſtrong fire you melt this powdered nitre, then each of the ſaline corpuſcles being ſub-divided into I know not how many others, and theſe inſenſible parts being variouſly agitated by the ſame heat, (both which may appear by their oftentimes piercing the crucible after fuſion, wherein they lay very quietly before it) the whole body will appear a perfect liquor, and be thought ſuch by any beholder, that ſhall judge of it but by the eye: and ſuch alſo is the fluidity of melted metals, in which, when they are brought to fuſion in vaſt quantities, I have ſeen the ſurface waved like that of boiling water, and ſometimes parcels of liquor thrown up a pretty way into the air. And not only fire, and other actually and manifeſtly hot bodies, are able to make ſome hard ones fluid, but it ſeems alſo, that ſome bodies may be brought to fluidity by others, which to the touch appear cold, if they be but fitted to change the texture of the hard body, and put its inflected parts into a convenient motion; as may be ſeen in the chymical experiment of turning the brittle body of camphire into an oil for the time, by letting it lie upon aqua fortis, which perhaps blends and complicates the formerly rigid particles, and puts them into ſuch a motion, that they do as well glide along as ſomewhat twine about each other. And I further tried, (not having found it mentioned by the chymiſts) that camphire may by a dexterous application of heat be brought in cloſe glaſſes, both to flow, and to boil almoſt like oil. It is true, that theſe liquors taken from the fire quickly loſe that name, and grow ſolid again. But the duration of a thing is not always neceſſary to denominate it ſuch; for the leaf of a tree, for inſtance, whilſt it flouriſhes, may be as truly green as an emerald, though the leaf will after a while wither and turn yellow, which the ſtone will never do: and in cold climates, where lakes, *&c.* at other times navigable, are ſometimes frozen ſo hard, that carts and even great ordnance may ſafely be drawn over them, ice and water are the one a ſtable, and the other a liquid body, notwithſtanding that the ſame portion of matter, which at one time is frozen into a hard and ſolid ſubſtance, was a little before a fluid body, and (now and then in a very ſhort time) will be thawed into a liquor again.

SECT. XVII.

I KNOW not whether it be requiſite to take notice, that the fluidity, which ſalt-petre acquires upon fuſion by fire, ſeems very much of kin to that, which is acquired by ſolution in water. But if fuſion be made rather by the ingreſs and tranſcurſions of the atoms of fire themſelves, than by the bare propagation of that motion, with which the agitated particles that compoſe fire beat upon the out-ſide of the veſſels, that contain the matter to be melted; in ſuch caſe, I ſay, both thoſe kinds or manners of fluidity newly aſcribed to ſalt-petre will appear to be cauſed by the pervaſion of a foreign body: only in diſſolution the fluid body is a viſible and palpable liquor, and conſequently more groſs, whereas in fuſion the fluid ſubſtance that permeates it is more thin and ſubtile, and divides it into much ſmaller parts, and ſo adds very little to its bulk.

SECT. XVIII.

BUT becauſe ſome ſcruple may poſſibly ariſe about this matter from hence, that the powder of nitre, how fine ſoever, ſeems fluid but juſt whilſt it is pouring out, and even then is but very imperfectly ſo; and that as for fuſion, that is wont to reduce the melted body to a new and permanent ſtate, as the formerly-mentioned powder of ſalt-petre, which before fuſion was but a heap of incoherent particles, is by it made a ſolid and conſiderably hard body: to prevent, I ſay, or remove ſuch ſcruples, we will ſet down one experiment that we long ſince met with, as to the main, in the ſhops of ſtone-cutters, which, though unregarded by them, will excellently ſerve to make out what we mention it for. Take then good alabaſter, or in defect of this, of that white ſtone, which is well known to our maſons by the name of plaſter of *Paris*, beat it very ſmall, and put as many pounds as you pleaſe of the finely-ſearched powder into any flat-bottomed (and firſt well heated) veſſel of braſs or iron (bigger or leſſer according to the quantity you intend to burn). Increaſe the fire by degrees, till it grow to be ſtrong, and when the calorific atoms ſhall have in ſufficient numbers pervaded the heap of powder, or, if you pleaſe, when the igneous corpuſcles have by their numerous and briſk ſtrokes upon the veſſel communicated by its means their agitation to the incloſed powder, and when by either of theſe ways, or both, the fire (which may alſo reſolve ſome of the more ſpirituous and exhalable parts, whereof diſtillation has ſhewn me, that alabaſter is not deſtitute, into vapours) ſhall have put the little bodies it conſiſts of into actual motion (which will be quickly done) you ſhall ſee it aſſume the form of a liquor, and boil with numerous great and confuſed waves juſt like a ſeething-pot. And if, whilſt it continues in this ſtate, you ſtir it with a ſtick, it will not like a heap of ſand, or, as itſelf would do at another time, reſiſt the motion thereof, but yield thereto like a liquor, and, like it, will ſeem to have ſomething of the nature of a coherent body: for by ſtirring it any thing ſtrongly near one ſide of the veſſel, you may make the waves beat very manifeſtly againſt the oppoſite part of it. And beſides all this, you may obſerve this further reſemblance betwixt this boiling matter and a liquor, that there will fly up out of the pot great ſtore of ſteams like ſmoke, but that they are white, which will ſometimes like ſmoke aſcend, for aught can be diſcerned, to the very top of the chimney, and leave its colour upon the places by which plenty of it hath paſt. Beſides, thoſe, that make this experiment often, as we have taken pleaſure to do, may have the opportunity to obſerve, that when the veſſel has continued ſo long over the fire, that the contained alabaſter relapſes into the form of a heavy moveleſs powder, by keeping it a while longer in the heat, it will for once at leaſt reſume the form of a fluid body, and boil again as before; the ſpirituous ſteams, whoſe avolation promoted the ebullition, being not yet quite ſpent. And laſtly, if when it ſeems moſt a liquor, you take up a little of it, and, as nimbly as you can, caſt it upon a ſheet of white paper, it will not at all wet it, but immediately diſcover itſelf to be a moveleſs incoherent powder, as it was before its being ſet over the fire. Whereby it (I hope) appears, that a heap or aggregate of ſuch little bodies as are neither ſperical nor otherwiſe regularly ſhaped, nor ſmall enough to be below the diſcernment of the eye, may, without either fuſion or being poured out, be made fluid barely by a ſufficiently ſtrong and various agitation (from what cauſe ſoever that proceed) of the particles that make it up, and loſe its fluidity immediately upon the ceaſing of it.

THUS have we ſeen, how very much it conduces to the making of a body fluid, that its ſmall parts be actually moved. And whence this motion proceeds, we ſhall not at preſent venture to determine. For though in the examples newly mentioned, and ſome others, moſt men will be forward to aſcribe the motion produced in the parts of the fluid bodies there mentioned, to the action of the fire, whereunto they were expoſed; yet what it is, that puts the parts of fluid bodies in general into the motion requiſite to make them ſuch, is a queſtion, of which the true reſolution indeed were very deſirable. But the full debate of it will not, I hope, be here expected from me, whilſt I am writing but notes, ſince it would engage me to diſcuſs two or three of the difficulteſt as well as the importanteſt controverſies belonging to natural philoſophy. For firſt, I ſhould be obliged to conſider whether motion, or a propenſity to it, be an inherent quality belonging to atoms in general, and not loſable by them; or whether all motion is communicated by impulſe from one body to another. And ſince thoſe, that of late have taught, that all viſible liquors, as water, oil, quick-ſilver, *&c.* owe their fluidity chiefly to the agitation of ſome thin and reſtleſs matter, which inceſſantly permeates them, do deduce the neceſſity of ſuch an ethereal ſubſtance principally from the impoſſibility, that there can be any vacuum properly ſo called in the univerſe, wherein yet are very many ſpaces unpoſſeſſed by either air or groſſer bodies than it: the examination of this ſubtile matter would draw on the conſideration of the nice controverſies, that perplex philoſophers concerning emptineſs, which it were more difficult for us to examine in few words, than it is neceſſary for us to meddle with them in this place; ſince not writing of the firſt principles of phyſiology, but of fluidity, which is but a ſecondary or derivative quality (if I may ſo call it) it ſeems ſufficient to give a notion of it, that we deduce it not from the unintelligible ſubſtantial form of the fluid bodies, but from thoſe ſimple and general affections of matter, the figure, ſituation, and motion of its ſmall parts.

SECT.

SECT. XIX.

WHEREFORE declining to add any thing in this place to what we have otherwhere diſcourſed concerning the origin of motion, and the poſſibility or impoſſibility of a vacuum; we will proceed to take notice, that there is one thing more, which we may learn from ſalt-petre touching the nature of fluidity, and that is the diſtinction betwixt a fluid body and a wetting liquor, which are wont, becauſe they agree in many things, to be confounded, but inconſiderately enough: for though every wetting liquor be fluid, yet every fluid body does not wet. The air, the æther, and even flame itſelf may be properly called fluid bodies, according to the notion of fluidity hitherto made out, and yet will ſcarce by any man be called moiſt liquors; and ſalt-petre, whilſt in fuſion, is really a liquor, and ſo is every melted metal; and yet theſe wet not the bodies they touch, as do water and other wetting liquors, which are fluid bodies with this peculiar qualification, that they ſtick to and moiſten the dry bodies, which they touch (or at leaſt abound with ſome parts, which being ſeparated from the reſt and reduced to a liquor, will do ſo.) And, according to this notion, methinks, it may be conceived, that the humidity of a body is but a relative thing, and depends chiefly upon the congruity or incongruence of the component particles of the liquor in reference to the pores of thoſe particular bodies, that it touches: for, ſometimes the little eminences and pores of the ſurface of the dry body, on or againſt which the liquor flows, are of ſuch magnitudes and figure, that the particles of the liquor find admittance into thoſe pores, and are detained there (by which means they uſually ſoften it) and ſometimes the pores and aſperities of the dry body's ſurface are ſo incommenſurate in bigneſs and figure to the particles of the liquor, that they glide over the ſurface, without ſticking or adhering firmly to any parts of it. This may be exemplified in quickſilver, which cannot be ſaid to be a humid body in reſpect of our hands or clothes, or of almoſt all other bodies of the world, upon whoſe ſurfaces it will roll without leaving any of its particles lodged in their pores, or faſtened to their little eminences, whence it is called by vulgar chymiſts, the water that wets not the hands: but in reference to divers metals, eſpecially gold and tin, quickſilver may be ſaid to be a humid liquor, for it inſinuates itſelf into their pores, and thereby mollifies their bodies, as other liquors do thoſe, that are moiſtened by them. And even water, that wets almoſt all other animal and vegetable, and many mineral bodies, beſides that it is commonly enough obſerved to ſtand in almoſt globular drops upon cabbage-leaves, ſeems not a humid liquor in relation to the feathers of ducks, ſwans, and other water-fowl; whom nature having deſigned to fly ſometimes in the air, and live ſometimes in the water, ſhe providently makes their feathers of ſuch a texture, that they do not, like the feathers of divers other birds, admit the water, which imbibed would make them unfit for the uſe of flying. And it is obſervable, that upon the change of texture in a liquor, it may be brought to ſtick to the ſurface of a body, to which before it would not adhere; as may appear by this, that though quickſilver alone will not ſtick to glaſs, yet if there be mixt with it a due proportion of lead, tin, and tin-glaſs, though neither of them will adhere to glaſs, yet their liquid mixture (as we have often tried and elſewhere * taught) readily will, even without the aſſiſtance of heat.

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* The writing here referred to, is the ſecond tome of *The Uſefulneſs of Experimental Philoſophy*, which ſhould have appeared before theſe ſpecimens.

SECT.

SECT. XX.

IF it be objected, that this various agitation of the inſenſible parts of water and reſembling bodies, wherein we make the nature of fluidity chiefly to conſiſt, is but an imaginary thing, and but precariouſly aſſerted, ſince by our own confeſſion they are ſo ſmall, that the particles themſelves, and more, the diverſity of their motions are imperceptible by ſenſe, which repreſents water, for example, to us as one continued body, whoſe parts are at perfect reſt:

IF this, I ſay, be urged againſt our doctrine, we ſhall not deny the objection to be plauſible, but muſt not acknowledge it to be unanſwerable.

FOR of the ſeeming continuity of water and other liquors this may be the reaſon, that the particles, whereof the liquor conſiſts, being too ſmall to be viſible, and being not only voluble, but in actual motion, the pores or vacant ſpaces intercepted between them muſt alſo be too little to be diſcerned by the eye, and conſequently the body muſt appear an uninterrupted or continued one: not to mention, that, were the parts of the liquor leſs minute, their ſhifting of places would hardly be perceived by the eye, each diſplaced corpuſcle being immediately ſucceeded by another like it. It is true, that a heap of grains of nitre, though upon its effuſion out of the veſſel it ſomewhat emulates a fluid body, does yet, when it reſts in the veſſel, appear to be but an aggregate of many little incoherent bodies heaped up together; becauſe the intervals or holes left between them are great enough to affect the ſenſe: but if the ſame ſalt be reduced into an alcohol (as the chymiſts ſpeak) or impalpable powder, the particles and intercepted ſpaces being then extremely leſſened, the body they make up will much more reſemble an intire maſs, though it be looked upon from a nearer diſtance; and ſo when this powder is by the fire further broken into parts incomparably ſmaller than thoſe of the powder, and which conſequently intercept ſuch extremely little pores, that not only ſalt-petre, but ſome metals, and even gold itſelf (from which it will not be ſuppoſed that any thing exhales to leſſen it) are by ſome affirmed (for I have not myſelf diligently enough obſerved it, and do yet doubt it) to take up rather leſs than more room melted than cold, why ſhould we not grant, that theſe pores may be little enough, not any where to diſcontinue the body as to ſenſe?

SECT. XXI.

AND that the incoherent parts of fluid bodies are alſo diverſly agitated, ſome this way, and ſome that way, though the ſenſe cannot diſcern it, may be proved by their ſenſible operations *. For without ſuch local motion, how could the particles of water pierce into the receſſes of bodies, and occaſion thoſe putrefactive alterations that are wont to be imputed to ſuperfluous moiſture? and how comes it elſe to paſs, that aqueous liquors ſo readily diffuſe themſelves into, and ſo exquiſitely mingle with one another? as we ſee when red and white wine are in a trice confounded into claret:

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* The author now finding, that ſomething concerning the various motion of the parts of fluid bodies, which he has but touched upon, has been, though but briefly and without experiments, yet excellently explained in a mathematical way by Monſieur *des Cartes* in the 56th and 57th articles of the ſecond part of his principles, thinks fit to refer the inquiſitive reader thither for fuller ſatisfaction about that particular.

and without this various agitation of the parts of water, how could it be, that lumps of ſugar or ſalt caſt into it ſhould quickly be ſo perfectly diſſolved in it, that the lumps themſelves totally diſappear, and the diſſociated parts are carried about every way by thoſe of the water, even from the bottom to the very top? As is evident particularly in ſea-ſalt, which when the ſuperfluous liquor is ſufficiently exhaled, begins viſibly to coagulate, not at the bottom, but upon the ſurface of the water; and not only ſalt, but even gold itſelf, though the heavieſt of bodies, may have its parts ſo ſcattered by the agitation of thoſe waters, as experience has taught us, and as you may eaſily try by putting a little of the ſolution of gold, made in aqua regis, into fifteen or twenty times as much fair water, which will all thereby be immediately ennobled with a golden colour. That the little bodies, whereof flame conſiſts, are fiercely agitated, appears oftentimes even to the eye, and will ſcarce be denied by him, that conſiders the operations of it, and the vivid beams it darts round about it againſt the neighbouring bodies. And that the particles, that compoſe our common air, are alſo very diverſly agitated, we may be induced to believe by ſundry particulars. As firſt, by thoſe little moats, that from a ſhady place we ſee ſwimming up and down in the ſun-beams, and by the tremulous motion, which that of ſwarms of little bodies in the air ſeems to impart to diſtant objects looked on after ſun-riſe through a good teleſcope (and which by the bare eye in hot weather may be often diſcovered by certain very dilute ſhades, which ſeem to tremble upon the walls of high roofed halls and churches, and other ſpacious buildings.) Next (and more eaſily) by this, that if you take ſalt of tartar, firſt brought to fuſion, and place it in a cellar, or even in an ordinary room, it will in a ſhort time (now and then in a few minutes) begin to relent, and have its ſurface ſoftened by the imbibed moiſture of the air, wherein if it be left long enough, it will totally be diſſolved into clear liquor; which would not be, if the moiſt vapours that help to conſtitute the air, did not move to and fro every way, and were not thereby brought to the ſalt, and enabled to inſinuate themſelves into its pores, and by that means diſſolve it, and reduce it with themſelves into a liquor.

AND even in ſummer, when the air is wont to be much dryer than at other ſeaſons of the year, one may quickly diſcover, that there are in the air ſtore of aqueous corpuſcles, moved ſome one way and ſome another, by the experiment of putting into a drinking-glaſs, for want of ice and ſnow, ſome beer or wine actually very cold: for thereby, after a while, the outſide will appear all bedewed with little drops of liquor; which ſeems plainly to be no other than the aqueous ſteams, that ſwimming up and down in great multitudes in the air, are by its agitation towards all parts carried, as every other way, ſo to the ſides of the glaſs, and being there condenſed by the coldneſs of that ſmooth body, turn into viſible and palpable water. And, if I much miſ-remember not, it was one of the circumſtances of the laſt experiment of this kind we have had occaſion to take notice of, that the drops, that faſtened themſelves to the outſide of the glaſs, purpoſely left in part unfilled, reached either not at all, or very little further than the ſurface of the liquor within the glaſs, whoſe coldneſs as it ſeems did not infrigidate thoſe upper parts of the glaſs, to whoſe level the liquor itſelf did not reach. To which I could eaſily add arguments to prove, that the drops we have been ſpeaking of proceeded not from the tranſudation of the liquor within the glaſs, if I thought it worth while to diſprove ſo unlikely a conjecture. But inſtead of that I ſhall only intimate, that from this experiment uſeful hints may be taken both theorical and practical, and particularly, that a reaſon may, perchance, be given of a ſtrange way of catching a ſalt and liquor out of air, barely by glaſs-veſſels of a peculiar and ſkilful contrivance. Much of what we have lately ſaid will, I preſume, be the leſs wondered at, if we ſubjoin what experience has taught us, that it is not difficult by the help of a convenient furnace and fit veſſels to make that ponderous metal, lead, aſcend to a good height in the open air, in the form of a copious ſmoke: ſuch a ſmoke we diſcerned after a while to be carried ſo many ways by the aërial corpuſcles, that it met with in actual motion, that it was ſoon diſperſed ſo far as to diſappear: which perhaps will be thought ſome confirmation of what we formerly delivered, when we taught, how much the being divided into very minute parts may conduce to the fluidity even of ponderous bodies.

SECT. XXII.

AND though quickſilver be, excepting gold, the heavieſt known body in the world, yet when it is reduced into vapour, it ſeems to be carried to and fro like the other terreſtrial particles, that ſwim up and down in our air: for I remember, that an expert gilder not long ſince complained to me, that if when he evaporated quickſilver, he forgot to take off his rings from his hand, though they touched not the quickſilver whilſt it was in a body, the roving fumes would oftentimes faſten upon the gold in ſuch plenty, as would put him to much trouble to get them off from his rings; one of which he ſhewed me, that he had lately thus whitened, and as it were ſilvered over with mercurial fumes, and was then to reſtore to its native yellow.

SECT. XXIII.

BUT let us return to viſible liquors, and endeavour to prove almoſt *ad oculum*, as they ſpeak, that their inſenſible parts may be every way agitated, though their motion be but ſeldom viſible to us. Take then what quantity you pleaſe of aqua fortis, and diſſolve in it as much as you pleaſe of ordinary coined ſilver (it not being neceſſary for this experiment that it be refined) and pour the coloured ſolution into twelve or fifteen times as much fair water, and then decant or filtrate the mixture, that it may be very clear. If you look upon this liquor, the parts of it will ſeem to be all of them as perfectly at reſt as thoſe of common water; nor will your eye be able to diſtinguiſh any corpuſcles of ſilver ſwimming in the liquor: and yet that there are ſuch metalline corpuſcles agitated to and fro with and by thoſe of the water, will quickly appear, if you immerſe into it a flatted piece of clean copper, for by that time you have held it two or three minutes of an hour (perhaps not ſo long) in the liquor, you ſhall ſee the particles of ſilver, that were roving up and down the liquor, faſten themſelves in ſuch ſwarms to the copper-plate, that they will appear in their native hue, and cover it, as it were, with a looſe caſe of ſilver, which may be eaſily ſhaken off in the form of a metalline powder: and if ſeveral ſuch plates be left all night, or for a competent number of hours, in the bottom of the veſſel, you may the next day find all the particles of ſilver, that were diſperſed through the whole body of the liquor, ſettled upon or about them; the deep bluiſh green tincture you will diſcover in the water proceeding only from ſome little parts of the copper-plates, and of the alloy of the coin, diſſolved by the ſaline particles of the aqua fortis. And I remember, that, to compleat the experiment, I have ſometimes made even theſe fall to the bottom of the veſſel, by leaving a lump or two of ſpelter there for two or three days:

for, not only thoſe metalline corpuſcles, that were juſt over or near to the determinate place, where I put the ſpelter, but alſo all the reſt, into how remote parts ſoever of the liquor they were diffuſed, did ſettle upon the ſpelter, as appeared both by its increaſe of bulk, and by their leaving the water clear and colourleſs; which plainly ſeems to have proceeded from hence, that the particles of the water were reſtleſsly and every way agitated, and ſo by frequently gliding along the ſurface of the ſpelter they muſt carry thither of the corpuſcles of copper mingled with them, ſome at one time and ſome at another, till at length all were brought to it and detained there.

SECT. XXIV.

THAT of the particles of ſpirit of wine, and ſuch like inflammable liquors drawn from fermented juices, though they ſeem to the eye to be at reſt, a good many do yet move confuſedly and very nimbly, I remember I have long ſince manifeſted by an eaſy and ocular proof, which I deviſed about ten or twelve years ago, when (being yet ſcarce more than a boy) I firſt began to conſider what fluidity might be. The experiment, as I writ it down, with all the circumſtances and obſervations relating to it, I have not now by me; but having divers times been deſired to ſhew it to learned men (phyſicians, mathematicians, and others) I cannot have forgotten thoſe phænomena of it, that are the moſt pertinent to our preſent ſubject. Suppoſing then, that in pure ſpirit of wine, beſide the aqueous parts that glide ſoftly along each other, there are ſtore of volatile and ſpirituous corpuſcles, whoſe agitation is ſtronger, I let fall (from a pretty height, that it might be broken into ſmall drops by its fall) into any wide-mouthed glaſs filled with this liquor (which muſt not be over dephlegmed, leſt the oil ſink in it) a little common oil or ſpirit of turpentine, which I therefore made choice of, becauſe its tenacity, greater than that of the chymical oils of ſpices, makes it, that it will neither mingle with ſpirit of wine, nor ſpread itſelf, as divers other diſtilled oils will, upon the ſurface of it, but keep itſelf in the form of round drops, whoſe ſhape facilitates their motion. The oily drops then ſwimming at the top of the ſpirit of wine, will be, by the diſorderly rovings of the agile parts of it (which hit againſt them little globes, as the vinous ſpirits aſcend to exhale) made to move reſtleſsly to and fro in an irregular manner, the drops ſometimes bearing up to one another, as if all or moſt of them were preſently to unite into one body, and then ſuddenly falling off, and continuing to ſhift places with one another, after a manner pleaſant and ſtrange enough to them, that never before ſaw the experiment: and this dance will continue for half an hour or an hour (or a ſhorter or much longer time, according to the quantity and ſtrength of the liquor) till the ſpirituous parts being flown away, the drops being no longer impelled lie at reſt upon the diſpirited liquor, as they would upon common water. And whereas the nimble motion of the drops might be ſuſpected to proceed from ſome ſecret contrariety in nature betwixt the oil of turpentine and ſpirit of wine; beſides, that I could eaſily ſhew, that thoſe two liquors have no antipathy, I not only tried the experiment with another inflammable liquor than ſpirit of wine, but (if I much miſremember not) found, as I expected, that little pieces of chopped ſtraw (ſuch both being light and not eaſily imbibing moiſture) being gently let fall upon the ſpirit of wine, were in a tumultuous manner carried to and fro upon the ſurface of it; though I am not ſure, but that the motion of the oily drops may be in part due to ſome partial ſolution made of them by the vinous ſpirit, which during that action may tumble them to and fro; not to add, that I have, by ſome trials, been tempted to ſuſpect the air may have ſome intereſt in the motion of the drops. However, I have mentioned the recited experiment, not as if I thought that either it or fugitive ſpirit of wine were fit to teach us the nature of fluid bodies in general, but to ſhew by an ocular example, that there may be a quick and inteſtine motion in ſome parts of a liquor, notwithſtanding that the unaſſiſted eye can diſcern no ſuch matter. I ſhall not here relate, how having cauſed to be hermetically ſealed up ſome of theſe liquors in a glaſs, to try how long the extravagant dance of the drops would laſt, when the more ſpirituous parts of the vinous liquor could not exhale, my veſſel was ſoon broken without any diſcernible violence. Nor ſhall I now take notice of any of the other phænomena of our experiment, partly, becauſe I have elſewhere mentioned moſt of them; and partly, becauſe I think it more pertinent to our preſent theme, to obſerve, that this unſeen agitation of the minute parts will not only hold in light and ſpirituous liquors: for, that the inſenſible parts even of the heavieſt liquors themſelves are alſo in actual motion, though many may think it unfit to be believed, will follow from what has been already delivered concerning the nature of fluid bodies, as ſuch; and may be confirmed by this, that whereas three of the heavieſt liquors, we yet know of, are quickſilver, oil of tartar *per deliquium*, and oil of vitriol; that firſt-named will even in the cold penetrate into the pores of foliated gold, and deſtroy the texture of that cloſeſt of metals: the liquor alſo of ſalt of tartar will in the cold draw tinctures from ſeveral bodies, and we have endeavoured to evince the agitation of the parts of oil of vitriol, not only by ſhewing, how in the cold it would corrode divers metals, but by caſting little pieces of camphire into it, which without the aſſiſtance of the fire were made liquid by it, and appeared like ſo many drops of oil. And he, that yet doubts, whether the parts of theſe two oils (as chymiſts abuſively call them) how ponderous ſoever they be, are fiercely agitated or no, may probably be ſoon ſatisfied by ſhaking an ounce or two of each of them together, and obſerving the heat, hiſſing, ebullition, and ſparkling, that will ſuddenly enſue upon their being blended.

SECT. XXV.

But here we muſt take notice, that though it belong to the nature of fluid bodies, that their parts do eaſily ſhift places; yet that is to be underſtood only as to the parts of the ſame fluid bodies, as water, or of ſuch differing fluid bodies as are diſpoſed readily to admit each other's particles, and mingle together, as we ſee in water and wine. For otherwiſe fluid bodies may be of ſuch differing natures, that when two or more of them are put together, they will not mix, but each retain its own diſtinct ſurface; ſo that in regard of one another, the contiguous bodies do in ſome degree emulate each of them the nature of a conſiſtent body: for though it cannot be looked upon as a hard body but a ſoft, becauſe of the eaſy ceſſion of its ſuperficies, yet it does like a compact or conſiſtent body deny to mingle permanently with the contiguous liquor or other fluid ſubſtance. And I ſomewhat wonder, that *Lucretius* and other Atomiſts ſhould (at leaſt for aught I remember) overſee this obſervation, ſince it is obvious enough in oil, which will not mix with water, but float upon its ſurface: not to mention, that quickſilver will not incorporate with any of the familiar liquors known to the antients. I had almoſt forgot, that I promiſed at the beginning of this diſcourſe an inſtance concerning flame, which I will therefore now recite. And it is, that having by an eaſy preparation of copper, by the intervention of a little ſal-armoniac (which I have already taught in another treatiſe *) ſo opened the body of that metal, as to make it inflammable; I took ſome ſmall grains of this prepared mineral, and put them under the wick of a ſtrong and actually burning candle, whereby (as I expected) they were with the melted tallow ſoon carried up to the bottom of the flame, and by it ſo kindled, that the green (not blue) flame of the cupreous body did (ſomewhat to the wonder of the ſpectators) burn for a good while (this combuſtible matter being marvelouſly laſting) diſtinct from the yellow flame of the candle, as if there had been ſome inviſible partition between them. But to return to the unmingleable liquors we were formerly ſpeaking of; the cauſe, why theſe retain their diſtinct ſurfaces, my preſent taſk does not oblige me to inquire into: but this I ſhall obſerve in general, that it ſeems to depend very much upon the texture of the particular liquors, and perhaps too upon the peculiar motions of their minute parts. For I have obſerved, that though pure ſpirit of wine and ſalt of tartar, reſolved into a liquor by the moiſture of the air, will, when put together, retain diſtinct ſurfaces, or preſently regain them if you ſhake the liquors never ſo ſtrongly together; yet by adding a little fair water to either of them, the texture being thereby altered, it will eaſily incorporate with the other. And thus although that (as I noted already) common ſpirit or oil of turpentine will not mingle with ſpirit of wine, yet having had the curioſity to make a trial with oil of turpentine abſtracted ſkilfully, and with a very gentle fire (for otherwiſe the experiment may eaſily miſcarry) from melted or at leaſt well decrepitated ſea-ſalt, we found, according to expectation, that though there appeared no viſible alteration in the oil, yet we could eaſily by ſhaking confound it with pure ſpirit of wine. Moreover, though lixiviate liquors and oils will not by an ordinary agitation be permanently joined, yet I have tried, that by digeſting a good while a ſolution of ſalt of tartar with oil of almonds, I could reduce them to a ſoft ſaponary ſubſtance: which experiment makes ſomewhat more to my preſent purpoſe, than the common practice of ſoap-boilers, becauſe I did not, as they, boil away the water, wherein the lixiviate ſalt is diſſolved. I might add alſo, that if you put one part of quickſilver into about four parts of oil of vitriol, you will find (at leaſt if the experiment proceed always after the ſame manner as it has done with me) that the two liquors will remain diſtinct whilſt you keep them in the cold, but if by degrees of heat you bring the oil of vitriol to boil, it will pierce the ſurface of the quickſilver, and by partly incorporating with it, reduce it to a ſubſtance very differing from what it was. But becauſe theſe two laſt experiments may be, with leſs improbability than the two that preceded them, referred to other cauſes, I ſhall no farther inſiſt on them, but take notice of one thing more concerning the difference of fluid bodies. And it is thus.

SECT. XXVI.

I OBSERVED, that ſome of them do not only not mingle with others, that are contiguous to them, but faſhion the ſurfaces of thoſe others, and reduce them to determinate ſhapes. This I have taken pleaſure to conſider in ſome chymical liquors, which I have purpoſely put together; for inſtance, having poured ſpirit of wine upon oil of tartar *per deliquium* (as chymiſts call it) I found, that the ſuperficies, wherein they touched each other, was flat, or (as to ſenſe) parallel to the horizon. But if this was done in a very ſlender or narrow glaſs, with the mouth unſtopped, though the lower ſurface of the ſpirit of wine which touched the other liquor appeared very level; yet the upper ſuperficies, which was contiguous to the air, was manifeſtly very concave. And if to theſe two liquors I did in a broader glaſs pour oil of almonds, that oil would ſink to the bottom of the ſpirit of wine (that being well rectified) and floating upon the oil of tartar, would ſeparate the two liquors, and both above and below retain a flat or level ſurface. But if inſtead of oil of almonds, or another expreſſed oil, I dropped into pure ſpirit of wine, ſwimming upon oil of tartar, ſome common oil of turpentine; the oil would gather into parcels ſome of the bigneſs of hail-ſhot, ſome as big as ſmall piſtol-bullets, and ſome of other ſizes, which in caſe they did ſwim in the ſpirit of wine, and touched neither of its ſurfaces, ſeemed globulous, and continued ſo (the glaſſes being ſtopped) for many hours: but in caſe they emerged to the upper part of the ſpirit of wine, as much of them as lay immerſed in the ſpirit (which was by far the greateſt part of them) appeared round, and continued ſo as long as I pleaſed; the upper parts only of thoſe little globes appearing to have the ſame ſurface with the ſpirit of wine. And I further obſerved, that ſome ſmall drops would, as it were, reſt conſtantly upon the ſuperficies of the oil of tartar, touching it but as it were in a point, and continuing to the eye ſperical; though the ſurface of the liquor were purpoſely now and then ſomewhat ſhaken. But that, which I took ſpecial notice of, was, that having (upon deſign) into pure ſpirit of wine (for upon common ſpirit oil will ſwim) let fall ſome great drops of oil of turpentine, they did at firſt ſink to the bottom of it, and lie upon the ſurface of the oil of tartar almoſt like hemiſpheres, whoſe convex part was all above the oil of tartar; but after a while they were, as I expected, preſſed on all ſides and faſhioned into round bodies (yet a little more protuberant at the ſides than the top) which ſeemed ſcarce to touch the ſurface of the oil of tartar on which they leaned. Divers other obſervations of this kind were afforded me by ſome peculiar mixtures, that I made of chymical liquors: but not having the leiſure to ſet them down, much leſs to inquire into their cauſes, I ſhould ſcarce have mentioned what I have already delivered (eſpecially ſince we found that a light variation of circumſtances would often alter the event of ſuch trials, which we have therefore ſet down barely hiſtorically) but that finding that drops of water, quickſilver, and other fluid bodies, ſeemed to be faſhioned into a round figure by that every way almoſt equal preſſure of the ambient air; and having likewiſe tried, that quickſilver ſuſpended in the air (as it may eaſily be, if the Torricellian experiment being made in a tube exceeding ſlender, ſome air be afterwards dexterouſly let in to divide the long mercurial cylinder into diverſe ſhort ones) has both at the top and bottom, where it is expoſed to the action of the air, a very protuberant ſurface; finding, I ſay, theſe effects of fluid bodies upon one another, I thought it not amiſs to intimate, how ſome experiments might be made, that may poſſibly facilitate the giving an account of the figuration of ſome of the more conſiderable fluid bodies, which, as we noted already, make up much the greater part of the univerſe: eſpecially, ſince I could eaſily enough make it probable, that ſuch ſteams of the terreſtrial globe, as may well be ſuppoſed to be the chief ingredients of our atmoſphere, may like a liquor retain a ſuperficies diſtinct from that of the ambient and contiguous body. And ſince we are ſpeaking of the diſtinct ſurface of fluids, the occaſion invites me to add an experiment, which, though apt to miſcarry upon the account of unheeded circumſtances, has yet, when all things were rightly ordered, ſucceeded very well: I will, I ſay, ſubjoin it here, becauſe it ſhews a way of dividing in a trice a liquor tranſparent, and, as to ſenſe, homogeneous, into two very differing liquors, the one diaphanous, and the other opacous, which will not mingle. The experiment is this. Diſſolve one ounce of clean common quickſilver in about two ounces of pure aqua fortis, ſo that the ſolution be clear and total; then whilſt it is yet warm, pour into it by degrees, leſt they boil over, half an ounce or one ounce of filings of lead, and if no error, nor ill accident have intervened, the lead will be in a trice precipitated into a white powder, and the mercury reduced into a maſs (if I may ſo ſpeak) of running quickſilver, over which the remaining part of the aqua fortis will ſwim, whereby we may ſee, that liquors being reduced into very minute parts, may mingle very well, the corpuſcles of the one ſupporting in that ſtate thoſe of the other, though in greater bulk; eſpecially, the texture of one being ſomewhat varied, they will retain diſtinct ſurfaces. *N. B.* Note, that when the operation ſucceeds not well, the mercury need not, for all that, be loſt, but may (in great part at leaſt) be recovered by freeing the precipitated matter from the reſt by filtration, and then diligently grinding the white precipitate with water, by which means, the mercury will little by little be gathered into drops. And though this be far from being the true mercury of lead, as I may elſewhere ſhew you; yet ſome inducements, not here to be named, incline me to look upon it, as ſomewhat differing from common mercury, and fitter than it for certain chymical uſes.

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* An Hiſtorical Dialogue about Flame.

SECT. XXVII.

And here I ſhould paſs on to the conſideration of firmneſs, but that, when a while ago I diſcourſed of the agitation of the corpuſcles that compoſe oil of tartar and oil of vitriol, I forgot to add, that not only in fluid bodies, but in ſome alſo of thoſe that are conſiſtent, there may perhaps be more motion in the inſenſible parts than our ſenſes diſcern, or we are wont to imagine; eſpecially in thoſe bodies, which having been once endowed with life, are, though not fluid, yet either ſoft, or at leaſt not perfectly hard. I have more than once taken pleaſure to look upon an heap of ſwarming bees, for though they make not up a liquid but coherent body, which may be turned upſide down without loſing its coherence, and which being beheld at a diſtance, ſeems to be one entire maſs or body; yet it is evident to him, that looks at them near enough, that the particular bees, that ſwarm, have moſt of them their diſtinct and peculiar motions, and that yet theſe motions of the particular bees deſtroy not the coherency of the heap; becauſe that when one of the more innermoſt bees removes, as ſhe lets go her hold from thoſe, that ſhe reſted on before, and goes away from thoſe that reſted on her, ſo ſhe meets with others, on which ſhe may ſet her feet, and comes under others, that in like manner ſet their feet on her; and ſo by this viciſſitude of mutual ſupports their coherence and their removes are made compatible. And if inſtead of bees, the ſwarm conſiſted of extremely little flies, their particular motions would perhaps be inconſpicuous. And that ſome ſuch thing may happen in the conſiſtent bodies we have been ſpeaking of, ſeems probable from hence, that in wainſcot and other hard wood, we often ſee little heaps of duſt produced in them by putrefaction; and not only in cheeſe we many times ſee multitudes of mites ſtart up, but in apples and other fruits we oftentimes find maggots, though the ſkin be whole (which could not be unleſs the parts of the matter were variouſly tranſpoſed, that is, put into a local motion, and connected after a manner ſuitable to the nature of the inſect to be produced). And by the growth of bones in the bodies of perfecter animals, as well in reſpect of the internal cavity, where the marrow lodges, as of the external ſurface, as alſo by the growth of the ſhells of oyſters and ſnails (though cold animals) from a ſize inconſiderable at firſt, in regard of what is afterwards attained to, and by ſome other reſembling particulars, that conſtitute even the ſolid parts of animals, are not, whilſt the creature lives (or at leaſt whilſt it grows) altogether exempt from ſome (though ſlow and inſenſible) local motion. And I remember, that it has by a very diligent obſerver been affirmed to me, that he ſaw ſeveral pieces of gum ſweat out of an old wainſcot of above twenty years ſtanding. Which I the leſs wonder at, becauſe I have ſeveral times ſeen viſcous exſudations diſcloſe themſelves like drops of turpentine upon deal-boards, which had been made uſe of about building. (But of this ſubject more, perhaps, elſewhere.)

SECT. XXVIII.

AFTER we have hitherto diſcourſed of fluidity, as conſidered in diſtinct bodies, we might properly enough ſay here ſomething of what furtherance or hinderance, in reſpect of fluidity, one body may receive by being mingled with another. But the conſideration of thoſe changes of conſiſtence, which may be produced by mixture, is a ſubject; that we ſhall have ſuch frequent occaſions to treat of in what we are to deliver about firmneſs, that we ſhall now only give this general admonition, that it is not ſo ſafe as one would think, to foretel the conſiſtence of a mixture of two or more bodies, from the bare conſideration of the conſiſtence of thoſe bodies, whereof it is to be compounded. And that we might at once both manifeſt this, and inſinuate what judgment ſhould be made of what is ſaid by ſo many chymiſts and others, who without limitation teach, that the addition of ſalts to metalline and mineral bodies does much facilitate their fuſion, I remember I purpoſely made and employed this experiment.

WE diſſolved crude copper in a due quantity of ſpirit of nitre, and by evaporation reduced the ſolution to a kind of vitriol of a lovely colour. We alſo corroded with two parts of ſpirit of nitre one of good tin, and ſuffered the mixture to reduce itſelf (as it eaſily did) to a ſubſtance almoſt like meal. Of this mixture we put a parcel into a crucible, and ſuffered it to grow (and for a pretty while to continue) red hot: nay, we put ſome of it upon a quick coal, and excited the heat by frequently blowing the fire, without finding, that this metalline meal did at all melt, though ſalt-petre be a fuſible ſalt, and tin itſelf be of exceeding eaſy fuſion. Whereas, although copper be a metal, which is much harder to melt, not only than tin and lead, but even than ſilver (as thoſe well know, that mix ſilver with copper to make a fuſible mixture to ſolder upon copper and braſs) yet was this metal, that will endure a long and ſtrong ignition by being joined *per minima* with the ſame kind of nitrous corpuſcles, that had ſo contrary an effect upon the tin, ſo ſtrangely diſpoſed to fuſion, that the vitriol would melt with as ſmall a heat as that of one's hand. Nay, we have made ſuch a vitriol either with ſpirit of nitre or with (what differed little from it) a certain aqua fortis, as would even in winter be made to ſtand melted for divers hours together, by the languid warmth of the ſun, though ſhining on it but through a window, where it alſo ſtood but in an unſtopped glaſs. So fit it is, that we conſider as well the new texture, that mingled bodies obtain by the aſſociation of their particles (whoſe ſize and ſhape, and perhaps motion may be thereby much altered) as the conſiſtence of the particular bodies before their being mixed.

THE
Hiſtory of FLUIDITY and FIRMNESS.

The SECOND PART.

Of FIRMNESS.

IT is ſufficiently known, that the chymiſts aſcribe the firmneſs and hardneſs of bodies to ſalt, and teach, that the ſaline ingredient of them is the principle of coagulation in them, and the cauſe of their compactneſs and ſolidity. But though this opinion of the chymiſts be embraced by ſo many modern philoſophers and phyſicians, that ſome may think it ſuperfluous to make enquiry after other cauſes; yet others (to whom the explications of chymiſts ſeem not always ſo much as intelligible) will, upon the very account of the receivedneſs of the propoſed opinion, think it rather worthy to be examined, than to be acquieſced in. However, without making it our buſineſs, either to ſide with, or oppoſe any ſect of naturaliſts, we will apply ourſelves a while to conſider the thing itſelf in proſecution of the deſign already begun. And having in the foregoing part of our little hiſtory taken a general view of fluidity; we will now proceed to try, what light it will afford us to diſcover the nature of firmneſs and compactneſs.

AND ſince fluidneſs and ſtability being contrary qualities, are to be apprehended under contrary notions, we may conceive, that the firmneſs or ſtability of a body conſiſts principally in this, that the particles, that compoſe it, beſides that they are moſt commonly ſomewhat groſs, either do ſo reſt, or are ſo intangled between themſelves, that there is among them a mutual coheſion, whereby they are rendered unapt to flow or diffuſe themſelves every way, and conſequently to be, without violence, bounded and figured by other ſurfaces than thoſe, which their connection makes themſelves conſtitute.

IN this rude deſcription of firmneſs we have intimated three principal cauſes of it, namely the groſſneſs, the quiet contact, and the implication of the component parts.

THE firſt is groſſneſs of parts, of which we have in effect almoſt ſufficiently diſcourſed already: for ſince treating of fluidity, we manifeſted at large, how conducive ſmallneſs of parts was to that quality; it is eaſy to deduce, that groſſneſs of parts in a body muſt commonly be a great diſpoſition to its being firm. And bigger corpuſcles being *cæteris paribus* more difficult than leſſer to be put into motion, when they are once at reſt, it is obvious, that a body conſiſting of ſuch particles is leſs diſpoſed to become fluid, and conſequently more apt to continue firm, than if its component parts were ſmaller, and thereby more eaſy to be diſplaced. But when I ſpeak of the groſſneſs of corpuſcles, I pretend not to determine, whether or no body or matter be ſo perpetually diviſible, that there is no aſſignable portion of matter ſo minute, that it may not at leaſt mentally (to borrow a ſchool-term) be further divided into ſtill leſſer and leſſer parts: for allowing this indefinite diviſibility of corporeal ſubſtance, it is plain, that it may in ſome ſenſe be averred, that there are no firm bodies, whoſe parts are not extremely minute. But I underſtand by the groſs parts I here ſpeak of, ſuch corpuſcles, as actually convening to conſtitute a body, are ſcarce diſſipable or diviſible into leſſer by the agitation of the ambient air or æther, or by the other cauſes of the fluidity of bodies.

It is alſo to be noted, that when I ſpake of the fitneſs of groſſer corpuſcles to make a firm body, I added, *cæteris paribus*, becauſe it may happen, that the breaking of the ſmall parts of a body into minute particles may make them but the fitter to contribute to the firmneſs of the body they belong to: for the parts of the divided corpuſcles may by their comminution acquire a new, and perhaps a more irregular ſhape than before, upon whoſe account they may be more diſpoſed to be entangled among the neighbouring particles, or may be better fitted to get into and fill the pores of ſome kind of bodies. And in ſuch little particles, not only the minuteneſs may make them lie cloſer together, and conſequently the better exclude the air; but the greatneſs of the ſurface in proportion to the bulk of the matter may perhaps in ſome caſes occaſion a fuller contact, and ſo facilitate the conſtitution of a very firm body, in caſe theſe minute parts (whoſe intervals, if they intercept any, need not be other than very ſmall) ſhall be placed and diſpoſed to the beſt advantage for a full contact of one another. But as I ſaid a while before, from what we have already delivered (concerning the ſize of parts, when we treated of fluidity) it may eaſily be underſtood, how much the magnitude of them may conduce to firmneſs; and therefore we will preſently paſs on to the mention of the two other things to be conſidered, in reference to conſiſtent bodies. Whereof the one is the bare reſt of the ſmall and contiguous parts, that make up the firm body; and the other the intricate texture of ſuch parts in the body they make up. And either of theſe two ſeems alone ſufficient to render a body ſtable; though nature do perhaps oftentimes make ſome (though not equal) uſe of both, to faſten the parts of the ſame body more firmly together.

Of the former of theſe cauſes, I am informed, that the juſtly famous Monſieur *Des Cartes* has alſo taken notice, but without adding proof from experiments, or obſervations. But it ſeems to have been either overlooked, or (as incongruous to their hypotheſis of the innate motion of atoms) rejected by the old atomiſts, and by *Lucretius*, who takes notice (that I remember) only of the latter: for though they did of old make mention of the ſudden divulſion of two flat and ſolid bodies, yet they employ that obſervation but to prove a vacuum, (without otherwiſe taking notice, that I have met with, of thoſe things, that are moſt material in ſuch obſervations to our preſent purpoſe, and without deducing thence what we ſhall endeavour to do in order to the explication of the cauſes of firmneſs.) Upon what account then ſoever the atomiſts have omitted to reckon for a cauſe of firmneſs that, which we have newly been ſpeaking of; yet (as we obſerved above) if two bodies be once at reſt againſt one another, it ſeems conſonant to the catholic laws of nature, that they ſhould continue in that ſtate of reſt, till, ſome force capable to overpower their reſiſtance puts them out of it. And whatever may be ſaid of the unloſable mobility of atoms ſtrictly ſo taken, yet that divers parts of matter may compoſe bodies, that need no other cement to unite them, than the juxta-poſition which we here preſuppoſe, and the reſting together of their parts, whereby the air and other fluid bodies, that might diſſociate them are excluded, I have been inclined to think by what I have obſerved in grinding of glaſſes: for ſometimes the convex ſurface of one body being ground upon the concave ſurface of another, the two ſurfaces will happen to be ſo cloſely and exactly fitted to one another, (their immediate contact in all their parts, or at leaſt in innumerable of them, hindering the intercurrence of the air) that a man is not able without breaking one or both of them to pull them directly aſunder; but if you will ſever them, you muſt be fain to make one of them to ſlip along the ſurface of the other: which makes the glaſs grinders often complain of the trouble they meet with in ſeparating ſuch bodies. Nay, if you lay two flat glaſſes ground very true and well poliſhed upon one another, ſo that their ſurfaces may almoſt every where touch each other, (to which it will be requiſite to rub them a little one upon another, for the better excluſion of the air) you may by lifting up the uppermoſt, lift up the lowermoſt (though perhaps, as we have often tried, two or three times bigger) with it, as if the two plates of glaſs made but one body. Nay, we have divers times taken up four or five pieces of glaſs at once, laid and preſſed thus one upon another, and might, perchance, have taken up a greater number, if we had had more of them at hand. And trial has alſo informed us, that if you hold a looking-glaſs very level with the unfoliated ſide downward, and rub a little againſt it a piece of other very flat and very ſmooth glaſs, you may eaſily, by that way only, faſten them to one another; ſo that the lowermoſt glaſs, though large, will hang between the uppermoſt and the ground, to the wonder of thoſe, that behold it, and can diſcern nor imagine nothing capable to keep it from falling: and by the ſame way (as we ſhall recite anon) we have often made one conſiderably thick piece of marble take and hold up another, having purpoſely cauſed their flat ſurfaces to be carefully ground and poliſhed, without which the experiment will not ſucceed. Nor is it requiſite, that the glaſſes be flat to make them adhere very cloſely together, provided their immediate contact be made according to a large ſurface: for to what we have already mentioned concerning the cohering of convex and concave bodies, we may add, that having purpoſely applied a long glaſs-ſtopple of an almoſt conical figure, and well ground, to the mouth of a thick quart bottle, whoſe neck was made long, and of a figure convenient to receive the ſtopple, and ground within fit for it, we found, that theſe two glaſs bodies touching one another in a multitude of parts, did adhere together ſo cloſely, that when the ſtopple was carefully put in, we could eaſily, and divers times one after another, lift up the bottle, though there was by our gueſs above a pound of liquor in it. Unleſs we ſuppoſe, without much probability, that becauſe it is found, that moving them to and fro upon one another, and preſſing down the ſtopple, promotes their ſticking, their adheſion may be in part aſcribed, either to ſome elaſtical motion in the parts of the preſſed glaſs, or to the exquiſite adaptation of the almoſt numberleſs, though very ſmall, aſperities of the one, to the as numerous little cavities of the other; whereby the ſurfaces do lock in with one another, or are as it were claſped together. For as poliſhed as the ſurfaces may appear to ſenſe, we muſt not deny, that there may be ſuch inequalities in them, ſince being wont to be poliſhed with putty or ſome ſuch powder, or heap of grating and irregularly ſhaped corpuſcles, they muſt needs make ſtore of little furrows, and ridges, and other aſperities on them. But to inſiſt on theſe conjectures, were to digreſs.

YET here we muſt not decline taking notice, that, at leaſt here below, the ſticking together of ſuch bodies, as are of ſenſible bulk, and whoſe ſmooth ſurfaces immediately touch one another, may poſſibly not ſo much proceed from this, that their parts, as we formerly obſerved, are at reſt among themſelves, and by their immediate contract do make up as it were but one body; as from the preſſure of the atmoſphere, proceeding partly from the weight of the ambient air, (mixed with the effluviums of the terreſtrial globe) and partly from a kind of ſpring, by virtue of which the air continually preſſes upon the bodies contiguous to it, though through accuſtomance

and

and negligence, and perhaps ſome other cauſes not here to be inſiſted on, we neither feel it in our own bodies, nor take notice of it in others. Now this preſſure of the air every way being ſuppoſed, I think the adhering of the ſmooth bodies we ſpeak of (for we ſuppoſe them far greater than the particles of the air) to one another may probably enough be aſcribed to this, that when, for inſtance, the ſmooth ſurfaces of two pieces of glaſs do ſo exquiſitely touch one another, that none of the ambient air is either intercepted or admitted between them, then the undermoſt glaſs will ſuffer no preſſure on that ſide, which touches the uppermoſt; the parts of the uppermoſt glaſs having no ſenſible ſpring in them (ſo that they can only reſiſt, but not repel the other:) but that ſide of the undermoſt glaſs, which is expoſed to the air, will be preſſed upon thereby; and there being, as we ſaid, no elaſtical preſſure on the other ſide of the glaſs to balance this, it is not to be wondered at, that the inferior glaſs ſhould not fall off from the other, in regard the weight, that would carry it downwards, is much too ſmall to overcome that force of the air, that thruſts it againſt the uppermoſt glaſs: as if one ſhould with his hand thruſt a plate of iron broad-wiſe againſt the flat cieling of his chamber, the iron would not fall as long as the force of the hand perſeveres to preſs againſt it. Nor is it material, that in our caſe the preſſure of the atmoſphere is ſuppoſed to force the lowermoſt glaſs upwards; for if we ſuppoſe the air to conſiſt of innumerable little ſprings (as it were) bearing upon and ſupporting one another, and whereof the lowermoſt are bent by the weight of all that are incumbent on them, it will be eaſy to conceive, that near the ſurface of the earth, (about which the air muſt diffuſe itſelf by reaſon of the gravity of its ſmall parts, and the reſiſtance of the earth againſt their deſcent) it may preſs almoſt equally every way, and by a kind of recoil (though not properly ſo called) from the terreſtrial globe upwards, may ſtrongly preſs any body, upon which it can bear, againſt any other, which has no ſuch elaſtical power to repel from it a body ſo preſſed againſt it.

THIS difficulty being thus diſpatched, we ſhall proceed by two or three particulars, to confirm our conjecture at the cauſe, why ſmooth bodies ſtick together upon bare juxta-poſition or contact. And firſt I obſerve, that if a piece of flat glaſs be, as we formerly mentioned, appended to a looking-glaſs held with the unfoliated ſide downwards, parallel to the horizon, though the adhering glaſs will not drop down, yet it will very eaſily be moved any way along the level ſurface; and if by inclining the looking-glaſs any way, you deprive it of its former level, the ſmaller glaſs will eaſily ſlide downwards upon the ſurface of the greater. Of which the reaſon ſeems to be, partly that the gravity as ſuch of the lower glaſs does not conſiderably reſiſt the horizontal motion of it, but only the motion upwards, whereby it muſt recede from the centre of heavy bodies, as might if need were, be probably deduced from divers inſtances obvious enough; and partly, or rather chiefly, that to the edges of the glaſs the ambient air is contiguous as well on the one ſide as on the other, and ſo the preſſure of the air being equal on every part of the edges, the gravity of the ſmaller glaſs is not hindered by the air (which can as faſt ſucceed on one ſide, as it is diſplaced on the other) from making it ſlide down the ſhelving ſurface of the greater glaſs; whereas of the broad and flat ſides of the lowermoſt glaſs the one is, as we ſaid, preſſed by the ſpring of the air, whilſt the other ſuffers no ſuch preſſure from the looking-glaſs, to which it was applied. And ſo, if you take a ſmall open-mouthed glaſs, and plunge it into a veſſel full of quickſilver with the mouth upward, that the quickſilver may fill it without leaving any air in it, and if then, whilſt it is under the quickſilver, you turn the mouth downwards, and ſo keeping it upright, lift it up till the mouth be almoſt come to the top of the mercury you ſhall perceive, that the glaſs will remain almoſt full of quickſilver in the veſſel: and this will continue ſo, though you incline the glaſs this way or that way, provided you keep the mouth of it within the mercury. And this experiment, though more noble when tried with quickſilver, will ſucceed alſo when tried, as it may more eaſily be, with water. Of which the reaſon ſeems to be, that the glaſs hinders the quickſilver in it from the preſſure of the incumbent air, whereas the quickſilver in the veſſel being expoſed to it, muſt by it neceſſarily be forced up againſt the ſurface of the inverted bottom of the glaſs, where it meets no elaſtical power to repel it downwards. For, that it is not nature's abhorrency of a vacuum, that keeps the quickſilver from deſcending till ſome air can come to ſucceed in its room, the famous experiment invented by *Torricellius*, and found true by many others, and ourſelves, touching the deſcent of the quickſilver in any tube of above two foot and a half long, ſeems clearly to evince. And to confirm what we have ſaid, and ſhew withal, that it is not ſo much the contact of bodies according to a large ſurface, as that contact is conſidered in itſelf, as by reaſon of its being ordinarily requiſite to the excluſion of air, that at leaſt here below keeps bodies from falling aſunder; I ſhall relate, that having by a certain artifice out of a large glaſs (with a narrow mouth) cauſed a certain quantity of air to be ſucked, we found, that by immediately applying a book (which then chanced to lie at hand) to the orifice of the veſſel, the book was readily lifted up and ſuſtained in the air as long as we pleaſed, though the ſurface of the ſuſpended body could be touched, as is evident, but by the ring, which incircled the orifice of the veſſel, and though the weight taken up (beſides that it was inconveniently ſhaped for ſuch a trial, which would probably have ſucceeded as well with a much greater weight, if we had had one fitly ſhaped at hand) exceeded twenty ounces *. Of which event the cauſe ſeems plainly to be this, that by reaſon of the exſuction of ſome air out of the glaſs, the elaſtical power of the remaining air was very much debilitated in compariſon of the unweakened preſſure of the external air, which being able to preſs the book againſt the orifice of the veſſel with greater ſtrength than the internal air can reſiſt, thereby it comes to paſs, that the whole orifice of the veſſel, though there be but part of it of ſolid body, does yet on this occaſion perform in ſome meaſure the part of an entire ſurface exactly ſmooth.

IT may be conſidered alſo (to add that upon the bye) whether upon the principle lately explicated may not in ſome meaſure depend the ſolidity of glaſs. For though its parts ſeem very little or not at all branched or interwoven one within another, and appear very ſmooth and ſlippery, yet ſince the fire that brought them to fuſion, and conſequently to be fluid, may well be ſuppoſed to have ſub-divided and reduced them into ſmall particles, and to have thereby aſſiſted them to exclude the air from betwixt them, it may ſeem, that it needs not much be wondered at, if the immediate contact of ſuch ſmall and ſmooth corpuſcles ſuffice to make them hold together; for that their union is ſtrict enough to keep out the air, may appear from this, that thoſe that blow glaſſes, and thoſe that diſtil in them, find not the air can traverſe the pores even of heated glaſs: and as for any more ſubtil matter, we ſee by the free paſſage of light and heat, or, to ſpeak more warily, of magnetical effluvia through the glaſs, without injuring its texture, that ſuch matter, but moderately moved, will not hinder the little ſolid parts from cleaving together. And on this occaſion it might be conſidered, how much the juxta-poſition of their corpuſcles crouded together by fuſion may contribute to the conſiſtence and brittleneſs of ſalt-petre, and divers other bodies, which may from an incoherent powder be readily turned into one maſs; as alſo how far the ſticking together (for I ſpeak not of the figures compoſed by them) of the ſmall parts of hanging drops of water, and ſuch other liquors, as are not thought to conſiſt of corpuſcles hooked or branched, may be aſcribed to the contact of their ſmall parts, and the excluſion of air. Theſe I ſay, and ſome other ſuch things might be here conſidered, but that we are forbidden to examine them particularly, and eſpecially what has been repreſented touching the ſolidity of glaſs, (which we ſuſpect another cauſe may have a great intereſt in) by our haſte, which calls us to the remaining part of our diſcourſe.

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* Much more conſiderable inſtances of this nature may be met with in the author's *New Phyſico-Mechanical Experiments.*

THOUGH then it be hence (to omit other proofs elſewhere mentioned) ſufficiently manifeſt, that the air has a ſpring, and that a ſtrong one, yet there appears no great neceſſity of having recourſe to it for the giving an account why the two ſmooth glaſſes above-mentioned were able to adhere ſo cloſely to each other: for a probable reaſon of the ſame phænomenon may be rendered by the preſſure of the air conſidered as a weight. And firſt, we muſt recall to mind what we (a little above) ſaid of the recoiling (or rebounding of the preſſure) of a cylinder of air from the earth, to the ſuſpended piece of glaſs, proceeding from this, that the fluid air, which is not without ſome gravity, being hindered by the reſiſting ſurface of the terreſtrial globe to fall lower, muſt diffuſe itſelf, and conſequently preſs as well upwards as any other way. Next, we may conſider, that when the ſurfaces of two flat bodies of any notable (and, for example, of equal) breadth do immediately touch each other, and lie both of them level with the horizon, and one of them directly over the other; in this caſe, I ſay, ſince the air cannot move in an inſtant from the edges to the middle of the two ſurfaces that lie upon each other, the lowermoſt cannot be drawn away downwards in a perpendicular line from the uppermoſt, but that by reaſon of the ſtiffneſs and contact of the two bodies, it muſt neceſſarily happen, that at the inſtant of their ſeparation, ſhould it be effected, the lowermoſt glaſs will be preſſed upon by the whole (crooked) pillar of air, ſuppoſed to reach from the top of the atmoſphere, and to have for baſis the ſuperficies of the undermoſt glaſs. For at that inſtant, the air having not time to get in between the two glaſſes, there is nothing between them during that inſtant to reſiſt the preſſure of that air, which bears againſt the lower ſuperficies of that undermoſt glaſs, and conſequently ſuch a revulſion of the lower glaſs cannot be effected, but by a weight or force capable to ſurmount the power of the weight of the above-mentioned cylinder of the atmoſphere; and this, as I ſaid, becauſe that by reaſon of the ſudden ſeparation, the upper ſurface of the glaſs has not any air contiguous to it, which, were it there, would (according to the nature of fluid and ſpringy bodies) preſs as much againſt the upper ſurface of the glaſs, as the pillar of the atmoſphere againſt the lower, and conſequently ſuſtain that endeavour of the air againſt the lower ſide of the glaſs, which in our propoſed caſe muſt be ſurmounted by the weight or force employed to draw down the lower glaſs. And hence we may underſtand (to add that upon the bye,) that it is not neceſſary, that the contiguous ſurfaces of the two flat glaſſes, we have been treating of, be paralleled to the horizon: for if you ſhould hold them perpendicular to it, their divulſion would not ceaſe to be difficult, provided it were attempted to be made by ſuddenly pulling one of the broad ſurfaces from the other in a level line, and not by making one of the ſurfaces ſlide upon the other; for in the former caſe, the ſeparation of the contiguous bodies will be hindered by the weight or preſſure of the lateral air (if I may ſo ſpeak) that bears againſt the broad ſides of the glaſſes contiguous to it. But whereas in theſe caſes we ſuppoſe the ſuperficies of the two glaſſes to be ſo exactly flat and ſmooth, that no air at all can come between them; experience has informed us, that it is extremely difficult, if at all poſſible, to procure from our ordinary tradeſmen either glaſſes or marbles, ſo much as approaching ſuch and ſuch an exquiſiteneſs: for we could very hardly get either experienced ſtone-cutters or perſons ſkilled at grinding of glaſſes, to make us a pair of round marbles, though of an inch or two only in diameter, that would for ſo much as two or three minutes hold up one another in the air by contact, though they would eaſily enough take up each other, if the uppermoſt were drawn up nimbly, before the air could have leiſure to inſinuate itſelf betwixt them.

BUT this notwithſtanding, we endeavoured by the following expedient, not only to manifeſt, that the power or preſſure of the air is in theſe experiments very great, but alſo to make ſome eſtimate (though but an imperfect one) how great the power is.

HAVING then provided a pair of marbles of an inch and half in diameter, and as flat and ſmooth as we could get, and having conſidered, that as it was the getting in of the air between them, that (for the reaſon above declared) hindered them from ſticking ſtrongly together; ſo the acceſs afforded to the air was for the moſt part due to that ſcarcely evitable roughneſs or inequality of their ſurfaces that remained in ſpite of the poliſh: conſidering theſe things, I ſay, we ſuppoſed, that the intruſion of the air might be for ſome while prevented by wetting the ſurfaces to be joined with pure ſpirit of wine; and that yet this liquor, that ſeems the freeſt that we know of from tenacity, would not otherwiſe than by keeping out the air prove a cement to faſten the ſtones together. But becauſe the eaſy ſeparation of ſuch ſmooth bodies, as adhere but by contact does in great part (as we formerly noted) proceeds from this, that whereas it is very difficult to hold ſuch bodies exactly level for any conſiderable ſpace of time, and yet the leaſt inclination any way gives the lower body opportunity to ſlide off; becauſe of this, I ſay, we reſolved in the firſt place to ſee what could be done by faſtening to the upper marble certain wires and a button, in ſuch manner, as that the lower marble, when it was joined, might freely fall directly down, but not ſlip much aſide, being hindered by the wire. And in purſuit of this we found, that not only the dry marbles could be made to take up and hold up one another, but that once by drawing up the upper marble nimbly, we could take up (but not keep up for any time) together with the lower marble, a ſcale, and in it a pound weight of 16 ounces Troy.

AFTER this we moiſtened the ſurfaces of the marbles with ſuch pure alkalizate ſpirit of wine, as we elſewhere teach to make, which was ſo thin and ſubtile, that not only we burned ſome of it, before we would employ it about this experiment, in a ſilver ſpoon, without leaving ſo much as any ſign of phlegm behind; but it would (in the open air) almoſt in a moment fly away from the ſurface of the marble anointed with it, and leave it dry and gloſſy. The marbles being ſkilfully wetted, and kept by the above-mentioned wires from ſlipping aſide, we caſt into the ſcale faſtened to the lower of them divers weights at ſeveral times, and by nimbly pulling up the higher ſtone, tried many times, how much we could draw up with the lower, and did ſometimes take up above an hundred ounces, and once an hundred thirty-two ounces Troy, beſides the ſcale, that contained them, and the marble itſelf, the diameter of whoſe ſmooth ſurface was by meaſure but about an inch and two thirds.

BUT here I muſt take notice, both in relation to this and the following experiments to be ſet down concerning ſmooth marbles, that we never yet found any ſort of experiments, wherein ſuch ſlight variations of circumſtances could ſo much defeat our endeavours; which we therefore mention, that in caſe ſuch experiments be tried again, it may be thought the leſs ſtrange, if others be not able to do as much at the firſt and ſecond, or perhaps the tenth or twentieth trial, as we did after much practice had made us expert in this nice experiment, and ſuggeſted to us divers facilitating circumſtances, which could not here in few words be particularly ſet down.

And now, becauſe we perceived, that the ſpirit of wine was too fugitive and ſubtile a liquor for our purpoſe, we ſuppoſed, that oil, as it would better fill up the little cavities of the ſtones, ſo it would more exactly keep out the air, and leſs eaſily vaniſh into it. And accordingly, having moiſtened the ſurfaces with a due proportion of good expreſſed oil of ſweet almonds, and having carefully obſerved the other requiſite circumſtances, we took up ſome drams above four hundred ounces Troy hanging at the lower marble.

And that you may not ſuſpect, that it was by gluing the marbles together, that the oil did enable them to make ſo much greater reſiſtance againſt ſeparation than the ſpirit of wine did; I ſhall add, that in caſe the flat ſurfaces of the joined ſtones were held not parallel, but perpendicular to the horizon, that ſo the air might (as we formerly alſo obſerved) immediately ſucceed, as the looſer marble ſhould ſlide off, the weight of ſome ounces was now and then requiſite to draw down the marbles, when they had nothing but ſpirit of wine between them; whereas they would eaſily enough ſlide off from one another, when they were cemented together with oil: perhaps becauſe that the ſpirit of wine, by reaſon of the ſmallneſs and penetrancy of its parts, and becauſe of its fugitive nature, did not ſo well fill up the little pores and furrows of the ſurfaces of the marbles; whereby the little protuberances getting into thoſe little cavities, might more reſiſt the ſliding of the marbles upon one another's ſurfaces, whoſe texture is better fitted to make their ſurfaces ſmooth and ſlippery.

And to ſhew, that the reſiſtance of ſuch contiguous marbles to a violent ſeparation is greater in thoſe, which being broader are preſſed againſt or reſiſted by a proportionably bigger (though not a longer) pillar of the atmoſphere; we cauſed two marbles to be made, whoſe diameter was three inches, or a very little more: and having after the above-mentioned manner employed ſpirit of wine to keep out the air from between them, we did after ſome trials, with the uppermoſt of them take up the lowermoſt, and with it four hundred ſixty eight, or four hundred and ſeventy ounces. But making uſe of oil of almonds inſtead of ſpirit of wine, we did with our own hands draw up twice, one time after another, with the undermoſt ſtone, a much greater weight, namely, eighty-four pound or 1344 ounces (Troy weight) notwithſtanding which weight we manifeſtly perceived the marble, at which it hung, to ſtick ſtrongly to the other.

And here again we will take notice, that the interpoſed oil was ſo far from being able as a cement, rather than by keeping out the air, to make theſe marbles ſtick ſo cloſe together, that, whereas bodies glued or cemented together are wont to make an almoſt equal reſiſtance to their being ſeparated, in what poſture ſoever you place them, I made our marbles, even when we tried this laſt experiment, very freely ſlide upon one another, by impelling the uppermoſt to the right hand or to the left, with my finger or my thumb (whereof the reaſon is intimated above, where we mentioned almoſt the like caſe in glaſſes) and having ſometime before taken up a weight, which we conjectured to be not much inferior to that laſt named, we preſently for trial-ſake held the marbles with their edges downwards, and found, that thoſe, that in an horizontal poſition could not be drawn aſunder by ſo great a weight, did in another poſture preſently fall aſunder by their own weight, which made one readily ſlide off from the other to the ground. Now although we have confeſſed, that this way of meaſuring the force of the air is not accurate; yet we hope it will not be thought uſeleſs, ſince (not to mention, that by thus breaking the ice, we may make way for the happier inquiries of others) it not only ſhows us, that this preſſing or ſuſtaining force of the air, as unheeded as it is wont to be, is very great, but it may alſo aſſiſt us to conjecture, how great it is, which though we cannot hereby determine preciſely and with certainty, yet we may eſtimate it with much leſs uncertainty than otherwiſe we could.

I KNOW, that the Peripatetics, and the generality of the ſchool-philoſophers, will confidently aſcribe the ſticking of the marbles, not to the cauſe we have aſſigned, but to nature's abhorrency and fear of a vacuum. But not to engage ourſelves now in a diſquiſition, that, when we diſcourſed of fluidity, we did (for the reaſons there expreſſed) decline to meddle with; we will, without diſputing whether or no nature either can at all admit, or do abhor a vacuum, content ourſelves to confirm the explication given of this phænomenon by theſe two conſiderations. The one, that if nature did ſo violently oppoſe a vacuum as is pretended, it is not likely, that any force whatſoever that we could employ, would be capable to produce one; whereas in our caſe we find, that a little more weight added to the lower of the marbles is able to ſurmount their reluctancy to ſeparation, notwithſtanding the ſuppoſed danger of thereby introducing a vacuum. And my next conſideration is, that (according to what we have hitherto delivered) without having recourſe to any ſuch diſputable principle, a fair account may be given of the propoſed phænomenon, by the preſſure or weight of the air. And that what we have ſaid concerning the latter of theſe may be entertained with the leſs difficulty, let us ſuppoſe, that when the marbles ſtick well together, the lowermoſt of them, or the appendant weight were faſtened to the ground; for in this caſe there appears no reaſon to believe that their power to reſiſt ſeparation would be leſs than it was before. And yet it ſeems evident, that the uppermoſt marble would not be perpendicularly pulled up but by ſuch a force as were at leaſt (I ſay at leaſt) able to lift up a weight equal to that of the laſt mentioned marble, and of a pillar of air having the ſtone for its baſe, and reaching to the top of the atmoſphere; ſince at the inſtance of revulſion, before the air can get in, and ſpread itſelf between the ſtones, there is not, for aught appears, any ſuch body under the upper marble, as can help the hand to ſuſtain the weight both of that marble and the incumbent cylinder of the atmoſphere, which then gravitates upon it, and conſequently upon the hand; becauſe there is no air, nor other equivalent body underneath it, to ſuſtain its part of the weight, as the lower air is wont to do in reference to the heavy bodies that lean on it, and to the weight of the incumbent air. And therefore we need not much marvel, if when only a leſs weight than that of the fore-mentioned pillar of the atmoſphere hangs at the lower marble, it ſhould be capable of being drawn up by the uppermoſt, rather than ſuffer a divulſion from it. As we ſee, that when two bodies being faſtened together, are endeavoured to be drawn aſunder by forces or weights not able to ſeparate them, they will uſually both of them move that way, towards which either of them is the moſt ſtrongly drawn. On which occaſion, I remember what I have ſometimes obſerved in one of the ways of trying the ſtrength of load-ſtones; for if the load-ſtone be able to take up more than its own weight, you may as well lift up the load-ſtone by a knife, as a knife by the load-ſtone. And though one accuſtomed to judge only by his eyes, would have imagined, that when I held the great weights formerly mentioned ſuſpended in the air, there was no ſtrong endeavour to pull up the upper marble from the lower, becauſe my hand being for a while held ſteady, ſeemed to be at reſt; yet he will eaſily be invited to ſuſpect, that in ſuch a thought there may be a great miſtake, who ſhall conſider, that neither did the weight ſenſibly appear to pull the lower marble downwards, though my hand aſſured me that the weight had not loſt its gravitation. And if I ſhall add, that once, when the weight after having been lifted up into the air, was caſually ſo looſened from the upper marble, as ſuddenly to drop down, my hand, unawares to me, was by the force of that endeavour, it juſt before employed to ſuſtain the fallen weight, carried up with ſuch violence, that I very ſenſibly bruiſed it by the ſtroke it gave againſt the face of a by-ſtander, who chanced out of curioſity to hold his head over the marbles.

AND here it will not be impertinent to bring in an experiment that I once deviſed, not only for other uſes, but to illuſtrate the ſubject we have been hitherto treating of. The trial I lately found regiſtered among my *Adverſaria*, in theſe terms. A braſs valve of about an inch diameter was with cement well faſtened to the ſhorter leg (which was but of very few inches in length) of a long glaſs ſiphon left open at the end of the other leg. This valve being let down to the bottom of a tall glaſs body full of water, ſo that it was (if I much miſremember not) between a foot and half a yard beneath the ſurface of the water, when there was let in as much water into the pipe, as reached in that as high as the ſurface of the external water in the tall cucurbite: then about an ounce weight was put into the oppoſite ſcale of a balance, to the neighbouring ſcale, whereof one end of a ſtring was tied, whoſe other end was faſtened to the ſaid valve, whoſe parts would be thereby drawn aſunder. But when the water was emptied out of the pipe, and the valve was let down to the former depth, there was requiſite about five ounces, that is four ounces more than formerly, to disjoin the parts of the valve, and let the water get in between. And when (the ſiphon being freed from water) the valve was lifted higher and higher, together with the pipe, there needed leſs and leſs weight to make a disjunction; two ounces of additional weight (to the one ounce requiſite to counterpoize the cover of the valve itſelf) on the water ſufficing to lift up the cover, when the valve was held about half way, between its lower ſtation, and the top of the water; a ſingle ounce ſufficing afterwards, and half an ounce of additional weight proving enough to disjoin the parts, when the valve was held but a little beneath the ſurface of the liquor.

THIS relation of an experiment, which I afterwards ſhowed to many virtuoſi, will perhaps ſeem ſomewhat dark to you without a ſcheme; but if you conſider it attentively enough to apprehend it thoroughly, I preſume it will ſhow you, that whether or no there be, upon any other ſcore, a repugnancy to the ſeparation of ſmooth bodies joined by immediate contact; yet certainly there may be a great repugnancy upon the bare account of the gravity of the medium, wherein the divulſion is attempted. For in our caſe the fuga vacui, if there be any, ought to reſiſt the ſeparation of the parts of a valve ſtill kept under water, as much near the top of the water, as at the bottom. And therefore the great difference found in that reſiſtance at thoſe different places may be attributed to the preſſure of the ambient water, that thruſt them together. And though it be true, that air is an exceeding light body in compariſon of water; yet in divers trials I have found the diſproportion in gravity of thoſe two fluids not to exceed that of 1000 to 1. So that conſidering how many miles, not to ſay ſcores of miles, the air may reach upwards, there ſeems no abſurdity at all to ſuppoſe that the bare preſſure of it againſt the marbles formerly mentioned may keep them as coherent as we found them to be *.

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* But ſince this I have been able to make an experiment, that does ſufficiently confirm the former doctrine. For having ſuſpended the two coherent marbles in a capacious glaſs, whence by a certain contrivance,

BUT it is now high time to look back to that part of our diſcourſe, which the conſideration of our marbles has ſo long inticed us from directly proſecuting. Although then it may from the paſt diſcourſe be conceived, that in bodies of ſenſible bulk, whoſe ſmooth ſurfaces touch one another, the force of the air does mainly make them cohere; yet it ſeems, that generally in bodies (whether greater or ſmaller) it is a ſufficient cauſe of coheſion, that the parts of the body are at reſt by one another, though perhaps the entire concretion be removed from place to place. For bodies of ſenſible bulk being either fluid or conſiſtent, and it being (as above we have taught) the chief requiſite of a fluid body, that its ſmall parts be in motion, there ſeems not any thing neceſſary to keep a body from being fluid, and conſequently to keep it a firm body, but that its contiguous parts be in a ſtate of reſt.

I KNOW, that almoſt all philoſophers both antient and modern require ſomething elſe than the reſt of the parts (of which ſcarce any of them takes the leaſt notice, as of a thing conducive to firmneſs) to the keeping together the parts of a dry and ſtable body. But although to engage very far in ſuch a metaphyſical and nice ſpeculation were unfit for me (at leaſt at preſent, when I am but to endeavour to explicate fluidity and firmneſs in the ſenſible bodies we converſe with;) yet we dare not quite ſkip it over, leſt we be accuſed of overſeeing it. The greater number of contemplators aſcribe the effect under conſideration to a certain ſubſtantial form, to which they aſſign, among other offices, that of keeping all the parts united into one body. But what this form is, and by what means it unites the parts ſo ſtrongly in a diamond or a ruby, *&c.* and ſo looſely in tallow, camphire, or the like ſlight concretions; and how the ſubſtantial form continuing the ſame in water and ice, the ſame matter may eaſily and frequently become by turns a hard and a fluid body; how theſe, I ſay, and divers other things are effected by the forms of ſolid bodies, is to me, I confeſs, at leaſt as difficult to conceive, as to imagine without it a cauſe of coheſion in the parts of a dry body.

OTHER learned men there are among the modern naturaliſts, who have recourſe ſome of them to a ſpirit, which penetrating and faſtening to each other all ſubſtances corporeal, unites them into one world; but others fancy rather a certain cement or glue, whereby they conceive the parts of bodies to be made as it were to ſtick to each other. But as for this laſt hypotheſis, it would be conſidered, that though glue is made uſe of to join together bodies of ſenſible bulk, yet glue itſelf being a body, that is ſo too, it muſt alſo itſelf conſiſt of leſſer parts ſticking to one another; which allows me to demand the cauſe of a mutual coherence of thoſe parts. And if it be anſwered, that they likewiſe ſtick together by the intervention of ſome more ſubtile glue, I ſhall again repreſent, that this glue alſo muſt conſiſt of corporeal parts; and therefore I ſhall further demand how theſe alſo ſtick together: and if the like anſwer be again made me, I ſhall ſtill renew the like demand, till at length the anſwerer be reduced to confeſs, that parts of matter ſo very ſmall cannot be reaſonably ſuppoſed to be kept together by a cement. And if the corpuſcles, that make up the fineſt

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vance, the air could little by little be drawn out, we found, as we expected, that whilſt there remained any conſiderable quantity of air, in the glaſs, the lower marble continued to ſtick to the other, the preſſure of the remaining air, though but weak, being yet ſufficient for the ſuſtentation of the lower marble, which it was not after the air was further withdrawn. And if when the disjunction was made, the upper marble were by another contrivance let down upon the lower, ſo as to touch it as before; though whilſt the external air was kept out of the glaſs, the upper part might eaſily be raiſed without taking up the lower with it; yet when the outward air was let in, the marbles were preſſed together, and became again ſtrongly coherent.

glue imaginable, are not kept together by a cement, we may be allowed to aſcribe their adheſion to the immediate contact and reſt of the component parts (which is a cauſe intelligible, and at leaſt probable) till ſome other ſufficient cauſe be aſſigned, which I do not take that to be, which is taught by the patrons of the firſt hypotheſis lately mentioned, concerning a ſpirit diffuſed through the maſs of matter. For not to mention, that the agility of a ſpirit ſeems not ſo proper a qualification for that, which is to faſten bodies together, we may conſider, that this ſubſtance which is called a ſpirit, is indeed but a ſubtile body. And why therefore may not the minute parts of other bodies, if they be conveniently ſhaped for adheſion, ſtick to one another, as well as ſtick to this ſpirit? And I ſhould here alſo demand, how the parts of this ſpirit are kept faſt to one another. If any ſhould anſwer, that this ſpirit conſiſts of parts, which are inſeparable, and yet perhaps of a hooked ſhape, which fits them to faſten themſelves to the bodies they take hold of, and thereby thoſe to one another; this would be to propoſe ſuch a new notion of the diffuſed ſpirit, as I know not whether thoſe, whoſe opinion I have been examining, did ever dream of, or would be content to adopt: and ſure, according to this hypotheſis, there muſt be a wonderful plenty of theſe little ſpirits in the groſſeſt bodies; and ice for example, which is thought ſo deſtitute of ſpirits, muſt be well nigh half made up of them: for theſe little ſpirituous parts can faſten no parts of other bodies together but thoſe they touch, ſince otherwiſe the parts of other ſorts of matter, if but contiguous, might cohere without theſe, which is againſt the hypotheſis. And ſince each of theſe ſmall ſpirituous corpuſcles, if I may ſo call them, being really a natural body, and by conſequence neceſſarily diviſible, at leaſt by thought, into parts, I ſhall aſk the propoſers of this new notion of ſpirit, upon what account this corpuſcle can be indiviſible; I mean, what it is, if it be not reſt and immediate contact, that hinders, but that the parts (or deſignable parcel of corporeal ſubſtance) which are diviſible by thought, ſhould be always kept together, and never be actually divided. I am not averſe indeed from granting, that they may almoſt always eſcape diſſolution: but I am apt to ſuſpect, that may be, becauſe that by reaſon of the extreme ſmallneſs, and the reſt and ſtrict contact of their parts, they can ſcarce ever meet with an agent minute and ſwiftly enough moved, to be able to ſhatter them, or diſſociate the combined parts. For to ſay, that it is the nature of every ſuch corpuſcle to be indiviſible, is but to give me cauſe to demand how that appears; for ſo important an aſſertion needs more than a bare affirmation for proof.

And if two of theſe corpuſcles, that are preſumed to be indiviſible, ſhould, being ſmooth and of the ſame figure (as for example cubical) happen to lie upon one another, and a third ſhould likewiſe chance to be fitly placed upon the uppermoſt of the two, what ſhould hinder, but that this aggregate may by the violent knock of ſome other corpuſcles be broken in the midſt of the whole concretion, and conſequently in the middlemoſt body? For ſuppoſe them as adamantine as you pleaſe, yet ſince corpuſcles as hard as they are, can be made very violently to knock againſt them; why may not theſe grate or break the middlemoſt corpuſcles, or any of the others? as we ſee that diamonds themſelves may be reduced to powder by other diamonds, though not (as artificers verſed in the trade inform me) by attrition with any other ſtone. To prove, that the coheſion of the middlemoſt of the three lately mentioned ſmall dyes with the other two, the one above it, the other below, is not ſo ſtrong as that of the parts of that middlemoſt corpuſcle, notwithſtanding that the contact between each two adjoining bodies is ſuppoſed to be full (for ſo it muſt be in ſuch bodies, though not always in others viſibly greater, in which ſome ſubtiler ſubſtance may be ſuppoſed to come in part between them;) to prove this, I ſay, there muſt be aſſigned ſome better cauſe of the coheſion of the matter in one part of the propoſed body than in the other. And it cannot with probability be pretended, that a corpuſcle preſumed incapable to be divided, ſhould conſiſt of hooked parts: and if that ſhould be pretended, yet even theſe hooks alſo being true bodies, the queſtion would recur concerning them, and be ſtill renewable *in infinitum*. If it ſhould be ſaid, that theſe minute bodies are indiſſoluble, becauſe it is their nature to be ſo; that would not be to render a reaſon of the thing propoſed, but in effect to decline rendering any. And though I know, that in every hypotheſis about the principles of things, ſomething is allowed to be aſſumed, as not being to be explained or proved by any thing more primary than itſelf; yet I know not whether this excuſe be proper in our caſe, wherein it ſeems, that the entireneſs and permanency of any parcel of matter, how minute ſoever, may be probably enough deduced from the immediate contact, the reſt and the extreme littleneſs of its deſignable parts. And if for a laſt refuge it ſhould be ſaid, that the deſignable parts of theſe corpuſcles are therefore unſeparable, becauſe there is no vacuity at all intercepted between them; beſides that this is contrary to the ſuppoſition formerly made; for ſuch extremely minute dyes as we imagined to be one upon another, having their ſurfaces, according to our poſtulatum, flat, ſmooth, and exquiſitely congruous, could no more than the parts of either of the three corpuſcles have any vacuity intercepted between them: beſides this, I ſay, this is both to ſuppoſe a vacuum in all diviſible bodies, and that too as the cauſe of their being ſuch, and to decline the former hypotheſis touching the uſe of this ſpirit, and take ſanctuary among the Atomiſts, to whoſe opinion about the account, upon which thoſe bodies they call atoms are not diſſipated, although ſome of the conſiderations we have alleged againſt the newly-examined opinion may in part be applied; yet divers of their other opinions do ſo fairly comport with the generality of our experiments in theſe notes touching *Fluidity and Firmneſs*, that I am willing to decline claſhing with them, by not purſuing now any further a diſquiſition, which, as I ſaid a while ago, is not neceſſary to my preſent deſign: eſpecially, ſince the dim and bounded intellect of man ſeldom proſperouſly adventures to be dogmatical about things, that approach to infinite, whether in vaſtneſs or littleneſs. Nor indeed would I have that looked upon as a reſolute declaration of what I think of ſo abſtruſe a ſubject, which I have rather propoſed to avoid ſaying nothing, where I ſuppoſed it expected I ſhould ſay ſomething.

THE other thing then, which in our deſcription of a firm body we mentioned as capable to make it ſo, is the texture of the parts (whether homogeneous or not) that conſtitute it; and though the juxta-poſition and reſt of theſe parts may poſſibly alone ſuffice to make the body ſtable; yet this texture ſeems to be the moſt uſual cauſe of ſtability, and ſometimes alſo it may ſuper-add a degree of that quality to that, which bodies may have upon the former account only. For, though whilſt the parts of the body are actually at reſt, it cannot be fluid, yet thoſe parts, if they cohere to one another but by reſt only, may *cæteris paribus* be much more eaſily diſſociated and put into motion by any external body actually moved, than they could be, if they were by little hooks and eyes, or other kind of faſtenings intangled in one another; it being often neceſſary in this caſe violently to break off theſe faſtenings, before the little bodies faſtened together by them can be diſjoined, and put into ſuch a ſeparate motion, as is requiſite to the conſtituting of a fluid body.

WE formerly made uſe of that familiar ſubſtance, the white of an egg, to illuſtrate the nature of fluidity: let us now try, whether it will alſo aſſiſt us in our inquiry after the cauſes of ſtability. When an egg is made hard by boiling, ſince whether we ſuppoſe this induration to be effected by bare motion or impulſe, or elſe by the inſinuation of fiery corpuſcles; ſince I ſay there is nothing, that appears to get in at the ſhell, unleſs perhaps ſome calorific atoms, and, perchance too, ſome little particles of the fluid water it is boiled in, it is not eaſy to diſcover, from whence elſe this change of conſiſtency proceeds, than from a change made in the texture of the parts, whereby they are connected and diſpoſed after a new manner, fit to make them reciprocally hinder the freedom of each other's motions. But if inſtead of hardening the whites of eggs by the heat of the fire, you beat them very well into froth, you may perceive that froth to emulate the nature of a ſtable body: for not only you may raiſe it up to a pretty height, and make it retain a ſharp top almoſt like a pyramid; but I remember I have, for curioſity-ſake, made with a little care a long and proportionably thick body of theſe bubbles hang down from my finger without falling, like an icicle from one of the reeds of a thatched houſe; and yet in this there appears not any alteration to be made in the fluid body, ſave a meer mechanical change of the diſpoſition of its parts: which may be confirmed by water beaten into froth, for there the heaped bubbles will quickly ſubſide and fall back into water of the very ſame conſiſtence it was of before.

Now there may be ſeveral things, whereby a body may be put into ſuch a texture, as is convenient to make it firm or ſtable. And of theſe, before we conſider of them particularly, it will be fit to take notice in general, that for the moſt part it is not from any of them ſingle, but from two or more of them concurring, that the ſtability of concretions proceeds.

THE firſt and chiefeſt of theſe ſeems to be the fitneſs of the ſhapes of the component particles to faſten to each other; as if ſome were figured like the handles of buckets, and others like the hooks, that are wont to be employed to draw them up out of the well, or ſome like buttons, others like loops, ſome like male, others like female ſcrews (as mechanicians ſpeak) or as if many together were ſo variouſly branched, that their parts may be ſo interwoven one within other, as not to be eaſily ſeparable (as we often ſee in a well-made dry hedge, of which if a man go to pull away one bough, he ſhall often be unable to do it without pulling away with it divers others, whoſe ſlender twigs will be intangled with it). An eminent example of the power of the bare texture of many ſmall bodies (even ſuch as each of them apart is not, perhaps, extraordinarily ſhaped for ſuch a purpoſe) to make a ſtable one, is afforded us by ropes and cables; where only by twiſting together and wreathing the ſlender and flexible threads the cable is made up of, they are ſo well, as it were, wedged in between and faſtened to one another, that they conſtitute a body not to be broken by the weight of an iron anchor, nor, perhaps, by the force of a ſhip violently driven on by the fury of the winds and waves. This figuration of the corpuſcles, that make up conſiſtent bodies, ſeems to have been the chief, if not only, cauſe of their conſiſtence, in the judgment of the antient Atomiſts, this being the account, that is given of it by *Lucretius:*

Denique quæ nobis durata ac ſpiſſa videntur,
Hæc magis hamatis inter ſeſe eſſe neceſs' eſt,
Et quaſi ramoſis alte compacta teneri.
In quo jam genere imprimis adamantina ſaxa
Prima acie conſtat, ictus contemnere ſueta,
Et validi ſalices, ac duri robora ferri,
Æraque quæ clauſtris reſtantia vociferantur.

AND indeed, ſo innumerable may be the correſpondent figures, which are fit to faſten bodies to one another, that it is very poſſible, that two bodies, whereof each apart is fluid, may upon their conjunction immediately intangle their parts in one another, and thereupon acquire ſuch a new texture, that their parts cannot as formerly diſſociate themſelves at pleaſure, and move along one another's ſurfaces, nor conſequently flow after the manner of liquors, but are ſo connected or intangled, that the motion of one of them will be reſiſted by many, and ſo the whole body will become firm or ſtable. Something like this may be ſeen in the experiment mentioned by our author, where he teaches, that the diſtilled liquor of nitre, and that made *per deliquium* out of fixed nitre, will preſently, upon their mixture, in part concoagulate into ſaline, and conſequently ſtable, bodies; but this ſeeming only a re-union of the ſaline particles, that did, though inviſibly, ſwim up and down in the aqueous parts of the mingled liquor, which after this ſeparation remains both more copious than the ſaline parts, and as fluid as before, we will add a noble inſtance (mentioned to another purpoſe by *Lully* and *Hartman*) to declare, how much the firmneſs of bodies depends upon their texture. If you take then the alcohol or highly-rectified ſpirit of wine, and exquiſitely dephlegmed ſpirit of urine, and mix them in a due proportion (as I remember the laſt time I made the experiment I took about equal parts by gueſs, though two of the former to but one of the latter, if this be excellent, be a better proportion) you may in and about a minute of an hour turn theſe two fluid liquors into a conſiſtent body; and I confeſs it was not without pleaſure, that I have immediately upon the ſhaking of theſe two liquors ſeen them ſhoot into the likeneſs of ſnow, and acquire ſuch a conſiſtence, that I could without ſpilling the mixture turn the veſſel, that contained it, upſide down.

BUT I dare not expect to have this experiment believed, even by moſt of them that ſhall try it, experience having taught me, that it will not ſucceed, unleſs the ſpirits of urine and of wine be both of them more exactly dephlegmed, than is uſual even among chymiſts. Yet ſo much more does this coagulation ſeem to depend upon the ſalt of urine as of ſuch a texture, than barely as urinous, that we will add that, as the ſpirit of fermented urine is not (whatſoever ſome eminent chymiſts may think or ſay) ſo indiſpenſably requiſite, but that my curioſity leading me to try, whether other liquors, which I ſuppoſed to be of a reſembling nat[illegible], might not ſerve the turn, I found, that ſufficiently-rectified ſpirit of hartſhorn (to mention that alone here) may be made to ſupply its place: ſo I endeavoured to make it probable by this *, that having tried a certain method (though that may ſeem ſtrange to moſt chymiſts) of ſo ordering urine, that without ſtaying at all to ferment or putrefy it either forty days, or half ſo many hours, I can make the volatile or ſaline ſpirit aſcend firſt in diſtillation, though I uſe but ſome ſuch gentle heat as that of a bath: having, I ſay, by this means diſtilled a very ſtrong ſpirit of unfermented urine, and rectified it too, I found, as I expected, that I could not by any means make it coagulate with ſpirit of wine, which ſeemed to proceed from the differing texture of this ſpirit from that of fermented, or rather putrefied, urine; ſince I had added nothing to the freſh urine I diſtilled, but what was extremely fixed, and belonging (as chymiſts ſpeak) to the mineral kingdom. Whereupon having had the curioſity to inquire of ſome of my chymical acquaintances, I found, that, they complained, that they had not been able to coagulate ſpirit of wine with the ſaline ſpirit made of meer urine, without any addition at all, when they diſtilled that urine without a previous putrefaction

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* This way the author afterwards publiſhed in another book.

(which is not wont to be perfected under ſix weeks or thereabouts). But to return to our coagulum, we will annex, that this is further remarkable in this experiment, that this white coagulated ſubſtance being put into a glaſs veſſel exactly ſtopt and kept in a gentle heat (which yet itſelf is not, perhaps, neceſſary, though expedient) for ſome weeks or months, will at leaſt for the greateſt part by much (for I have not yet totally ſeen it do ſo) reſume the form of a limpid liquor; as if either all the crooked particles, that connected the ſmall coalitions of the vinous and urinous corpuſcles to one another, were, by the motion they were put into by the external heat one after another, broken off, or elſe the ſame little concretions (for the menſtruum to conſiſt chiefly of them, being able to perform other matters than either of the ſingle liquors whereof it is conſtituted) either aſſiſted by outward warmth, or enabled thereto by ſome other cauſe of mobility, did, after many and various attempts to clear themſelves of each other, little by little ſo unbend or break off the crooked particles, that intangled them, as at length to extricate themſelves, and become capable of freely ſhifting places among themſelves, and ſo of the form of a liquor.

And here I ſhall add a couple of experiments, for the ſake of their affinity with ſome parts of the newly-recited experiments about the vino-urinous coagulate.

And firſt it ſeemed to me worth trying, whether ſome acid ſalts, being duly ordered, would not concoagulate with ſpirit of wine, as well as with urinous ſalts; and having for a while digeſted together in a convenient proportion pure *ſaccharum Saturni*, made with ſpirit of vinegar, and rectified ſpirit of wine, I found the mixtures ſo changed in point of conſiſtence, that upon inclining the glaſs, which contained it, none of it would run down the ſides. But this experiment did not afterwards ſeem to me either eaſy or conſiſtent, nor is it more than one of the ways, and I doubt none of the beſt, of attempting what we have propoſed.

The other experiment I promiſed you, relates to the reſolution of the coagulum of ſpirit of wine (which to be performed by digeſtion requires a very long time). And I wiſh I had not ſome reaſons to hinder me from communicating to you the way of making of it at preſent; it being an experiment that ſeems ſomewhat ſtrange in its kind; but that part of it, which is directly pertinent to our preſent argument, you will, I truſt, believe upon my relation, which is, that by the addition of nothing but of a very fixed and very dry body (inſomuch that it will not yield any thing by the common way of diſtillation, even in a naked fire) by the ſole addition, I ſay, of this dry body, the newly-mentioned coagulum, which is alſo a conſiſtent body, may in a few hours be brought into a permanent liquor (quite diſtinct from the dry body) which, when experience firſt recommended this way to me, was of too ſubtile and penetrating a nature, not to make me expect from it conſiderable effects, both in chymiſtry and phyſic, of which intimation you may be pleaſed to take notice.

But to return to what I was about to ſubjoin after the mention of our coagulum, as that is an example of firmneſs produced by texture; I will here, becauſe it is not eaſy to procure ſpirits pure enough to make ſuch a concretion as that; I will here, I ſay, ſet down another way of ſpeedily hardening one fluid body by another; for if you take the white of an egg, and beat it till it become thin, and then ſhake well into it about half its quantity (perhaps much leſs might ſerve the turn) of right ſpirit of ſalt, you ſhall have in a few minutes the mixture ſo coagulated, that I remember when we turned the glaſs, wherein we made it, upſide down, not a drop of liquor did run out, though ſome hours after we obtained a little by breaking the curdled matter. And another experiment much of the nature of this is ſaid to be delivered by Sir *Francis Bacon*, who teaches to coagulate whites of eggs with ſpirit of wine:

and indeed, if you obſerve a circumſtance (unmentioned, that I hear of, by him) which is the ſhaking of the two bodies well together, and if your ſpirit of wine be good, the experiment will ſucceed very well; inſomuch that I remember I have made this way a coagulum, from which no liquor would drop down in about a minute of an hour. But whereas this great naturaliſt conceives this hardening of the egg's white to be performed by the heat of the ſpirit of wine, I ſhall willingly confeſs he has aſſigned the cauſe ingeniouſly, but muſt doubt, whether he have done it truly: for there are divers things, that ſeem to argue ſpirit of wine, as inflammable as it is, to abound with a piercing ſalt, and that ſuch ſaline corpuſcles may ſuffice to curdle whites of eggs, our freſhly-mentioned experiment of curdling the white of an egg with ſpirit of ſalt does ſufficiently declare; and not only we have performed the like effect with ſome other acid ſpirits, and particularly that called oil of vitriol, but it may be produced, though more ſlowly, even by a crude ſalt; for by long beating the white of an egg with a lump of allom, you may bring it for the moſt part into white curds. So that if we will allow the coagulation we treat of to be performed by the ſpirit of wine as hot, it ſeems, that that heat muſt be only ſuch, as may be aſcribed to the active particles of ſaline bodies, which yet are commonly accounted rather cold than hot. But becauſe I ſomewhat doubt, how juſtly they are reputed ſo, I will add, that I did purpoſely for trial-ſake, take the ſerum or whey, that is wont to ſwim upon men's blood after it is cold and ſettled, and endeavoured in vain to coagulate it with ſuch ſpirit of wine as would coagulate eggs; and yet this whey will at leaſt as ſoon, as (if not much ſooner than) whites of eggs, coagulate over a gentle heat of embers: which makes it doubtful, whether the effect proceed not from the greater correſpondency in texture of the ſpirit of wine with one of the liquors than with the other, rather than from the heat aſcribed to it, which did not at all coagulate the whey.

But although we have mentioned ſome examples to ſhew, that two fluid bodies may be aſſociated into a conſiſtent one; yet we want not an experiment to make it appear, that likewiſe by the change of texture a fluid body may be divided into two conſiſtent ones. This experiment, which we have partly taken notice of before (treating of fluidity) is, that having for trial-ſake by convenient degrees of fire diſtilled over a due proportion of the more volatile parts of ſallet oil, neither the liquor, that came over, nor the ſubſtance, that remained behind in the retort, was fluid, though the oil that yielded them had been ſo.

But when I put to the oil before diſtillation a convenient quantity of common ſalt, and one or two other things, that were fit to change the texture of the branched or hookt corpuſcles whereof it conſiſted; I could then obtain an oil of common oil, that both dropt into the receiver in the form of a liquor, and continued a fluid body; which may probably be of good uſe to ſurgeons, varniſhers, and men of ſome other profeſſions.

And to make it the more likely, that by additaments of ſome ſuch nature as that newly-mentioned, ſome groſſer and cloggy parts are retained, or elſe much ſubtilized and otherwiſe altered; I ſhall add, that proſecuting a hint I happened to meet with in the diſcourſe of a wandering chymiſt, I practiſed a way ſo to defecate the dark and muddy oil of amber drawn *per ſe*, that a pretty proportion of it would come over ſo tranſparent and finely coloured, that the experiment did not a little pleaſe thoſe I ſhewed it to. And if it do not appear upon trial, that this way of preparing oil of amber does by detaining ſome parts, which, though more groſs than the reſt, may yet be no uſeleſs one, impair the remedy; and that it does not, upon ſome other ſcore, infringe the medicinal virtue of the oil, the experiment will not be unuſeful.

For the liquor, that is thus prepared, is not only very diaphanous and well coloured, but ſo pure and ſubtile, that it will ſwim, not only upon water, but upon ſpirit of wine itſelf. And it will be no deſpicable thing, if by extending or further applying this experiment to other indiſpoſed bodies, many empyreumatical oils diſtilled by ſtrong fires in retorts can be brought to emulate eſſential oils (as chymiſts call them) drawn in limbics, as to clearneſs and lightneſs.

The additament I laſt thought fit to make uſe of for purifying oil of amber was briefly this. ℞. two pound or ſomewhat leſs of good brandy, one pound of good ſea-ſalt, and half a pound of the oil to be ſubtilized, mix, and diſtil them together.

Upon the mention I made above of the white coagulum of the ſpirits of wine and urine, I remember what I have ſometimes obſerved in the eſſential oil of anniſeeds (as chymiſts ſpeak) diſtilled with ſtore of water in a limbic and refrigeratory; namely, that in the heat of ſummer it would remain a perfect liquor like other chymical oils; but during the cold of the winter, though they, notwithſtanding that ſeaſon, continued fluid as before, the oil of anniſeeds would coagulate into a body, though not of an uniform texture to the eye like butter (but rather almoſt like camphire) yet like it white and conſiſtent, not without ſome kind or degree of brittleneſs.

And on this occaſion I will here inſert an experiment, which ſhould have been ſet down in that part of the former hiſtory of fluidity, where I mention, that the ſmall parts of a body may be ſufficiently agitated to conſtitute a liquor by the air or other agents not ſenſibly hot themſelves. The experiment take thus. Caſting by chance my eyes in the winter-time upon a glaſs of oil of anniſeeds, which ſtood coagulated by the cold of the ſeaſon, I preſently bethought myſelf of making a liquor (whoſe proceſs belongs to another treatiſe) of which, as ſoon as I had prepared it, I made this trial. I melted with a gentle heat the congealed oil of anniſeeds to make it flow, and then covered part of it in another glaſs with a mixture I had provided: and having let them both reſt in the window, I found, that the meer oil being fully refrigerated again, coagulated as before; but that, which was covered with the other liquor, continued fluid both day and night, and in ſeveral changes of weather, and does ſtill remain at the bottom of the menſtruum a clear oil diſtinct from it, though I have purpoſely ſhaken them together to confound them.

And becauſe, *Pyrophilus*, I have not diſcovered to you the menſtruum I made uſe of, I will here preſent you with a ſuccedaneous experiment made with a common liquor. I took then good clear Venetian turpentine, and having ſlowly evaporated about a fourth or fifth part of it, till the remaining ſubſtance being ſuffered to cool would afford me a coherent body (or a fine colophony) I cauſed ſome of this tranſparent and very brittle gum (of which I have elſewhere taught you ſome uſes) to be reduced to fine powder: of which I put into pure ſpirit of wine a greater proportion, than I judged the liquor was capable of diſſolving, to the end, that when the ſpirit had taken up as much of the powder as it could, there might remain at the bottom a pretty quantity of our colophony. On which, though the menſtruum (being already glutted) could not act powerfully enough to diſſolve it, yet it might give the matter (which it had already ſo far ſoftened, as to reduce it into a coherent maſs) agitation enough to emulate a fluid (though ſomewhat viſcous) body. And accordingly I obtained a ſluggiſh liquor, which continued fluid as long as I pleaſed to continue the menſtruum upon it. The like experiment I tried with clarified roſin, and with fine colophony, though but bought at the ſhops; and although the trial ſometimes ſucceeded not ill, yet I found not the ſucceſs conſtant and uniform, whether becauſe the bodies to be diſſolved were not defecated and pure enough, or that I did not hit upon the beſt proportion between the ſolvent and them. But this circumſtance I ſhall not omit, that when the glutinous liquor was ſeparated from the menſtruum, it would by degrees, though but ſlowly, harden in the air. The application of which property, for the preſervation of ſmall and very tender bodies, I ſhall not here more expreſsly hint than by having barely named it. I had forgot to add, that whilſt the ſubſtance continued fluid, I could ſhake it (as I lately told you I could the oil of anniſeeds) with the ſupernatant menſtruum, without making between them any true or laſting union.

WHICH laſt circumſtance brings into my mind another experiment, that I likewiſe forgot to add to that part of the former hiſtory of fluidity, where I take notice, that the particular textures of fluid bodies may be reckoned among the chief cauſes of their being diſpoſed, or indiſpoſed to mingle with one another. For partly to confirm this conjecture, and partly to manifeſt, that it is not univerſally true, which chymiſts are wont to think, that acid ſalts and oils will not incorporate or mingle; I took an arbitrary quantity (and, as I remember equal weight) of common oil of vitriol and common oil of turpentine, as I bought them at the druggiſts: theſe I put together very ſlowly (for that circumſtance ſhould not be omitted) and obtained, according to my deſire, an opacous and very deep-coloured mixture, whoſe almoſt balſam-like conſiſtence was much thicker than either of the liquors that compoſed it. (The like experiment I alſo ſucceſsfully tried with ſome other chymical oils, but found none preferable for this purpoſe to oil of turpentine.) And to make it probable, that the diſpoſition of theſe liquors to mingle thus preſently together depended much on their texture, we made the mixture be warily diſtilled over (for elſe the experiment will ſcarce ſucceed) and thereby obtained (as we elſewhere mention to another purpoſe) a certain groſs ſubſtance, which was that, which ſeemed to mediate the former union betwixt the two liquors. For this ſubſtance being ſeparated, and thereby the texture of one of the liquors (or perhaps both) being changed, the liquors (which came over very clear into the receiver) ſwam upon one another; nor have I ſince been able by ſhaking them together to confound them for any conſiderable time, but they preſently part again, and do to this day remain diſtinct as well as tranſparent. But after having forgot to ſet down theſe things in their proper place, I muſt not forget alſo, that to employ here more words about them were to digreſs.

To this then annex we, that the liquor we elſewhere mention ourſelves to have diſtilled from benzoin, has been, and is ſtill, ſubject to much more frequent viciſſitudes of fluidity and firmneſs: for part of it all the year long continues in the form of a blackiſh oil, and the reſt, according as the ſeaſon of the year or of the day makes the weather cold or hot, frequently changes its texture, ſometimes appearing perfectly the ſame with the newly-mentioned oil, and ſometimes ſhooting into clear and variouſly-ſhaped cryſtals, which faſten themſelves to the bottom and ſides of the veſſel, till a warmer part of the day or of the ſeaſon reſolves them again into a liquor. And theſe two laſt obſervations may alſo ſerve to confirm what we formerly taught, that the fluidity of ſome bodies depended almoſt wholly upon the various agitation of their parts: for in theſe inſtances the parts of the anniſeeds and thoſe of the benzoin, upon the operation or abſence of the languid heat of the ambient air, ſometimes agitating them, and ſometimes ſuffering them to reſt, did conſtitute a fluid or a conſiſtent body. And having thus taken notice of this upon the by, we will add to the other examples mentioned under this ſecond head that, which it afforded us to our preſent purpoſe by ſalt-petre, which being diſſolved in a ſufficient quantity of common water, will ſeem to be loſt in it, and to conſtitute with it one uniform fluid ſubſtance;

but,

but if a competent quantity of that water be boiled (or permitted to exhale) away, and the remaining liquor be ſuffered to reſt a while, eſpecially in a cool place, the ſaline particles will by re-uniting themſelves, and by the excluſion of the aqueous parts, conſtitute ſtable and determinately-figured icicles of cryſtals.

THE conſideration of this may ſuggeſt to us another way, that ſeems quite contrary to the former, whereby ſome bodies may become firm and ſolid; and that is by the intermingling of a due proportion of water or ſome other liquor. For, though the ſmall parts of ſuch fluid bodies, being themſelves in motion, are apt to give thoſe of others ſuch an agitation as we have formerly taught, that fluidity principally depends on; it ſeems, that the admiſſion of any liquor muſt rather conduce to the making of a body fluid than conſiſtent: yet if we conſult experience, it will inſtruct us otherwiſe; for when I have taken either an equal or a double weight of oil of vitriol and diſtilled it warily from running mercury, very much the greater part of the liquor would come over, and leave behind it a very white powder conſiderably fixed. And if we examine that familiar production of chymiſtry *mercurius dulcis*, (which they now uſe to make, by ſubliming together of two parts of crude mercury, with but one of ſublimate, which conſiſts chiefly of mercury already) we may find that in that, which is counted the beſt, the fluid body of quickſilver is ſo contexed with the ſalts it carries up in ſublimation, that the dry and brittle body they compoſe may contain far more (perhaps twice more) quickſilver than ſalt. And other experiments may perſuade us, that the mixture of a convenient liquor may cement bodies into one hard concretion, which would ſcarce be compacted together otherwiſe. Nor is it againſt reaſon, that it ſhould be ſo; for there may be differing qualifications required to a body, whilſt it is conſtituting, and when it is conſtituted; and though the motion of the parts, that make it up, oppoſe the firmneſs of a formed body, yet it may conduce to the making of a firm body. For when a great many hard corpuſcles lie together looſe and incoherent, they do, as we formerly noted, emulate a fluid body; whereas by the mixture of a liquor, thoſe looſe corpuſcles being for a while diſſociated and put into motion, they may after many evolutions apply themſelves to one another after that manner, that is moſt requiſite to make them touch one another cloſely, and according to a greater ſurface. Whereupon it often follows, that the liquor, in which they did formerly ſwim, is either ſqueezed out upon their cloſing, or elſe ſo diſperſed in ſmall particles, and diſpoſed of among thoſe of the harder corpuſcles, that they are unable to agitate them, or prejudice their mutual coheſion.

AND here to dilucidate the ſubject under conſideration by an inſtance, that ſeems very pertinent to it, we will make a further uſe of the experiment formerly mentioned touching the burning of alabaſter. For if the powder, after it has done boiling, and has been ſufficiently burnt, and kept ſome hours (the moſt experienced artificers obſerving, that it is not ſo convenient to employ it preſently after it is taken off the fire) be well beaten and tempered up with fair water almoſt to the conſiſtence of thin pap, if the powder have been rightly prepared and ſkilfully tempered, you ſhall ſee that fluid ſubſtance in a few minutes of an hour begin to ſet (as the tradeſmen ſpeak) that is, to exchange its fluidity for firmneſs; ſo that if it were before caſt into a mould, it will perfectly retain the figure of the internal ſurface thereof.

Now that in our mixture there is for a while ſuch an agitation of the hard parts produced upon the affuſion of the water, and afterwards an excluſion of the ſuperfluous water, we may confirm partly by this, that when any conſiderable quantity of burnt alabaſter is tempered up with water, the mixture after a little time grows ſenſibly hot, and ſometimes continues ſo for a pretty while; and partly alſo by this, that having purpoſely for trial's ſake filled a new and good glaſs phial, containing about half a pint, or pound, with the mixture we ſpeak of, and when it was top full, ſtopped it up very cloſe, the liquid mixture within leſs than half an hour cracked the phial (though ſtanding in a window) in ſeveral places, and at thoſe crevices diſcharged itſelf of about a ſpoonful of clear water; the remaining mixture retaining perfectly the figure and dimenſions of the phial, and growing as hard as chalk, or ſomewhat harder, inſomuch that we were fain to employ ſeveral ſtrokes with a ſtrong iron to divide the maſs.

And let me here add, that ſome other ſubſtances may this way afford much ſolider bodies than burnt alabaſter does; and therefore it may be a thing of good uſe to inquire out and try what other bodies, eaſily to be procured, may be thus brought to a new and laſting ſolidity. For the learned hydrographer, *Fournier*, ſpeaking of thoſe dams or digues (as he calls them in his language) which are ſometimes made in the ſea to ſecure ſhipping, (as I have ſeen at the port of *Genoa* and elſewhere) after having told us, that the Romans made the faireſt harbours in the world by the help of a certain ſand to be met with at *Cuma* and *Puteoli* in the kingdom of *Naples*, which ſand mingled with a third part of quick-lime acquires in the water a flint-like hardneſs; ſubjoins this obſervation of his own, * *J'ay veu, &c.* that is, I have ſeen (ſays he) in *Flanders* near *Tournay* a certain ſort of aſhes of lime made of marble, which was excellent for any kind of work made in the water. For having made a bed of great ſtones, they caſt upon them whole bags full of ſuch aſhes inſtead of mortar, and the water betwixt the ſtones having tempered up theſe aſhes, petrified them to that degree, that in a ſhort time they became as hard as marble. Thus far he. But to purſue our former diſcourſe.

That alſo, which we intimated of the conduciveneſs of the various tumblings to and fro of the hard particles to their uniting into one firm concretion, ſeems confirmable by what we have obſerved in ſome ſaline liquors, eſpecially certain parcels of ſpirit of hartſhorn, which whatever were the conſtitution of the ambient air, remained fluid ſome of them for many months, after which the ſaline corpuſcles began to ſhoot at the bottom of the remaining liquor into exquiſitely-figured cryſtals, which at length grew copious enough. For this ſpontaneous coagulation of the little ſaline bodies happening ſo late, it ſeemed, that it was preceded by almoſt innumerable evolutions, which were ſo many and ſo various, that at length the little bodies came to obvert to each other thoſe parts of themſelves, by which they might be beſt faſtened together and conſtitute a firm body. Which conjecture ſeemed the leſs improbable, becauſe we could not well imagine, that this coagulation proceeded (as that of diſſolved alum and other ſalt is wont to do) from the evaporation of the ſuperfluous liquor; for the glaſſes, wherein what we have mentioned happened, being carefully ſtopped, there was no danger of ſuch an avolation; and if any thing could get away, it muſt have been the ſubtile, piercing and fugitive ſpirit, (which indeed, as my noſe had informed me, does oftentimes penetrate ordinary ſtopples, for the flying away of thoſe volatile parts would only have left the remaining liquor more aqueous. And it is well known to thoſe, that deal with ſuch kind of liquors, that the more aqueous they are, the leſs apt they are to cryſtallize. And however it will ſerve our turn, that there was but an inſenſible diminution of the liquor upon the receſs of whatever it was that got through the cork.

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* *Hydrograph. du P. G. Fournier*, Lib. II. Cap. 6.

To the ſame purpoſe I remember alſo, that having in a cryſtal phial carefully kept a pretty quantity of well-coloured tincture of amber, made with pure ſpirit of wine, it remained fluid for a year or two, and during that time preſented us with a ſtrange phænomenon, that belongs to other papers †. But having been abſent for two or three years from the place where we locked it up, we found, when we came again to look upon it, that though it had formerly remained fluid ſo long, yet ſeveral yellow lumps of amber, almoſt like beads, with one ſide flat, had here and there faſtened themſelves partly to the bottom, and partly to the ſides of the glaſs; the reſt of whoſe internal ſurface continues yet tranſparent.

ANOTHER thing, whereby bodies become ſtable, is the admiſſion of adventitious corpuſcles into their pores and receſſes. And of the ways, by which theſe foreign corpuſcles may bring the ſubſtance they invade to be compact, theſe four appear the chief.

FIRST then, the adventitious corpuſcles we ſpeak of may produce ſtability in the matter they pervade, by expelling thence thoſe voluble particles, which, whilſt they continued in it, did by their ſhape unfit for cohesion, or by their motion oppoſe the coalition, or diſturb the reſt of the other particles whereof the body conſiſted. But of this having already diſcourſed, proceed we to what is to follow.

IN the next place then, foreign bodies may contribute to the ſtability of a ſubſtance they get into, by hindering the motion of the little bodies, that conſtitute it.

AND thirdly, ſuch advenient bodies, eſpecially if they be not of the ſmalleſt ſize, may produce a firmneſs in the ſubſtance, which they get into, by conſtituting with the particles it conſiſts of corpuſcles more unapt for motion, and fitted for mutual coheſion.

THESE two we mention together, becauſe that very often nature employs them together, for the introducing of ſtability into matter.

To theſe ſeems to be reducible the way of turning the fluid body of milk into curds by the mixture of a little runnet; whoſe ſaline particles pervading the body of the milk, do not only make a commotion in the parts of it, but faſten the branched particles of it to one another, and with them conſtitute a body of another texture than was the milk; and the weight of theſe curdled bodies reducing them by degrees into a cloſer order, does, whilſt it preſſes them together, ſqueeze out the thinner and more ſerous liquor, which the runnet was unable to coagulate, and which being thus ſever'd from the groſſer parts of the milk, may well be more fluid than milk itſelf is wont to be. And that there is ſome coalition of the particles of the runnet with the coagulated ones of the milk, may appear by the complaints, that houſewives ſometimes make of their dairy-maids, that the cheeſes taſte too ſtrong of the runnet, when too great a proportion of it has been mingled with the milk. And though we aſcribed the curdling of the milk to the ſaline particles of the runnet, we ignore not, that, not only common runnet, but alſo divers juices of herbs will curdle milk, as is well known in thoſe parts of *Italy*, where cheeſe is made without runnet. But we made eſpecial mention of the ſaline corpuſcles of the runnet, becauſe really houſewives are wont to ſalt it, and becauſe ſaline liquors do manifeſtly and powerfully operate in the coagulation of milk, which may be curdled by juice of lemons, and I know not how many other acid ſalts. And to manifeſt yet further the coagulative power of them, we have ſometimes in about a minute of an hour arreſted the fluidity of new milk, and turned it into a curdled ſubſtance, only by dexterouſly mingling with it a few drops of good oil of vitriol. But of the effects of various ſalts upon milk we elſewhere may, and therefore ſhall not now, diſcourſe.

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† This phænomenon is partly deſcribed at large in one of the author's *Phyſico-Mechanical Experiments.*

BETWEEN this laſt recited experiment, and the two following ones, it will not be improper to inſert the immediately enſuing one, for the affinity, which it ſeems (in different reſpects) to have with both.

I REMEMBER then, that I divers years ago prepared a ſalt, which either was, or at leaſt anſwered well to the qualities aſcribed to that which is now called *Glauberus*'s *ſal mirabilis*, which ſeemed to have in it a coagulative power, in reference to common water. For whereas ſalt of tartar, common ſalt, nitre, *&c.* being diſſolved in water, do upon evaporation of a ſufficient quantity of that water, recover indeed their priſtine ſaline forms, yet they do but coagulate themſelves without concoagulating with them, either any water, or at leaſt ſo much, as chymiſts have thought worth the taking notice of. Whereas this ſalt we ſpeak of, being prepared for the purpoſe, and diſſolved in a convenient quantity of water, does upon its re-coagulation ſo diſpoſe of the aqueous particles, among its own ſaline ones, that if the experiment be well and carefully made, almoſt the whole mixture will ſhoot together into fine cryſtals, that ſeem to be of an uniform ſubſtance, and are conſiſtent enough to be even brittle, and to endure to be pulverized, ſifted, *&c.* though the concretion may have ſuch a proportion of water in it, that (as I remember) when the experiment ſucceeded well, from three parts of water, and but one of ſalt, I had about four parts of cryſtals.

I NEED not tell you, that this ſalt ſeems to have a ſomewhat more than ordinary reſemblance of a true coagulum, ſince it reduces ſo much water into a ſtable conſiſtence; yet it does in no contemptible proportion materially concur to the body produced. But I may hereafter (which I muſt not do now) entertain you about a ſalt of a differing kind from this; and which put me upon conſidering, whether there may not be a coagulum more properly ſo called of common water, which may in a very ſmall proportion operate upon a great quantity of that liquor, as runnet does on milk.

I HAVE not yet examined, whether it will be ſufficient to refer merely to the ſecond and third ways lately mentioned of making bodies become ſtable in the phænomena I am about to ſpeak of, or whether it may be reaſonably ſuppoſed (and added as a fifth way) that the bodies to be coagulated may (in great part) be brought to be ſo; by ſo acting upon the bodies to which they are put, that the agent liquor (if I may ſo ſpeak) does by its action communicate to the ſubject it works on, or loſe upon ſome other account ſome ſubtile parts whoſe abſence fits the diſpoſed remaining fluid for ſuch a coheſion, as may ſuffice to make a body be (though very ſoft, yet) conſiſtent. But however, it will not be amiſs to take ſome notice of effects, which, what e'er the cauſe be, belong to the *Hiſtory of Fluidity and Firmneſs.*

I SOME years ſince prepared a ſubſtance of a whitiſh colour, which would not only deſtroy the fluidity of ſome other liquors, but would give a conſiſtency to a notable proportion of oil of vitriol itſelf, though the parts of this liquor be preſumed, upon the ſcore of its corroſiveneſs, and its aptneſs to grow very hot with many other bodies, and make them ſmoke, to be very vehemently agitated.

AND I remember, that I ſometimes ſhewed the curious a glaſs phial well ſtopped, upon the bottom of which lay a little of this newly-mentioned whitiſh powder, over which there was a conſiderable proportion of oil of vitriol in a conſiſtent form, without ſeeming to have any thing to do with the powder, as indeed it had been only poured upon it, and ſuffered to ſtand in the cold for ſome time (which if I miſtake not was a day or two) at the end of which the above-mentioned change was wrought on the liquor by the powder, which did not appear to be diſſolved thereby. Which phænomenon ſeemed indeed to argue, that there happened in this experiment (that was not the only one of the kind I then made) ſomething like the coagulation formerly mentioned of quickſilver by the vapour of lead; ſome ſubtile parts of the coagulator, if I may ſo call it, inviſibly pervading the liquor, whoſe fluidity was to be ſuſpended, though it ſeem not improbable to me, that the effect produced might depend upon both cauſes, this newly expreſſed, and the other a little above mentioned; where I gueſſed, that a change of texture, and thereby of conſiſtence in the menſtruum, might be the reſult of the operation of the menſtruum, and the body it acts upon. And becauſe this powder is not ſo eaſy to be prepared, I ſhall add, that you may (though not ſo well as by the newly-mentioned way) ſee the coagulation of a menſtruum upon a firm body, which it does not ſeem to diſſolve, by the enſuing experiment: Take cryſtals of ſalt-petre very well dried, but not powdered; and gently pouring on it in a glaſs phial ſome good oil of vitriol till it ſwim about half an inch, or perhaps more, above the ſalt; leave the phial cloſed with a cover of paper in a cool quiet place, where it may not be ſhaken: and if the trial ſucceed with you as it did with me, the liquor will, though ſlowly, ſo ſettle itſelf about the nitre, that though you incline the phial to any ſide (or perhaps turn it upſide down) it will not run out: and I have ſometimes taken notice of little ſaline bodies, and as it were fibres, that ſeemed to keep the parts of the mixture united together. I made alſo ſome other trials to coagulate unflegmatick A. F. upon nitre and ſome other bodies, the phænomena of which trials did not oblige me to renounce the lately-mentioned conjectures about the cauſes of ſuch changes of conſiſtency in liquors, as I have been ſpeaking of: for I ſtill think it highly probable, that the beſt coagulator I have met with acts but as a finer ſort of runnet, which in an inconſiderable quantity really diſperſes material parts of itſelf through the liquor to be wrought on, though theſe, when the coagulator is a conſiſtent body, be, perchance, ſo few or ſubtile, as not to make any viſible diminution of the body it parts with.

A MORE eminent example to our preſent purpoſe may be afforded us ſometimes, (for I am ſure the experiment will not always ſucceed) by the notable way of coagulating quickſilver, and thereby turning it from a fluid into a firm body, by the vapour of melted lead, in which, when it is taken off the fire (but before it be quite grown hard again) a little cavity muſt be made with a pebble or a ſtick, that the quickſilver tied up in a rag may be nimbly put into that hole, and be congealed by the permeating ſteam of the cooling lead. Which effect may be leſs hopefully expected by the way wont to be preſcribed by authors (moſt of whom I doubt never made trial of it) than by another that I have practiſed, and may on another occaſion ſhew you. And that ſome metalline ſteam does really invade the quickſilver, ſeems probable by the waſting of lead by fuſion, and by the operations aſcribed by chymiſts to the fume of lead upon gold, about which I may elſewhere tell you what is come to my knowledge.

AND I remember, that not long ſince, an ingenious phyſician of my acquaintance keeping ſome lead long in fuſion to reduce it *per ſe* into a calx, and holding his head often over the melting pot to obſerve the alterations of the metal, was ſuddenly purged divers times both upwards and downwards, which both he and I aſcribed to the ſaturnine exhalations. And though I ſuſpected the congelation formerly mentioned might proceed from the egreſs of ſome ſubtile ſubſtance, that formerly agitated, but after deſerted, the mercurial corpuſcles; yet that the concretion of the quickſilver might be effected by ſome benumbing vapour of the lead, ſeems confirmable by a notable accident, that befel that famous geometrician Doctor *Wallis*, who related it to me as a phænomenon he knew not well what to make of; namely, that he and other learned men at *Oxford* being minded to make the experiment under conſideration, they found that, upon the firſt fuſion of the lead, the immerſed quickſilver was very well coagulated by it; but when they came to melt it the ſecond time, and put new quickſilver into it, the experiment would not ſucceed; at which they wondered, finding by trials, that the lead might be ſo eaſily deprived of its power of hardening quickſilver. That this obſervation will always hold true, I am not apt to believe; but that ſuch learned and candid naturaliſts ſhould either be miſtaken in making it, or miſrelate it, it were injurious to ſuſpect: wherefore ſuppoſing that to have then at leaſt happened, which one of them regiſtered in writing, and more than one of them told me; it ſeems to countenance what we have delivered, and looks as if, according to our doctrine, there were in lead a coagulative ſteam or ſpirit, and yet (at leaſt in that parcel of metal) in ſo ſmall a proportion, as that it almoſt totally diſlodges or ſpends itſelf upon the firſt opportunity it meets with of paſſing into quickſilver.

We have elſewhere, to another purpoſe, mentioned our having ſometimes (for, as we there advertiſe, it will not always ſucceed) made an experiment, which ſeems of kin to the former, and may give much light to the matter under conſideration, and it was this; we poured upon aqua fortis common ſallet oil, which floated together at the top of it, but after ſome hours had its texture ſo changed by the aſcending ſteams or other ſubtile inſinuating particles of the ſaline liquor, that it was turned into a white conſiſtent (and ſometimes a brittle) body like butter, remaining all in one cake on the top of the menſtruum. And the like experiment (but in a longer time) we have performed with expreſſed oil of ſweet almonds inſtead of common oil.

And to ſhew further, how much the operation of the ſame viſible agent may be diverſified as to the production of fluidity or firmneſs, according to the differing diſpoſitions of the bodies it acts on; I have ſometimes (I remember) taken the ſame aqua fortis, or ſpirit of nitre, wherewith I had coagulated expreſſed oil of olives, and having poured it off from the butter-like ſubſtance; I caſt into it ſome good camphire, which, without heat, was thereby reduced into an oil, that retained a diſtinct ſuperficies from the menſtruum, which it ſwam upon, and would not incorporate with; ſo that the ſame numerical menſtruum, without the help of any degree of fire, turned a brittle body into a liquor, and the liquor into a brittle body, (for ſuch is the ſubſtance, that may be made of common oil, if it be ſuffered to float long enough upon the coagulating liquor;) which brittle ſubſtance (to add that upon the bye) ſeemed to have received a more durable alteration from the ſteams of the menſtruum than was expected. For not only when melted with fire, it would upon refrigeration recover its conſiſtence without becoming again fluid, as when it was in the form of oil; but I made a trial or two, without ſucceſs, to reduce it to a liquor, by mixing it with oil of tartar *per deliquium*, which, you know, has a great faculty to find out and mortify acid ſpirits, ſuch as thoſe of the nitre or aqua fortis, that had (whether as mere acids I now examine not) coagulated our oil.

The fourth and laſt way, whereby corpuſcles entering from without into a body may give it a ſtable conſiſtence, is by making ſuch a commotion in the parts of it, as may make them apply themſelves one to another according to a greater ſurface, or otherwiſe complicate and diſpoſe them after the manner requiſite to make them ſtick together.

THIS way of making bodies become conſiſtent, is ſeldom or never employed by nature without the concurrence of ſome of the other ways already mentioned: but we have diſtinguiſhed it from the two laſt recited, becauſe in them we ſuppoſe, that ſome of the adventitious corpuſcles are ſtopped in the body, to whoſe firmneſs they conduce, and (though perhaps but in a very conſiderable proportion) do concur to make it up; whereas here we ſuppoſe, that without materially concurring to conſtitute the body they work upon, they do only agitate and variouſly move the particles it conſiſts of, perhaps breaking ſome, bending and twiſting others, and in a word ſo altering the texture, that the parts, that did formerly either move ſeparately, or adhere together but looſely, are now reduced to a cloſer order, or a more implicated texture, and thereby more firmly connected to one another. That the bare diſpoſition of the parts of a body in reference to each other, without any addition of foreign matter, may do much towards ſtability, we may ſee both in ſome examples formerly mentioned, and in oſier wands, which, when lying looſely in an heap together, may each of them very eaſily be diſſociated from the reſt; but when they are breeded into a baſket, they cohere ſo ſtrongly, that when you take up any one of them, you ſhall take up all the reſt. To which may be added thoſe many obvious, though perhaps unheeded, inſtances, wherein by the bare texture of the ſlender hair or filaments, whereof wool or ſilk conſiſts, cloth, ſilk-ſtockings, and many other durable garments are made by illiterate tradeſmen.

WE may alſo obſerve the force of bare motion in altering the texture, and thereby the conſiſtence of bodies by the common way of churning, for there the external impulſe of the churn makes a great commotion in the parts of the cream, and tumbles and ſhuffles them perpetually to and fro among themſelves; whereupon it happens, that the more branched corpuſcles meeting with one another, are intangled, and thereby ſeparated from the reſt, and after many occurſions all theſe parts are at length faſtened to one another, and excluding thoſe of the butter-milk, which ſeem not ſo conveniently ſhaped for mutual coheſion, do conſtitute butter: which is wont to be made yet more conſiſtent, or rather more compact, by being beaten or otherwiſe compreſſed, as the parts thereby reduced into a cloſer order ſqueeze out the fluid butter-milk, that was intercepted among them.

IT will perhaps be thought more ſtrange, that a fluid body, nay a diſtilled liquor, which is very volatile, and paſſes for ſimple and homogeneous, and is at leaſt far leſs compounded than milk, ſhould by motion, without the mixture of any new matter, be made coherent: and therefore I hope, that it will not only ſecond the example newly alleged, but likewiſe confirm ſome main points of our doctrine touching firmneſs, if we obſerve, that even the chymical oil of turpentine, which paſſes for one of the principles or elements of that body, may be in great part, if not wholly, coagulated without addition. And yet (not to anticipate what I may have occaſion to deliver elſewhere concerning this experiment) I ſhall now only relate, that inquiring a while ſince of a very expert chymiſt, whether he had not ſometimes obſerved (which I have often done, as I elſewhere declare) oil of turpentine to begin to coagulate, if it were often diſtilled; he went with me to his laboratory, and there let me ſee in a receiver ſome oil of turpentine, which he had often diſtilled over *per ſe*, in good part coagulated into a whitiſh and conſiſtent body: affirming alſo to me, that he had ſometimes, by frequent diſtillations, without addition, obtained from clear oil of turpentine a far greater proportion of ſuch a ſtable ſubſtance. Whoſe conſiſtence,

ſiſtence, whether it ſhould be aſcribed to the fire's breaking the oily corpuſcles into parts more fit for mutual coheſion, or whether it proceed from a new texture of the ſame corpuſcles, only chancing by thoſe various evolutions to be diſpoſed after ſuch another manner, as to complicate or otherwiſe connect them, I need not now ſpend time to inquire; ſince it is enough for my preſent purpoſe, that in this example we have one, that declares, how much even motion, without the addition of any ſenſible ſubſtance, may in ſome caſes conduce to firmneſs.

AND here to illuſtrate our doctrine about this quality and fluidity, by ſhewing what the inteſtine motion of the parts, even without the aſſiſtance of adventitious heat, may do, to make a body change its conſiſtence, according to the previous diſpoſition of the matter, and become of firm, fluid; as we lately ſaw oil of turpentine made of fluid, firm: I will add on this occaſion, what I obſerved of oil of wax diſtilled in a retort with an additament of the like nature with that I formerly mentioned, when I ſpoke of the fluid oil drawn from oil of olives. For this oil of wax, though at firſt it came over for the moſt part, if not totally, in the form of a butter; yet by ſtanding on a ſhelf (and that not in a hot place, as a ſtove or laboratory) I obſerved it little by little to reſolve into a tranſparent oil; and purpoſely inquiring of him, that looked after it, whether or no this effect might not be aſcribed to the increaſed warmth of the weather, he aſſured me of the contrary, having taken notice what effects the changes of weather had upon it.

BUT what if we ſhould ſay, that fluidneſs and ſtability depends ſo much upon the texture of the parts, that by the change of that texture the ſame parts may be made to conſtitute either a fluid or a dry body, and that permanently too? Theſe laſt words I add, becauſe of what may be ſaid to this purpoſe concerning the change of water into ice, and ice into water, and of metals into fluid or hard bodies, by fuſion and refrigeration: for in theſe examples the acquired hardneſs of water and fluidity of metals may be preſently loſt upon the bare removal of thoſe bodies into a temperate air; whereas in the inſtance we are to give, the acquired texture is ſo durable, that without an extreme external violence, ſuch as would deſtroy moſt other ſtable bodies, it is not to be deſtroyed. And this inſtance is afforded us by that admirable repoſitory of nature's wonders, quickſilver: for if ſome ounces of this fluid mineral be put into a convenient glaſs veſſel, and that veſſel be firſt exactly ſtopped, and kept for ſix, eight, or ten weeks (or longer, if need be) in a ſand-furnace, whoſe heat may be ſtrong and conſtant, the corpuſcles, that conſtitute the quickſilver, will, after innumerable revolutions, and perhaps bendings, twiſtings, and other changes, be ſo connected to one another, that inſtead of a fluid body, they will appear in the form of a red powder, that chymiſts precipitate *per ſe:* which change is ſo unexampled, that though among the more curious Spagyriſts it be very well known, yet many Naturaliſts cannot eaſily be brought to believe it; whom to convince of the poſſibility of it by a much leſs tedious preparation, I take half a pound or a pound of quickſilver, and with a ſtrong fire diſtil it out of a glaſs retort, and for the moſt part there will remain in the bottom and about the ſides of the veſſel a little red powder, which ſeems to be nothing but part of the fluid body (moſt expoſed to the action of the fire) turned into a dry one in eight or ten hours ſpace.

AFTER what manner the fire produces ſo odd a change in the quickſilver, I do not preſume to know. It is true, that though the parts of liquors do, as we have formerly taught, touch one another but in part of their ſuperficies, yet they all of them ſeem to have ſome degree of viſcoſity, or ſome ſlight and looſe complication or other kind of adheſion of parts, as appears by their being ſo eaſily contexed into thoſe thin membranes or films we call bubbles, inſomuch that not only ſpirit of wine, that ſeems the moſt light, and moſt fluid of liquors, will afford bubbles, but (what may ſeem ſtrange) we have divers times purpoſely obſerved, that quickſilver itſelf, as ponderous as it is, eſpecially being ſuffered to fall in a ſlender ſtream into a veſſel almoſt full of the ſame mineral, will afford bubbles numerous and large enough, although (as thoſe alſo of the ſpirit of wine) quickly vaniſhing.

And hence it might be imagined, that in the operation we are treating of, ſome ſuch change is made in the quickſilver, as we formerly obſerved to be made in the white of an egg, when by a new diſpoſition of its parts, either heat or beating it makes it a kind of ſtable body, or elſe it might be pretended, that there is a variety of parts argued to be in quickſilver by the great variety of its effects upon other bodies, and that by the frequent evolutions, which the fire makes of thoſe parts among themſelves, they come at length to be ſo applied to one another, that either they lock into each other, as it were, or ſlip upon one another's ſurface in ſuch a manner, as that as much of their ſurfaces immediately touch one another, as is requiſite to make them cohere, as we formerly mentioned of ſeveral very ſmooth pieces of glaſs mutually adhering without any other cement, than the congruity and immediate contact of their ſurfaces. But though theſe conjectures and divers others might be propoſed, yet I fear all of them would prove but meer conjectures. Nor were we much aſſiſted to make better by looking upon our mercurial precipitate in one of the beſt magnifying glaſſes in the world; for what we there diſcovered was only, that the red powder had in it many corpuſcles of ſundry other colours, and that the little grains of powder ſeemed to be of no determinate ſhape, but looked like ſlender fragments of red coral. And having put ſome ſmall duſt of a ſhining precipitate of gold and mercury into the ſame augmenting glaſs, all we could diſcern was, that the little grains of this precipitate differed from thoſe of that made of mercury alone, in that theſe (which a chymiſt would take notice of) were ſo tranſparent throughout, that one would verily think he beheld the beſt ſort of thoſe precious ſtones goldſmiths call granats. But though we pretend not to make out, how the new texture is produced in the quickſilver, yet to make it ſtill more evident, that its change of conſiſtence proceeds from its change of texture, we will add, that having a great curioſity to try whether our powder could not be made fluid again, I procured ſome precipitate *per ſe* of a perſon, who formerly lived with me, and was expert in many mercurial operations, and preſented me ſome of his own making: this being weighed, and put into a convenient glaſs, was carefully preſſed with a naked fire (which ſhould be ſtronger than that wherewith it was precipitated) and at length it roſe by degrees in fumes, which ſettled in the neck of the glaſs in many drops of revived running mercury; all which being collected into one, we found, that there wanted but about a ſixth or ſeventh part of what we had put in, and we ſuppoſed we ſhould not have wanted that neither, but that the vehemence of the fire had melted the glaſs, which ſwallowed up a part of the powder, that made a great ſhew through it, after what was colliquated had been removed from the fire.

This experiment brings into my mind another, that was judged uncommon enough, and it was this: Being not long ſince maſter of about half an ounce of a certain mercury, which ſome ways of examining it, that I had employed, induced me to think mercury of Saturn; I imagined (for ſome reaſons) that it might be made very ſerviceable to confirm our doctrine touching fluidity and firmneſs. And accordingly I found upon trial, that I could, barely by ſhaking it long, reduce it to a black powder: in which form it would continue as long as I pleaſe to let it do ſo.

And when to the byſtanders there appeared nothing in it, that gave ſuſpicion of a fluid body, I could in a trice, only by dexterouſly rubbing it in a ſmall marble mortar, reduce it little by little into running mercury, as it had been before. Which quick paſſage, from one quality to another, being made, not only without the help of fire, but without adding or taking away any viſible ſubſtance, proved no ignoble inſtance, how much motion and reſt, and the thence eaſily reſulting texture of the component corpuſcles of a portion of matter, may contribute to its fluidity or firmneſs.

FROM the experiment of precipitating quickſilver *per ſe*, and from ſome other things, partly delivered already, and partly to be delivered by and by, we may learn what to think of the opinion of ſome eminent modern philoſophers, who teach, that a fluid body is always diviſible into bodies equally fluid, as quantity into quantities, as if the particles of fluid bodies muſt alſo be fluid themſelves: for by them it ſeems to appear, that quickſilver, and ſome other actually fluid bodies conſiſt very much of hard corpuſcles, ſince by the change of their texture they may be deprived of their fluidity and become ſtable. We ſee alſo, that the ſtiff and ſolid particles of ſalts diſſolved in common water, and of ſilver diſſolved in aqua fortis, being by thoſe liquors ſufficiently diſſociated and ſeparately agitated, do with them conſtitute fluid bodies. And we have elſewhere mentioned to another purpoſe an experiment, which may not impertinently be repeated here; namely, that by putting together into a glaſs retort one part of quickſilver and four of common oil of vitriol, and diſtilling them in a ſand furnace with a ſtrong fire, there remained in the bottom of the veſſel a ponderous calx or powder, ſo far from being fluid, that it was but in part diſſoluble in water: and that, which ſeems to prove, that in the very liquid oil of vitriol, though a diſtilled liquor, the ſaline corpuſcles, that chiefly compoſe it, do retain their ſtiffneſs (generally to be found in undiſtilled ſalts) is, that by ſteeping our calx in fair water, we could ſeparate from it a conſiderable quantity of particles, which upon the evaporation of the water coagulated into ſtore of ſaline and brittle bodies. And that theſe proceeded rather from the menſtruum than the metal, we were induced to think, by obſerving the dry calx, before any water was poured on it: for though the ſaline part of the mixture did not weigh (perhaps any thing near) ſo much as the mercurial diſtinctly did, yet the aggregate or mixture did weigh a great deal more than the quickſilver did when it was put in; and the oil of vitriol, that was abſtracted, a great deal leſs, than it did before it was committed to diſtillation. Nay, I once or twice obſerved in a glaſs, where I kept a quantity of oil of vitriol, that there did ſpontaneouſly faſten themſelves to the ſides little ſaline cryſtals, which, when I took out, I found hard and brittle; but when I had, for trial-ſake, expoſed them to the air, they preſently reſumed a fluid form, and appeared to be oil of vitriol. In the obſervation alſo lately mentioned concerning the ſpontaneous coagulation of ſpirit of hartſhorn, it ſeems evident, that bodies, which are all or moſt of them hard, and appear ſo when they are commodiouſly connected to each other, may yet conſtitute a fluid body when they are reduced to ſufficient ſmallneſs, and put into a convenient motion. And indeed, if the leaſt particles of fluid bodies were not (many of them at leaſt) indowed with their determinate bigneſs and ſhapes, but that ſuch fluid bodies could be always divided into particles fluid alſo, how comes it to paſs, that ſome liquors cannot pierce into or moiſten ſome bodies, which are eaſily pervious to other liquors? For if the particles of the excluded liquor were of neceſſity always diviſible into fluid ones, there ſeems no reaſon, why they ſhould not be ſub-divided into ſo very ſmall ones, as that no pores can be ſuppoſed little or oddly figured enough to keep them out.

IT is true indeed, that as it is hard to demonſtrate, ſo it is not eaſy to diſprove, that the matter, whereof fluid bodies conſiſts, is capable of being indefinitely divided: and it may be granted too, that by how much the ſmaller parts a body is divided into, by ſo much the more eaſily, *cæteris paribus*, are the parts of that body to be put into motion. But this diviſibility of a fluid body into perpetually leſſer and leſſer parts, belongs not to it properly as it is fluid, but as it is a body; ſuch diviſibility, if ſuppoſed true, being a primary affection of matter itſelf, and belonging as well to thoſe portions of it that are hard, as to thoſe that are fluid. And though it were admitted, that ſuch an endleſs diviſion, as is preſumed might be made mentally, (as they ſpeak in the ſchools) that is, by the thought or operation of the mind, yet it would remain a great queſtion, whether or no nature does actually ſo far mince and ſub-divide bodies; as may appear by what has been freſhly noted. And however, it is not only requiſite to the conſtitution of a fluid body, that the parts of it be ſmall enough, but that they be alſo actually moved. For we obſerved, not long ſince, that the duſt of alabaſter put into motion did (though its corpuſcles were not inſenſible) emulate a fluid body, and immediately ceaſed to be fluid, when they ceaſed to be agitated: whereas the particles of water, as minute and apt as they are to conſtitute a fluid ſubſtance, do yet make that hard and brittle body we call ice, when thoſe little particles, upon what account ſoever, are reduced to be at reſt.

BY what has been hitherto diſcourſed, we may alſo be aſſiſted to judge of the doctrine of the chymiſts, who teach, that in all bodies, coagulation, ſtability, hardneſs, and brittleneſs depend upon ſalt: for though what above has been ſaid of curdling of milk by ſaline liquors, and the hardneſs and brittleneſs obvious in ſalts themſelves, may keep us from denying, that the ſaline principle is very powerful in the coagulation of ſome bodies, and does produce much firmneſs or even brittleneſs in many or moſt of the concretes wherein it is predominant; yet this hardening power of ſalt ſeems not to proceed from any peculiar and inexplicable property it has to coagulate other bodies, or make them compact, but from the ſhape and motion of its corpuſcles, which it ſeems are more fitted by nature, than thoſe of many other concretes, to inſinuate themſelves into the pores of other bodies, and faſten their particles to themſelves, and to one another, either by wedging their corpuſcles together, or by their ſtiff and ſlender parts, or their ſharp angles, or edges piercing divers of them together; as when many pieces of paper are kept from ſcattering by a wire, that runs through them, or as when a knife takes up at once divers pieces of bread and meat, by being ſtuck into them all. But whenſoever there is in the conſtituent parts of the body a ſufficient fitneſs and diſpoſition to adhere firmly to one another, nature may of thoſe parts compoſe a ſtable body, whether they abound in ſalt or no; it not being ſo much upon chymical principles, or even upon the predominancy or plenty of any one ingredient, as upon the ſhape and motion of the component parts of bodies, that their fluidity and firmneſs depend. I will not here urge, that ſalts are generally reducible by an eaſy mixture with water into the form of liquors; nor that ſea-ſalt, ſalt of tartar, and divers other ſorts of ſalts, will of themſelves, even in the air, if not very dry, aſſume the form of fluid bodies; nor yet will I preſs the ſhortly to be mentioned example of coral, which is confidently affirmed to be ſoft, whilſt it remains in the ſalt water, and to grow hard when taken out of it: I will not here, I ſay, preſs theſe and the like arguments, but content myſelf to have hinted them, becauſe they are ſuch, as I cannot well in few words make out and vindicate. Wherefore I ſhall rather demand, what ſalt can be made appear to paſs out of the body of melted lead into that of quickſilver, to perform in it the coagulation above mentioned? What acceſſion of ſalt is there to be obſerved, when running mercury is precipitated *per ſe* into a powder? And how will it be proved, that when in a well-ſtopped glaſs the whole body of water is, in froſty nights, turned into firm ice by the cold of the ambient air, that coagulation is performed by ſalt, it having not yet been made appear by chymiſts, that either ſalts or even the diſtilled ſpirits of them can penetrate, without a kind of prodigy, the narrow pores of unheated glaſs? It is uſually obſerved in eggs, that though at their firſt coming out of the hen's belly, the ſhells are ſoft, yet ſoon after they grow hard and brittle; and yet it appears not, how the ſaline ingredient is increaſed to effect this ſpeedy induration: and (to ſubjoin that by the by) albeit I am not averſe from thinking, that the coldneſs of the outward air, and its imbibing ſome of the looſeſt of the moiſt parts of the ſoft-egg-ſhell, may concur to this effect; yet there are many obſervations of egg-ſhells, that have been found hard in the womb of the hen. And I well remember I have taken notice, that divers eggs not yet laid, but found at one time in the body of the ſame hen, were each of them furniſhed with a compleat and brittle ſhell. But I think I can draw a much ſtronger argument againſt the chymical opinion from the conſideration of an egg; for I demand, what plenty of ſalt can be made appear to pierce the hard ſhell, and more cloſe-wrought membrane, that both lines it and involves the egg, eſpecially ſince it is certain, that in *Egypt*, and divers other places, eggs may be hatched by a temperate external heat without the hen. And yet we may here obſerve, that the ſame internal ſubſtance of the egg, which at firſt was fluid, the yolk and white, that compoſed it, being ſo, is upon the excluſion of the chick turned almoſt all of it into conſiſtent bodies, ſome of them tough, as the membranes and griſtles of the bird, and ſome of them harder and almoſt brittle, as his bones and beak; and all this, as we ſaid, without acceſſion of new ſalt. At would be hard for chymiſts to prove, that diamonds and rubies, which are counted the hardeſt bodies we know (and at particular trials of whoſe hardneſs I have ſometimes wondered) do abound in ſalt; at leaſt it will not be unreaſonable for us to think ſo, till chymiſts have taught us intelligible and practicable ways of ſeparating (at leaſt ſome) true ſalt from either of thoſe jewels. And it may be alſo doubted, whether the blood of animals, when it is freeſt from ſerum, do not (though a liquor) as much abound with ſalt as their ſkins or their fleſh.

AND ſince it is with chymiſts that I am now reaſoning, I preſume I may be allowed to preſs them with arguments drawn from ſome of the eminenteſt writers of their ſect. For the generality of chymiſts, and even thoſe, that are by the reſt, and themſelves too, called philoſophers, not only granting, but aſſerting and maintaining the tranſmutation of great quantities of quickſilver and other ignobler metals into ſilver or gold by means of the white or red elixir, I ſhall demand of them, whence it happens, that one grain of the powder of projection can turn a whole pound of mercury into true gold or ſilver, and conſequently change a very fluid body into a very firm one; though the proportion of ſalt employed to coagulate the whole maſs of quickſilver would not amount to the ſix thouſandth or ſeven thouſandth part of the liquor; though we ſhould grant, that the powder employed to work this marvellous change were all of it ſalt, to which yet chymical writers aſcribe much more of the ſulphurous nature. And to this I ſhall add, what the famous and acute *Helmont* does to another purpoſe relate upon the experience of *Raymund Lully* and his own, concerning his

Helmont de Febr. cap. 14.

his prodigious liquor, alkaheſt; namely, that being abſtracted from common quickſilver, it does in a quarter of an hour coagulate it: and yet in this coagulation he points at this as a ſingular phænomenon, that this liquor, which is as well immortal as exceeding ſaline, leaves nothing of itſelf with the mercury, on which it works, and yet ſo coagulates it, that he preſcribes the making it into a ſubtile powder.

I REMEMBER alſo to our preſent purpoſe, that a phyſician of much veracity in what he relates, diſcourſing with me the other day about an odd preparation, that he ſaw at the preſent Duke of *Holſtein*'s (that learned prince and great chymiſt) aſſured me, that among other things he there took notice of a glaſs of ſpirit of urine, which in warm weather remained in the form of a liquor, but in cold weather did totally coagulate into cryſtalline ſalt. And being aſked by me, if he knew how this urinous body had been prepared? he anſwered me, that the Duke cauſed ſpirit of urine exceeding rich in volatile ſalt to be diſtilled very many times; after every diſtillation re-conjoining all, that came over in a liquid with that, which remained in a ſaline form, till by very frequent cohobations all the parts of the urinous ſubſtance were brought to the union or coalition above-mentioned. What we may propoſe concerning the various conſiſtence of the ſaline part of urine upon our own knowledge, we ſhall for certain reaſons reſerve for another place.

Hiſt. Moral. Cap. 5.

Hiſto. Nat. & Med. Braſil. l. 4. c. 1.

AND on this occaſion we will annex a few particulars, which may tend not only to the making of the chymical hypotheſis about the coagulation of bodies doubtful, but to the confirmation of much of the doctrine by us propoſed. The firſt ſhall be an obſervation afforded us by the art of making ſugar, wherein very great care is taken, that nothing acid (and eſpecially juice of lemons) fall into the chaldrons or other veſſels, wherein the juice of the ſugar-cane is to coagulate into ſugar: for though acidity be generally by the chymiſts aſcribed to ſalt, yet here the ſaline bodies are ſo far from promoting the coagulation of the ſaccharine ſyrup, that they would quite hinder it. And becauſe that through the want of ſugar-canes in theſe parts, we are reduced to take this obſervation upon the credit of others, and becauſe alſo in itſelf it ſeem ſomewhat ſtrange, we will vouch for it two eminent authors, in whoſe writings we met with it. The one is the ingenious French publiſher of the natural and moral hiſtory of thoſe American iſlands, commonly called by the French *les Iſles Antilles*, called by us the Caribbee iſlands; who deſcribing particularly how his country-men make ſugar in thoſe parts, gives this caution towards the latter end, *Sur tout*, &c. that is, above all, great heed muſt be taken, to let no juice of citrons (or lemons) fall into the caldrons, for that would abſolutely hinder the formation of the ſugar. The other is the diligent *Gulielmus Piſo*, who having given us a particular account, both by words and pictures, of the way of making ſugar, tells us that, *ſi momentum ſucci limonis vel acidi quid injiciatur, ſacchari conſiſtentiam nunquam acquiret, ſed in totum perditur.* To which I ſhall add, that having purpoſely inquired concerning this obſervation, it has been confirmed unto me by perſons, that pretend more than ordinary knowledge of the art of ordering ſugar: which likewiſe affords us another obſervation not impertinent to the theme we treat of; for the beſt authors, that write hereof, inform us, that the juice ſqueezed out of the ſugar canes is wont firſt to be boiled and depurated in vaſt veſſels of copper or braſs, whence it afterwards is conveyed to be further purified and coagulated into ſmaller ones; and that whilſt it is in the former, they uſe to pour upon it ſome very ſtrong lee to facilitate the ſeparation of its feculencies, as in the ſmaller ones it is uſual to pour a little oil or butter upon the boiling juice, to keep the ſyrup from boiling over. Now that, which they further obſerve to our purpoſe, is related almoſt after the ſame manner both by our

French author and by *Piſo*, and by the latter of them in theſe words: *obſervatu dignum* (ſays he) *ſi oleum majoribus inderetur abenis in quibus liquor primus, caldo dictus, purificatur, ſaccharo conficiendo plane foret ineptus: viciſſim ſi minoribus lixivium ſicut majoribus infundatur, æque impoſſibile ſaccharum conficere.* So much the fluidity and firmneſs of bodies depend upon their texture, how much ſoever chymiſts would have them depend upon ſalt.

Gulielm. Piſo, & Author Gallic. ubi ſupra.

But to this borrowed obſervation, though borrowed of authors not to be diſtruſted, we will add two or three experiments of our own, which we hope may the more confirm the doctrine by us propoſed touching ſtability in bodies, becauſe it was our aim in them to bring light by them to the matters we treat of.

First then, we prepared a liquor, elſewhere to be deſcribed, which is almoſt, if not altogether, as ſaline as *aqua fortis* itſelf, or any other acid ſpirit, that is commonly known: and yet when in this liquor we laid fragments of ſolid harts-horn of ſeveral ſizes to ſteep, even in a cold place, the menſtruum was ſo far from hardening them, that it would (without diſſolving them as corroſive liquors do metals) gently pierce into them and ſoften them; ſo that in about two or three days it would reduce them to a kind of white ſlime, or mucilaginous ſubſtance, at the bottom of the liquor. We took alſo good ſalt of tartar, and on it poured good ſpirit of vinegar, as long as the affuſion of it would produce any ebullition: then we diſtilled off the liquor, which came over almoſt inſipid, the ſaline parts, that make ſpirit of vinegar ſo ſharp, being retained by the ſalt of tartar. Upon the remaining dry mixture we poured freſh ſpirit of vinegar, as long as any hiſſing enſued thereupon, and afterwards abſtracted the aqueous parts of this parcel of liquor alſo; and ſo we proceeded, till having ſufficiently impregnated the fixed ſalt with the ſaline parts of the diſtilled vinegar, we obtained, according to our deſire, a mixture, which (though it were all made up of ſalts, and ſuch ſalts too as being made by the chymical analyſis of the bodies whence they were drawn, may according to the chymical doctrine be looked upon as pure and elementary) was yet ſo near fluidity, that it required not the heat of the fire to turn it preſently into a liquor, which ſhape it aſſumed with a gentler warmth, than one would expect from a ſaline body. Laſtly, we took common oil of vitriol, and caſt into it divers little pieces of camphire, which floating upon it were by degrees and after ſome hours wholly reduced into a reddiſh oil, that was to be ſeen altogether upon the top of the other liquor. Then having formerly tried that oil of vitriol would eaſily mix with common oil, we tried alſo, by ſhaking the ſaline and camphorate liquors together, to unite them, and eaſily confounded them into one high-coloured liquor, which ſeemed very uniform, and continued ſo (at leaſt as to ſenſe) for many hours. Then we added to this mixture three or four times as much fair water, and (as we expected) the camphire immediately recovered a white conſiſtent body, and by degrees ſettled at the top of the liquor: where we may obſerve, that the camphire is not made hard but fluid by its mixture with the ſaline corpuſcles of oil of vitriol, and exchanges its fluidity for firmneſs upon the affuſion of ſaltleſs water. And thus much it may ſuffice to have ſaid touching the chymiſts deriving the ſtability of bodies from their abounding in ſalt.

And as for the hardneſs and brittleneſs they aſcribe to the ſame principle, how much they may be increaſed or diminiſhed in a body, without the acceſſion or decrement of the ſaline principle or ingredient, may appear by that experiment mentioned by us to ſeveral purpoſes, of tempering a ſlender piece of ſteel: for when it has been ſufficiently heated, by plunging it red-hot into fair water, which is more likely to diſſolve than increaſe its ſalt, you may make it not very hard alone, but very brittle;

whereas by only ſuffering it to cool leiſurely in the air, it will be both much leſs hard and more tough. And if after having quenched it in cold water, you again heat it till it have attained a deep blue, it will become (comparatively) ſoft and very flexible, and that not from any waſting of the ſaline ingredient by the fire; for if this ſoftened ſteel be again heated red hot, and ſuddenly refrigerated, whether in water or otherwiſe, as before, it will re-acquire both hardneſs and brittleneſs.

Now that by theſe operations a real change is made in the diſpoſition of the ſmall parts of the ſteel, we have elſewhere evinced even by a ſenſible proof. And that by procuring a cloſer order and more immediate contact of the parts of a body, a man may without increaſing the ſalt increaſe the hardneſs of it, is, as we formerly alſo noted, obvious in ſnow, which whilſt it lies in flakes as it falls upon the ground, compoſes but a ſoft and yielding body; but when the ſame ſnow is, by being ſtrongly preſſed every way betwixt the hands, formed into balls, the little weather icicles or frozen bubbles it conſiſts of, are ſo approached to one another, and forced into an order, which allows ſo little waſte of room, that the formerly-intercepted ſpaces being moſt of them filled up with little bodies, the icicles can no longer yield, as they did before, to the preſſure of a man's fingers, but conſtitute a maſs conſiderably hard, which yet may be made harder being melted into water, and afterwards frozen into ice: for this having been a fluid body (and in ſuch, room is wont to be better huſbanded than in others) the bubbles intercepted in it cannot keep it from being of ſo cloſe a texture, as to be conſiderably hard.

I KNOW, that not only profeſt chymiſts, but other perſons, who are deſervedly ranked amongſt the modern philoſophers, do with much confidence entirely aſcribe the induration, and eſpecially the lapideſcence of bodies, to a certain ſecret internal principle, by ſome of them called a form, and by others a petrifying ſpirit, lurking for the moſt part in ſome liquid vehicle. And for my part, having had the opportunity to be in a place, where I could in a dry mould and a very elevated piece of ground cauſe to be digged out ſeveral cryſtalline bodies, whoſe ſmooth ſides and angles were as exquiſitely figured, as if they had been wrought by a ſkilful artiſt at cutting of precious ſtones, and having alſo had the opportunity to conſider divers other exactly or regularly ſhaped ſtones and other minerals, ſome digged out of the earth by my friends, and ſome yet growing upon ſtones newly torn from the rock; I am very forward to grant, that (as I elſewhere intimate) it is a plaſtic principle implanted by the moſt wiſe creator in certain parcels of matter, that does produce in ſuch concretions as well the hard conſiſtence as the determinate figure. We deny not then, that theſe effects depend moſt commonly upon an internal principle; but the difficulty conſiſts in conceiving, how that internal principle produces its effects, which theſe writers not pretending to explicate intelligibly, we thought it not amiſs briefly to ſurvey ſome of the principal ways, by which it ſeems, that nature makes bodies firm and ſtable, whereby we may be aſſiſted to judge, whether it be as neceſſary to have recourſe to a plaſtic principle or a Gorgonic ſpirit in all other quick and notable indurations of bodies in the cold, as in the hardening ſuch bodies, whoſe curious and determinate either internal textures or outward ſhapes (common to ſeveral concretions of one kind) argue their having been framed by ſome one formative power, or by divers ſeminal principles convened together. But this we will do without affirming, either that ſhe cannot by ſome other yet unobſerved way make conſiſtent bodies, or that of the ways by us diſcourſed of, ſhe is wont ſo to confine herſelf to any one, that ſhe does not frequently make uſe of two or more of them, to produce the ſame effect.

AND becauſe hardneſs is a high degree of firmneſs, I ſuppoſe it will not be impertinent to ſhew by ſome examples, how ſmall an external operation may, without any appearing adventitious ſalt, make a ſoft body hard, and even brittle, when there appears not any other change to be made, than that of the texture or diſpoſition of its component particles.

IT is a tradition amongſt naturaliſts, that coral grows ſoft at the bottom of the ſea, but when it is brought up into the open air, though it retains its bulk and figure, it hardens into a ſtony concretion, according to that of *Ovid:*

> *Sic & coralium, quo primum contigit auras*
> *Tempore dureſcit, mollis fuit herba ſub undis.*

Ovid. 15. *Metamor.*

WHETHER or no this tradition is ſtrictly true, we had not opportunity, when we ſtaid at *Marſeilles* (whoſe neighbouring ſea is the chiefeſt in *Europe* where coral is wont to be fiſhed) to give ourſelves an ocular ſatisfaction. But whatever ſome ſay to diſcredit the tradition, nay, how confidently ſoever *Beguinus* (who ſeems to have the moſt ſtrongly argued againſt it) hath rejected it, it muſt not be denied to be, ſometimes at leaſt, true (and that is enough to ſerve our preſent turn). For the learned *Gaſſendus* in the life of *Piereſkius*, relating how that incomparable gentleman had the curioſity to fiſh for coral near *Toulon* (a noble port not far from *Marſeilles*) has among other things this paſſage: *(viz.)* The plants, which were plucked up and down out were neither red nor handſome till their bark was pulled off; in ſome parts they were ſoft; and would give way to the hand, as towards the tops, which being broken and ſqueezed, they ſent forth milk, like that of figs. I remember likewiſe, that the learned Jeſuit *Fournier*, who being alſo a French hydrographer, and one that writes of *Marſeilles* and *Toulon* as places very well known unto him, may be ſafely credited on this occaſion; after he has particularly deſcribed the way of fiſhing corals near *Toulon*, he adds, theſe plants are neither red nor poliſhed, when they are drawn out of the water, till their rind have been taken off; nay, they are ſoft, and being broken or elſe ſqueezed betwixt the fingers, they throw out a kind of milk reſembling that of figs; and when one leaves off preſſing them, he may ſee the ſmall holes or pores, that harboured the milk, that was ſqueezed out. Thus far he. The credibleneſs of a good part of theſe narratives has been confirmed to me by a practiſer of phyſic in the *Eaſt-Indies*, who having made ſome ſtay at his return on the iſland of *Mehila* (near that of *Madagaſcar)* where ſtore of white coral is reported to grow, I enquired of him, whether he had gathered any, and whether he found it ſoft whilſt it was growing? and he anſwered me, that he had of late years divers times gathered coral upon the ſands of that iſland; and found it, when he gathered it, exceeding white, and (to uſe his expreſſion) as ſoft as an onion: adding, that though it would in a very ſhort time grow hard in the air (which he aſcribed, how juſtly I know not, to the external heat of the ſun) yet it is very well known to the ſea-men, that deal in that ware, that if it be not gathered at a ſeaſonable time of the year, it will not keep long, but either crumble away or otherwiſe decay: which diſagrees not with the experienced *Piſo*, who in his natural hiſtory of *Braſil*, ſpeaking of ſome places of the *Braſilian* coaſt, where divers ſtony plants, ſome like little trees, ſome otherwiſe framed, may be ſeen in clear weather growing in the bottom of the ſea, tells us, that, *è fundo erutæ mox duriſſimæ, ſi inſolentur in littore, ſiccæ niveique coloris fiunt.* As remarkable a change is that I meet with in *Scaliger*, who tells us, as upon his own knowledge, of ſome, who at the urinary paſſages voided a ſlimy matter, which in the air coagulated into a

Beguin. Tyrocin. Chym. Lib. 2. Cap. 10.

Lib. 4. *Anno Domini* 1624.

Hydrogra. de P. Fournier. Lib. 4. Cap. 27.

Lib. 4. Cap. 68.

firm ſubſtance. The ſtory being memorable, take it in his own words thus: *Ex bovillis oppidanus noſtris adjutus medicamentis eminxit vitrum ſane ex illa nobili Paxagoræ pituita, dum mingeretur albuminis mollitie emiſſum vitri duritie ac ſplendore, ſenatoris filius ejecit, puttis modo multos, & maximos: qui aëris contactu poſtea in gypſeam tum ſpeciem tum firmitatem concrevere; hic quoque nunc rectè valet.* Having likewiſe had the acquaintance of an inquiſitive merchant of *Dantzick*, and alſo of an ingenious chymiſt, that ſpent ſome time in that city and the neighbouring country, along whoſe coaſt our European amber is wont to be dragged out of the ſea; I inquired of them, whether they had never obſerved in amber a property like that which is reported of coral: and one of them, as I remember the other alſo, hath aſſured me upon his own particular obſervation, that lumps of amber are ſometimes taken ſoft out of the ſea, and grow hard in the air; which is the more credible to me, becauſe I have at a *Polonian* nobleman's ſeen (beſides other intercepted things) a whole ſpider, and that none of the leaſt, perfectly incloſed in a piece of hard and tranſparent yellow amber. And elſewhere I have ſeen ten or twelve (if I miſremember not the number) pieces of ſuch amber, which contained, one a fly, another a ſpider, a third a ſtraw, and each of the reſt ſome ſuch other thing. And it ſeems not impoſſible, that the contact of the external air may put the parts of ſuch ſmall bodies into a new motion, whereby ſome voluble corpuſcles, that hinder their reciprocal adheſion, may be excluded, and the particles themſelves preſſed or otherwiſe diſpoſed into a cloſer order. And we find, that ſome oil-colours, after they are brought to their due temper, may be preſerved very long in the ſame degree of ſoftneſs, if they and the ſhells, that contain them, be kept all the while under water, whereas in the air they would quickly change their texture, and become dry and hard.

But though in this laſt mentioned example, and ſome others, the removal of the body out of the water into the air ſeem manifeſtly to contribute to its growing hard, yet it ſeems not to us ſo eaſy to determine, what ſhare the air has in effecting ſuch indurations: for *Gaſſendus* relates of *Piereſkius*, that he being wont in the ſummer time to waſh himſelf in one of the leſſer ſtreams of the river of *Rhone*, he there made the following obſervation. Once upon a time he felt the ground, which he had wont to find even and ſoft, to be grown hard with little round balls or bunches, like hard boiled eggs, when the ſhell is peeled off; at which wondering, he took ſome of them up, and carried them home, that he might ſhew them to his maſter, and demand of him the reaſon. But the miracle was increaſed, when a few days after returning to the river, he found thoſe little balls or lumps turned into perfect pebble ſtones, which he obſerved likewiſe to befall thoſe, which he had carried and laid up at home. But how far this ſtory will prove, that ſuch coagulations muſt be effected by a ſubſtantial form of a petrifying liquor, we define not; eſpecially ſince, not to repeat what we delivered already touching calcined marble out of *Fournier*, we have elſewhere delivered upon our own obſervation, that two or three ſpoonfuls of ſuch pap of burnt alabaſter, as we have lately been ſpeaking of (and inſtead of which artificers uſe another ſtone called by them plaiſter of *Paris*, burnt and tempered up with fair water) did in the bottom of a veſſel-full of water, into which we poured it, in a ſhort time coagulate into a hard lump, notwithſtanding the water, that ſurrounded it; which, it ſeems, by the texture of the maſs, was kept out of its pores, as it is out of thoſe of the oils of cinnamon and cloves, which, though fluid bodies, and ſinking in water, ſuffer not its particles to inſinuate themſelves into theirs: and artificers obſerve, that the coagulating property of burnt alabaſter will be very much impaired, if not loſt, if the powder be kept too long, eſpecially in the open air, before it be

Gaſſend. in vita Piereſkii, Lib. 1.

made uſe of; and when it has been once tempered with water, and ſuffered to grow hard, they tell me they cannot by any burning or powdering of it again make it near ſo ſerviceable for their purpoſe as before; ſo much doth the coagulation of theſe powders mixed with water ſeemed to depend upon their texture and other mechanical qualities.

I REMEMBER alſo, that though the bones found in the hearts of deer, and ſo magnified by phyſicians, do, in the air, acquire a hard and bony conſiſtence; yet having had the curioſity to conſider one of them in the heart of a deer newly killed, I found it there of a cartilaginous ſoftneſs and flexibility.

AND here I will adventure further to confeſs, that I have oftentimes doubted, whether or no, not only conſiſtent bodies, but ſome of the moſt ſolid ones in the world, may not have been fluid in the form either of ſteams or liquors, before their coalition and their concretion either into ſtones or other mineral bodies. I know there are many, who think, that ſtones, marchaſites, and other ſuch ſolid and durable bodies, were made together at the creation or other beginning of the univerſe, and who will not admit, that ſuch concretions can be now generated. But not here to debate that famous controverſy, whether ſtones may be ſaid to grow and to be nouriſhed, in the ſtrict ſenſe of thoſe expreſſions, I think it not difficult to ſhew, that ſuch parcels of matter are now to be met with in the form of ſtones, as did not before appear under that form; but whilſt it was divided into minute parts, either was itſelf ſome fluid body or other, or at leaſt did as a material part concur to the conſtituting of one, that was ſo. Of this, beſides what we elſewhere deliver concerning it, we ſhall anon have occaſion to mention ſome proofs; and therefore we ſhall now only mention two or three inſtances. The firſt whereof ſhall be, that we ſaw, among the rarities of a perſon exceedingly curious of them, a ſtone flat on the outſides; on one of whoſe internal ſurfaces was moſt lively engraven the figure of a ſmall fiſh, with all the fins, ſcales, *&c.* which was affirmed to have been incloſed in the body of that ſtone, and to have been accidentally diſcovered, when the ſtone chancing to receive a rude knock upon its edge, ſplit aſunder. I remember alſo, that a while ſince a houſe-keeper of mine in the country informed me, that whilſt a little before he cauſed in my abſence one of my walls to be repaired, the maſon I was wont to employ caſually breaking a ſtone to make uſe of it about the building, found in it (to his wonder) a piece of wood, that ſeemed part of the branch of ſome tree, and conſequently afterwards incloſed with that ſolid caſe, wherein he found it. This cavity in the body of the ſtone, and, as I remember, the ſtick itſelf he took out of it, he forthwith brought my houſe-keeper, to whom I have given directions to ſend them me. For this example ſeems to me a more cogent proof of the increaſe of ſtones, than ſome others, that eminent naturaliſts much rely on, for reaſons diſcourſed of in another place*: where we alſo make particular mention of that ghur or metalline juice, which though the Latin writers of chymical, and even of metalline, matters have not, that I remember, given us any account of, yet I find a German or two, that were very converſant in the mines themſelves, to have in books written in their own language taken a ſpecial notice of it. Beſides, I have at preſent ſomething to deliver upon my own obſervation, which, unleſs we will ſuppoſe (what ſeems not probable) that there were from the beginning made together with and in the midſt of great maſſes of one kind of mineral little parcels of another of a quite differing ſort, ſeems manifeſtly enough to argue, that either whole quarries of ſtone, or heavy and ſhining minerals, or both, may have been fluid bodies. The obſervation, whereon I ground this conjecture, is, not only, that we have met with in lead-ore, and alſo in *Minera Antimonii* particles of a white ſtone or ſpar environed with a metalline body, though I think I have yet by me ſuch lumps of ore; but chiefly, that I have, with my own hands, taken a hard and ponderous ſhining mineral, which I keep for a rarity, like a marchaſite, of the ſhape of a pear, and of about the bigneſs of a walnut, out of the very body of a ſtone, wherein I ſuſpected it to be incloſed, and which environed it on all ſides: and this I took not out of a ſmall and looſe ſtone, but a large ſtone digged out to make ſtatues of. And I remember, that one of thoſe, that wrought upon it, told me, that in faſhioning it into ſtatues they found ſome more minerals in the ſame parcel of ſtone, which were alſo preſented me. To which I ſhall add, that an ingenious ſtatuary having in another place taken much pains to ſaw aſunder a very large ſtone, when he came to the midſt of it, found he could ſaw on no further, and the ſtone being afterwards broken, he perceived, that that, which ſo obſtinately reſiſted his ſaws, was a round marchaſite, which he brought to me, as a lover of ſuch curioſities. But I made him for my further ſatisfaction bring me alſo that part of the ſtone, wherein the marchaſite ſtuck, and by comparing them together diſcerned, that as much of the ſtone, as was contiguous to the marchaſite, had a kind of ruſt about it, and fitted the marchaſite ſo cloſe, as if either the marchaſite had been formerly liquid, and had afterwards been as it were moulded in that receptacle, or the ſtone had been formerly of ſome ſoft or fluid matter, which did exactly accommodate itſelf to the ſhape of the other body; or elſe, as if both the matter of the ſtone and that of the marchaſite had been at once fluid bodies, but had each of them preſerved its own ſurface diſtinct (according to what we formerly noted of different fluids) till one of them (probably the marchaſite) firſt growing hard, the other, as being yet of a more yielding conſiſtence, accommodated itſelf to the harder's figure.

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* In ſome of the Author's papers about the origin of minerals.

BUT the moſt eminent inſtances to declare, how much the fluidity and firmneſs of bodies depend upon the contrivance and texture of their partts, are afforded us by thoſe waters, which being permitted to reſt a while, do ſpontaneouſly ceaſe to be fluid, and coagulate into ſtone itſelf. There was lately an ingenious man, who going to viſit ſome leaden mines, wherein he had a ſhare, found in the mountain, in whoſe entrails they were hid, a cave, from whoſe arched roof there dropped down a petreſcent liquor, which oftentimes before it could fall to the ground congealed, and by appoſition of like matter increaſed ſo much, that they hung from the roof like icicles in a froſty-night from the ſides of a houſe; and of theſe he gathered and brought me divers, which are perfect ſtones hard and brittle, and of eight or ten inches long, and proportionably thick. Another ingenious friend of mine being lately in *France*, in the cave ſo famous for petrifying liquor to be there ſeen, obſerving ſome drops of water to congeal into ſtone, whilſt he ſtood by, took them away with him, and ſent them me in a letter. Nay, we ſhall ſcarce deny, that an external agent of almoſt inſenſible bulk may turn animal bodies into ſtony ones, by introducing a new texture into their parts, if we will, with ſome modern writers, believe *Aventinus*, who in his *Bavarian Hiſtory* has recorded, that at a time and place by him ſpecified, above forty countrymen, as alſo ſome milk-maids with their cows killed upon an earthquake, had their bodies by a terrene ſpirit turned into ſtatues, which, he ſays, were ſeen by the chancellor of *Auſtria* and himſelf. And ſome relations of this nature we meet with in other authors, which, if they be allowed of, ſeem much to confirm our doctrine; for in theſe ſtrange petrifications, the hardening of the bodies ſeems to be effected principally, if not only, as in the induration of the fluid ſubſtances of an egg into a chick, a chick, by altering the diſpoſition of their parts, ſince the petrifying wind or ſteam cannot be ſuppoſed to have any ſuch conſiderable (perhaps not any ſenſible) proportion as to bulk to the body changed by it, as to be thought to effect this change principally as an ingredient.

ADD we to all theſe things, that *Pamphilio Piacentino* is by another author quoted for writing an unparalleled ſtory, which becauſe written in Italian, I ſhall Engliſh the ſubſtance of it, which is this: 'That a woman in *Venice*, after having eaten an 'apple, was taken with hideous tortures, and in the ſpace of twenty four hours 'dying, was turned into exceeding hard ſtone; and this was judged to be the effect 'of the poiſoned apple ſhe had eaten.' Which narrative, if we may believe it as confidently as the famous alledger of it *Pamphilio* appears to do, would ſeem to argue, that even to the wonderful induration of bodies there is ſometimes no other principle requiſite, than what may reſult from the lucky mixture of the parts of ſeveral bodies. And leſt we ſhould ſeem to build altogether upon the obſervations of others, which cannot by us be now brought to ſtrict examination, we will have recourſe to a practicable experiment of our own trying, which, though we have elſewhere mentioned, we ſhall not ſcruple here to repeat, becauſe we there omitted to ſpeak of that circumſtance of it, which is the moſt pertinent to our preſent deſign.

TAKE then two ounces of quickſilver, two ounces and a half of the beſt verdigreaſe, about half an ounce or an ounce of common ſalt, a pint or pound of white-wine vinegar, and as much fair water; mingle the verdigreaſe, quickſilver, and the ſalt very well, and put the mixture with a little of the vinegar and water into a new frying-pan (I tried it in a new earthen veſſel, but without good ſucceſs) in which fry it over the fire for divers hours, keeping it continually ſtirred, and putting in more vinegar and water from time to time, as that already put in conſumes away: then take out the mixture, and in ſeveral clean waters waſh it carefully from the adhering ſalts; then dry away all the aqueous moiſture with a clean linen cloth, and you ſhall have a bright amalgama almoſt like quickſilver. Now that which is remarkable and to our preſent purpoſe in this experiment, is, that though this dried mixture be a good while after it is perfectly cold not only ſoft, but ſo near to fluid, that I have caſt it into moulds, and made imboſſed images of it (when it has been dexterouſly made, but ſcarce otherwiſe;) I have found, that by laying it a few hours in the air, which ſeemed leſs cold than itſelf, it has acquired ſuch a hardneſs, that being thrown againſt the floor it would rebound, and was brittle like over-hardened ſteel. And yet in this example the induration of the amalgam appears not to proceed from an innate and inward principle, but from the new texture reſulting from the coalition of the mingled ingredients, that make up the amalgam, whoſe parts being variouſly moved, partly by the fire (and, perhaps too, by the ſalts) and partly by the native propenſity to motion of the mercurial corpuſcles, were by little and little, or by degrees, ſo diſpoſed, that whereas before touching one another but looſely, it was eaſy to thruſt ſome of them towards the middle of the body, without ſtirring much of the maſs (as to ſenſe) by this change of texture, the particles are brought to touch one another more cloſely and in greater portions of their ſurfaces, and to be ſo complicated, intangled, or otherwiſe connected among themſelves, that you cannot endeavour to thruſt one of them out of its place, but that its motion ſhall be reſiſted by many others, to whom it is ſo faſtened, that you cannot move one part of the maſs without either moving the whole with it, or manifeſtly breaking it off from the whole, and thereby deſtroying the continuity and unity of the body.

Now whereas, in ſetting down this experiment, we ſpoke as if ſeveral ingredients did concur to conſtitute the ſoft maſs, which afterwards grew ſo hard, we might very ſafely do ſo, ſince the quickſilver was not ſo barely changed in texture, as that formerly ſaid to have been coagulated by the meer fume of lead, but concealed in its ſelf a great number of metalline corpuſcles beſides others, as we made appear by ſeparating from the amalgam, meerly by the force of fire, a pretty quantity of true and perfect copper. That the ſalts alſo both were ingredients (though in ſmall proportion) of the maſs, and might have ſome operation upon the other particles, we may render probably by this, that having purpoſely expoſed ſome of this maſs for a pretty while to a moiſt air, we found, as we looked for, that the formerly inviſible particles of ſalt, that had ſo inſinuated themſelves into the amalgam, that all the water, wherein it was waſhed, did not ſeparate them from it, had ſo wrought upon the metalline particles, that were moſt outward, that they had in many parts of the ſurface of the maſs turned themſelves with it into a kind of verdigreaſe, which ſeemed almoſt to hide the ſurface of the concretion. And that in the more inward parts of a much harder body than our yielding amalgam, where cupreous particles abound, ſaline corpuſcles may have a great operation, may appear by certain ſorts of minerals to be found in ſome parts of *England*, and elſewhere, under the form of ſtones, of which they make vitriol: for theſe abounding with vitriolate, that is, both ſaline and metalline particles, will, after they are taken out of the ground, and laid in the open air, by the working of the inward ſalt, ſome ſooner and ſome later, ſwell and burſt aſunder, which could hardly come to paſs, without a great change made in the internal diſpoſition of the parts of ſuch ſtony concretions. And I remember, that having laid a mineral, of kin to theſe ſtones, a while in the air, though but in a chamber, I found its ſurface powdered with little grains of vitriol, as both their colour and their taſte informed me.

Now whether or no we ſuppoſe, that the fire did put the parts of the amalgama into any laſting agitation, yet the maſs being almoſt fluid after it was taken from the fire, its parts may, according to our notion of fluidity, be well ſuppoſed to have ſome kind of motion among themſelves; and it will not be denied, that the fire might concur with other things, to make that motion convenient to cauſe the parts to faſten themſelves to one another: for that the motion, wherein a ſoft and almoſt fluid body is once put, may poſſibly tend to harden it long after that motion ſeems to be extinct, may be made probable by what has been affirmed to me by eminent and experienced maſons, namely, that the beſt ſort of lime made into mortar will not have attained its utmoſt compactneſs till twenty-five or thirty years (perhaps not till three or fourſcore) after it has been employed in building; and this is given me as one of the reaſons, why in the demoliſhing of antient fabrics, it is ſometimes more eaſy to break the ſtone than the mortar.

AND laſtly, that we alſo made mention of the texture reſulting from the mingled ingredients of our amalgam, we might juſtify by ſaying, that having changed the proportion of the quickſilver to the verdigreaſe, we found, that the amalgam coagulated much more ſlowly, and, when it was coagulated, was much leſs hard, than when one uſed the quantities above ſpecified.

HERE I ſhould put a period for the preſent to this diſcourſe, but that having in a late writer met with a notable obſervation of the natural induration of a ſoft body, I think it worthy to be here annexed; partly, becauſe the French book is not common, no more than the obſervation; and partly, that by conferring together this natural induration with that artificial one freſhly mentioned, it may the better appear, how nature and art have ſometimes reſembling operations in rendering bodies ſolid. My * author then (by name *Pierre Pelleprat)* being not long ſince ſent with ſome other Jeſuits upon the laudable errand of preaching the goſpel to the Indians of the Southern *America*, has among other things this paſſage in the ſhort relation he makes of the American continent: 'There is (ſays he) one thing worthy of obſervation 'near the mouth of this great river, (he ſpeaks of that of the Amazons) which is, 'that men find there a kind of green clay, that is ſoft as long as it is in the water, 'ſo that one may print on it all kind of figures, and give it what ſhape one pleaſes; 'but when it is expoſed to the air, it hardens to that degree, that diamonds are not 'much harder than the ſtones it affords. — I have (adds he) ſeen hatchets made of 'this clay, which the ſavages employed to cut wood with, when they had not the 'uſe of ours, *&c.*'

AND now at laſt, I ſee it is time to put a period to a diſcourſe, that has been unawares lengthened far too much already: but yet I think you will eaſily pardon me, if I conclude it not abruptly; but with the recital of an experiment, which having had the honour to be ſeen, as to the main part of it, by an illuſtrious meeting of curious men, their having been pleaſed to ſpeak very advantageouſly of it to others excited a curioſity among them, to know by what artifice effects, that were ſo uncommon, had been produced. The ſcope therefore, and the manner of making the experiment, were in ſhort as follows.

BEING deſirous to ſhew, how much fluidity and firmneſs may depend upon the texture and upon the motion or reſt of the inſenſible parts of bodies, I firſt make with good ſpirit of vinegar, a ſolution of coral ſo ſtrong, that when it is filtrated and cooled, it will commonly, after ſome time, begin to have a kind of ſediment at the bottom; the clear liquor I gently pour off, when the experiment is to be made, and to this I put a convenient proportion of very well dephlegmed ſpirit of wine, which, if it be poured on very ſlowly and warily, may be made for a pretty while to ſwim upon it in the form of a diſtinct liquor: but if by a few ſhakes I mingle them together, they will preſently unite into a concretion, of which, when the experiment ſucceeds very well (as it did when I ſhewed it to the above mentioned aſſembly) not one drop will fall to the ground, upon turning up the wide-mouth'd glaſs it ſhould be made in, and holding it with the mouth directly downwards.

AND this ſo haſtily-produced conſiſtence may be durable enough, if it be carefully looked to: but to diſpatch the whole experiment in a ſhort time, I take a little ſtrong ſpirit of nitre (which perhaps is not needful if good aqua fortis be at hand) and putting about an equal, or other convenient quantity of it to this mixture, I nimbly ſtir it and the ſpirit together: whereupon the whole is reduced in a very few minutes to a tranſparent liquor.

N. B. THOUGH I have divers times made and ſhewn this experiment, yet there are ſo many circumſtances requiſite to make the firſt part of it ſucceed very well, (for to make it ſucceed in ſome meaſure is not ſo difficult) that the event has ſometimes deceived me, in ſpite of the ſeveral trials I have made. Wherefore it will not be amiſs to intimate,

FIRST, That one of the firſt times, if not the firſt, I made ſuch an odd concretion, was, with the ſolution not made with ſpirit of vinegar, but with ſpirit of verdigreaſe: (which I commonly diſtil without additament) though afterwards I was invited to prefer ſtrong ſpirit of vinegar, which was the liquor, wherewith the recited experiment was exhibited.

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* *Relation des Miſſions des P. P. J. J.* Part. II. Cap. I.

SECONDLY, That it often happens, that if the ſolution of coral (which is not the only body wherewith I have made ſuch trials with indifferent good ſucceſs) be not ſufficiently ſtrong and impregnated with ſaline parts, or the ſpirit of wine be not ſufficiently rectified, the ſhaking of the two liquors will not change the conſiſtence of the whole mixture, but leaves ſome part of it fluid; or elſe the concretion will not begin preſently to be made, but require to be waited for a while.

THIRDLY, That I once at leaſt (if not oftener) obſerved, that when by mingling the two liquors and ſhaking them in a narrow-mouthed glaſs, whoſe orifice was ſtopped, they would not con-coagulate (as it was confidently expected they ſhould) yet by trying the experiment in a wide-mouthed glaſs, to which the air had free acceſs, it ſucceeded to my content.

FOURTHLY, That in the reduction of the concretion to a fluid body, it is not proper to employ in ſtirring it a knife or any other metalline body except it be of gold; but rather ſome ſtick of glaſs, or at leaſt ſome clean ſtick of wood, leſt the menſtruum ſhould corrode it, and thereby ſpoil, or at leaſt blemiſh the experiment.

FIFTHLY, that the proportion betwixt the coralline ſolution and the ſpirit of wine depends ſo much upon the ſtrength of the former liquor, and the dephlegmedneſs of the latter, that it is ſcarce poſſible to determine generally and exactly, what quantity of each ought to be taken; and therefore a trial or two made with a little of the particular ſolution you intend to employ (for ſome ſolutions require more, others leſs ſpirit of wine to concoagulate adequately with them) will better direct you, what proportion of ſpirit will ſuit that particular parcel of liquor, than any general rule I can propoſe.

I KNOW I might here, and perhaps it may be expected, that I ſhould take an occaſion to treat alſo of hardneſs, ſoftneſs, brittleneſs, toughneſs, ſtiffneſs, and thoſe other qualities, that are of kin to fluidity and firmneſs; but though I confeſs, I once had thoughts of writing a kind of hiſtory of more qualities than thoſe, yet remembering that wiſe counſel given us by one of the ancients, *Noſcenda eſt menſura ſui*, I am now very well content, after having already tired myſelf and, I fear, you, to recommend ſo uſeful but difficult a work to perſons more able and more at leiſure, than I find myſelf, to go through with ſo great an undertaking; contenting myſelf at preſent, to have attempted in what has been delivered concerning a couple of qualities of ſuch extent, that every ſenſibly big body in the univerſe ſeems indowed with one or other of them (I mean concerning fluidity and firmneſs) the explicating of qualities ſomewhat more intelligibly than is wont to be done in the Peripatetick ſchools, and to have opened a way (which I hope many will tread) of applying chymical obſervations and experiments to the deduction of thoſe effects of qualities from ſuch general and obvious affections of matter; as bigneſs, motion, and figure, which even the hermetical writers have hitherto contented themſelves to refer to ſalt, ſulphur, mercury, and the like: the chymical notion of which three principles, though of very good uſe in ſome other (eſpecially of the more practical) parts of phyſiology, ſeems not as yet to have brought any great light to ſuch matters as we have been treating of having been hitherto directed not ſo much to the indagation of cauſes, as to the production of effects.

OF
ABSOLUTE REST
IN
BODIES.

An Advertiſement.

SINCE it hath not been thought amiſs, that ſomething ſhould be intimated to the reader about the occaſion of the enſuing tract, I ſhall acquaint him, with it as briefly as I can, by telling him, that it was this. Some very ingenious gentlemen happening to meet as viſitants at the author's lodgings, fell accidentally into a diſcourſe about the abſolute reſt ſuppoſed to be in many bodies, that ſeemed to have its riſe from a miſtake of the true meaning of a paſſage or two in the *Hiſtory of Fluidity and Firmneſs*, (that was then reprinting.) But the conference chancing to have a period put to it, whilſt ſeveral things pertinent to the author's purpoſe remained yet unſaid; the curiouſneſs of the ſubject invited him to draw up (haſtily enough) the ſum of what he had ſaid, and might further have ſaid, if opportunity had ſerved, about the point in debate, for the further ſatisfaction of an inquiſitive virtuoſo, that was preſent at it. And this was the riſe of the following diſcourſe, which being written on an occaſion adminiſtered by the *Hiſtory of Fluidity and Firmneſs*, whereof a new edition was ready to come abroad; it was thought not improper, that this tract ſhould attend it, as a kind of Appendix, without the firſt and laſt part of a letter, whereof the body only is neceſſary to the deſign of it.

An ESSAY of the inteſtine Motions of the Particles of Quieſcent Solids:

Where the abſolute Reſt of Bodies is called in queſtion.

SECTION I.

TO remove the doubt or ſcruple, that began to be diſcourſed of juſt before we laſt parted, I ſhall need to do little more than enlarge the particulars, which (you know) I had then time but briefly to make mention of. For the ſtate of the queſtion was (as you may remember) this, *Whether there be among bodies any abſolute reſt?* On which occaſion I anſwered, that reſt being a word, that to me ſeemed ſomewhat ambiguous, I thought it was requiſite to clear the ſenſe of the queſtion, before I offered at anſwering it.

For the word reſt, when we ſpeak of diſtinct maſſes of matter, looked upon as quieſcent, does in the vulgar acception of the term ſignify, ſuch a ſtate of a viſible and entire body, or (rather) of the corpuſcles it conſiſts of, that they are actually unmoved as to ſight; the eye (and perhaps the touch) being not able to diſcern any local motion in them.

Consonantly to this firſt member of the diſtinction of the word *reſt*, I briefly intimated to the company, that in this ſenſe of a corpuſcle's being at reſt, I thought it manifeſt, that there is ſuch a thing *in rerum natura*; ſince without granting ſuch a reſt in the component particles of ſome kind of bodies, as diamonds, iron, porphyry, *&c.* it will be (I conceive) very hard to explain, how there can be ſuch ſolid maſſes (as thoſe minerals are) made up of ſmall and ſeparable particles. Which being ſaid, I added, that I ſaw no reaſon, why ſuch a kind of firmneſs, where the inward motion of the inſenſible particles is almoſt infinitely ſlow, may not ſuffice to give an account of as great a firmneſs as we uſe really to find among conſiſtent bodies.

But whereas I had intimated to the company, by the lately begun diſtinction, that beſides this popular ſenſe of the word reſt, there was a ſecond more rigid and philoſophical notion, or kind of reſt, which for diſtinction-ſake may be called abſolute or perfect reſt, which imports a continuance of a body in the ſame place *preciſely*, and includes an abſolute negation of all local motion, though never ſo ſlow or imperceptible; I told them, that in this rigid ſenſe of the word reſt, I durſt not affirm, that there are any bodies at reſt in the univerſe (at leaſt for any long time) but willingly allowed it to be made a problem, whether there be any or no; adding, that perhaps I inclined to the negative part of the queſtion.

Having thus hiſtorically ſummed up what paſſed betwixt us about the ſtate of the controverſy, I need not tell you, that the doubt I expreſſed was thought to reliſh too much of a paradox; and therefore ſince the company's quick ſeparation allowed me then no opportunity of enlarging, and ſince I promiſed no better arguments, than might be expected in a point, that I propoſed but as problematical; I ſhall now endeavour to ſhew you, that the ſide of the problem I was judged inclined to, is (at leaſt) not ſo improbable as ſome thought it.

To prove negatives directly, being wont (as you well know) to be no eaſy taſk, and eſpecially in ſuch caſes as this; you will not, I preſume, expect, that I ſhould attempt the proving of my conjecture otherwiſe than by ſhewing poſitively, that ſome of thoſe bodies, which we think to have their parts moſt at reſt, are not exempted from having inteſtine motions in them; ſince it will be conſequent to ſuch a proof, that it muſt be probable, that in other bodies, whoſe ſolidity is confeſſed to be inferior, the component particles are not in a ſtate of perfect reſt.

SECT. II.

IF it were neceſſary and expedient, I ſhould begin my arguments by ſaying ſomething againſt abſolute reſt, in favour of the contrary opinion, by arguing *à priori*, as they ſpeak, from the conſtitution of the world, whether we conſider it according to the Epicurean or the Carteſian hypotheſis of the origin of things. For the Epicureans ſuppoſing this world to be produced by the caſual concourſe of atoms, and aſcribing to every particular atom an innate and unloſeable mobility, or, rather, an actual motion, or a reſtleſs endeavour after it; it is conſonant to think, that though in concretions they ſo entangle one another, that they cannot in a ſhort time, or a viſible manner clear themſelves from one another, yet they do inceſſantly ſtrive to diſentangle themſelves and get away: by which means there are always in the atoms even of ſolid bodies actual endeavours of each of the diſtinct atoms to extricate itſelf from the reſt (which endeavours uſually at laſt ſucceed, whence comes the decay and deſtruction of bodies) and in the mean while theſe perpetual and contrary endeavours produce inteſtine commotions in the internal parts of the body, wherein theſe atoms were impriſoned.

ON the other ſide, according to the Carteſian doctrine, the *materia ſubtilis*, that conſtantly paſſes like a ſtream through the pores even of the ſolideſt bodies, may well be ſuppoſed in its paſſage to be continually ſhaking, or otherwiſe agitating the inſenſible particles, that make up the body, that ſeems to be at reſt, without diſcovering their motion to the eye: as when in ſummer-time (to explain myſelf by a compariſon) a gentle breath of wind paſſes through a grove removed a pretty way off from the ſpectator, though his eye diſcern no change in the grove he looks on, yet the wind, as it blows through the trees, will ſhake ſome of the branches as well as the flexible twigs; and not only blow the leaves into various poſtures, but blow ſome of them quite away.

I MIGHT eaſily enlarge on this ſubject, but having elſewhere done it on another occaſion, I think it may be now more proper to ſatisfy ſome of the company, who are yet entangled with the ſame prejudice with many other very learned men, who look upon it as a precarious and chimerical fancy of the Atomiſts, to imagine, that in ſolid, and, as to ſenſe, quieſcent bodies, there ſhould be any inteſtine motion of the component particles, neither the motions nor the corpuſcles themſelves being to be ſeen, and both of them being therefore as well incredible as inviſible.

A SOLEMN debate of the whole queſtion about the minuteneſs of atoms belongs not to this place, where it may ſuffice to anſwer the objection.

SECT. III.

AND firſt, as I have elſewhere hinted, it may appear by divers of the phænomena above cited (in the *Hiſtory of Fluidity*) that when water and ſeveral other liquors ſeem to be continued maſſes of matter, and to be as much at reſt, as the very glaſſes, that contain them; their conſtituent corpuſcles are in an actual and various, though ſlow and unperceived, motion.

NEXT, that there may be likewiſe ſuch a motion in the minute parts of ſilver and iron themſelves, may be eaſily argued, by heating thoſe metals, till they come to be almoſt red hot: for then, though the eye can diſcern no motion of the corpuſcles thoſe metals conſiſt of, yet their being able to burn thoſe, that hold them in their naked hands, ſhews, that their briſk motions may be diſcovered by the help of the touch; and if you ſpit upon them, the liquor will boil, as if it were over the fire. And leſt it ſhould be objected, that ſo anomalous and violent an agent as the fire is neceſſary to theſe trials, I ſhall add, that, provided the minute parts be ſufficiently agitated, it matters not, whether the motion be produced by fire or no: for by the nimbly hammering of iron or ſilver, you may put the minute parts into ſuch a motion, as will make the metal very hot to the touch, and being communicated to ſpittle or water, will excite bubbles, and ſcatter the diſſipated parts of that liquor into the air, in the form of ſmoke or vapours: nay, I elſewhere ſhew, how I have eaſily excited a very ſenſible, though not a viſible agitation, and heat in the internal parts of a metal, barely by my naked hands, without any external inſtrument whatſoever.

AND whereas it may be objected, that though the motion already generated is unſeen, yet we may diſcern a change of the component corpuſcles of a body, which are in the act of altering its texture, and introducing a new alteration or quality in the body to be wrought on, or deſtroying ſome pre-exiſtent quality: I briefly anſwer (for I would not here repeat what I have elſewhere ſaid of this point) by this clear experiment, that though your eye can diſcern no change in the outward and viſible, much leſs in the more latent and internal corpuſcles of iron; a vigorous loadſtone, by paſſing along its axis from one pole of the ſtone to the other, and back again, yet the texture of the iron is by that action of the loadſtone ſo changed, that it acquires, and then loſes thoſe admirable qualities we call the attractive and directive virtue or faculty peculiar to magnetical bodies.

AND to ſhew you, that the inviſible motions even of metalline bodies may be quick and briſk enough, and may be ſenſible, though not viſible: we ſhall need to conſider but the ſtate of a good bell ſo long after the clapper has ſtruck it, that no ſhaking or other motion is to be ſeen in the body of the bell itſelf, and yet it cauſes in the air an odd kind of ringing, or, if I may ſo call it, undulating ſound or motion, which will ſometimes laſt a conſiderable while; and if the bell be fitted for ſharp notes, it will not be without a ſhrillneſs: for if ſounds proceed, as is elſewhere made probable, from the nimble percuſſions of the air put into a quick and waving motion by ſonorous bodies, this acuteneſs of ſound will ſhew, that whilſt to the eye the bell ſeems to be at reſt, yet the minute parts of it continue in a very briſk motion, without which they could not ſtrike the air ſtrongly, and faſt enough, to make it produce ſo ſhrill a noiſe in the ear.

BUT, I confeſs to you, that my thoughts preſent me a difficulty, which, though unmentioned at our meeting, may afford an objection, perhaps more difficult than any of (not to ſay all) the foregoing; namely, that it is ſcarce imaginable, how ſuch ſolid and hard bodies ſhould have their internal parts wrought upon by ſuch ſlight agents as the air, and perhaps ſome yet minuter matter, that is diſperſed in it; and how it is poſſible, that where there is an actual motion, it ſhould be ſo ſlow, that a corpuſcle of iron, for inſtance, ſeated in the internal part of a magnetic needle, ſhould ſpend ſo long time, as our conjecture requires, in travelling ſo little a ſpace as from thence to the next ſuperficies of the needle. But to this double objection, though ſome inſtances, which you will meet with in the following part of this paper, may be properly applied to ſolve it; yet not to make your curioſity wait, I will here ſpeak a word or two to each of the members of the objection.

SECT. IV.

AND, to the firſt, I ſay, that theſe inteſtine motions of the corpuſcles of hard bodies need not be ſolely, nor perhaps principally aſcribed to thoſe obvious external agents, to which we are wont to refer them, ſince theſe may but excite or aſſiſt the more principal or internal cauſes of the motions we ſpeak of, as you may gather from what was but lately mentioned of the connate and unlooſeable mobility of the atoms, according to *Epicurus*, and the permeation of the moſt ſolid bodies by the Carteſian *materia ſubtilis*; and we may ſee by the ſudden effects of the loadſtone, in endowing ſteel with magnetic qualities, and depriving it of them again (both which ſuppoſe the intervention of a change of texture, and this a production of local motion in the metal) that very minute and inſenſible corpuſcles of matter are not uncapable of effecting durable changes in the ſolideſt bodies.

AND as for the other member of the objection, I confeſs it is not eaſy for us, who are wont (perhaps too much) to follow our eyes for guides in judging of things corporeal, and to deny exiſtence to moſt things, whereto nature has denied a viſible bulk: it is not eaſy, I ſay, for us to imagine ſo great a ſlowneſs, as it is very poſſible for nature to make uſe of in her operations, though our not being able to diſcern the motion of a ſhadow of a dial-plate, or that of the index upon a clock or watch, ought to make us ſenſible of the incompetency of our eyes to diſcern ſome motions of natural bodies, which reaſon tells us ought to be incomparably ſlower than theſe. But not now to diſpute about the exiſtence and attributes of infinite ſlowneſs, or at leaſt a ſlowneſs in the next poſſible degree to infinite; I conſider, that it has not, that I know of, been demonſtrated, nor attempted to be ſo; that the motion of the corpuſcles, for example, of the needle above-mentioned, muſt be made in a direct line from the place, where it was firſt ſuppoſed to be, to the ſuperficies of the needle; for it ſeems more rational, and to agree better with the phænomena, to ſuppoſe, that the way of this corpuſcle in the body it would quit is extremely crooked and intricate (almoſt like that of a ſquib in the air, or on the ground) for it being on the one hand urged on by the cauſes, whatever they be, that make it ſtrive to fly away, and on the other hand hindered by the corpuſcles, whereto it is connected, and by the occurſions of other corpuſcles, whoſe motions may be oppoſite to, or diſagreeing with thoſe of our deſigned corpuſcle; it may probably, before it can extricate itſelf, be reduced to encounter and wreſtle, as it were, with many other corpuſcles, and be by them ſometimes thruſt or impelled to the right hand and to the left, and ſometimes alſo repelled inwards, even after it is come to the ſuperficial part of the needle, whence it may not preſently have the liberty to fly away, but may be drawn back by ſome other corpuſcle, wherewith it is yet connected, and which happening to be itſelf thruſt inward, may be drawn after it, and ſo entangle again our almoſt diſbanded corpuſcle: beſides that the gravity of the component particles of a body is ſometimes ſuch, that it is eaſier for the agent, that puts them in motion, to continue them in that ſlow motion among themſelves, than drive them up into ſo light a medium as the air; as experience ſhews in thoſe bodies, that are called fixed, as gold and glaſs, though in actual fuſion.

But I forget, that I promiſed you to decline ſpeculations, and therefore I ſhall only name to you a couple of inſtances, which will ſerve to confirm both what I was lately ſaying, and what I am now in proving.

SECT. V.

The firſt of theſe I ſhall take from what is uſually granted as matter of fact; namely, that if a ſpring, though made of ſo hard a body as ſteel, be forcibly bent, and kept but a moderate while in that poſture, as ſoon as the force, that kept it bent, is removed, it will again return to its former figure; but if it be kept too long in that forced poſition, it will by degrees loſe that, which they call the motion of reſtitution, and retain its new crooked figure, though the force, that bent it, be removed; which ſhews both the power of ſome of the more familiar agents in nature, and (which is that, the ſhewing whereof I here chiefly aim at) that where there is a continued endeavour of the parts of a body, to put themſelves into another ſtate, yet the motion, or rather the progreſs may be much more ſlow, than men ſeem as yet to have taken notice of, ſince it was a great while before the texture of the corpuſcles of the ſteel were ſo altered, as to make them loſe their former ſpringineſs.

But I will ſecond this with a more illuſtrious experiment, which will at once confirm what I have juſt now ſaid, and ſhew, that the air or the inviſible corpuſcles harboured in it may have no inconſiderable power to act upon, and effect changes in the ſolideſt bodies.

To this purpoſe I ſhall only obſerve to you, that though if a bar of iron having one of its ends held perpendicularly, and at a fit diſtance, to the lily or north-point of the mariner's compaſs (I mean that, which points towards the north) it will, as I elſewhere mention, drive it away towards the eaſt or weſt: and if this ſame lower end of the bar of iron be put into a contrary poſture, it will preſently loſe its temporary magnetiſm, as I elſewhere declare. Yet, if this bar be very long kept upright in a window or other convenient place, then, as ſome late magnetical writers will tell you, it will have acquired a conſtant and durable magnetic power. Which is a phænomenon, that makes exceedingly for our preſent purpoſe, ſince it hence appears, both that the air, together with the magnetical effluvia of the earth, that it receives in its pores, is able without outward force to work durable changes in ſo ſolid a body as iron, and that the motions of the internal parts (for theſe are requiſite to the change of the metal's texture) are performed with a wonderful ſlowneſs, ſince the bar muſt be very long expoſed to the air, perhaps before it acquires any durable magnetiſm at all, but at leaſt before it acquires ſo vigorous and fixed a magnetiſm, as by this means it may attain to.

But, becauſe moſt of the inſtances to be propoſed in the following part of this eſſay may ſerve for confirmations of what we have been diſcourſing, I ſhall proceed to them; yet not till I have advertiſed you, that I purpoſely decline to mention divers phænomena, that may even by learned men be thought fit examples on this occaſion (ſuch as the nutrition, growth, and waſting of animals and vegetables) becauſe ſuch bodies receiving conſtant ſupplies of corpuſcles, of ſeveral, and often unknown, natures, there may be difficulties ſuggeſted about them, not eaſy to be cleared without longer diſcourſes, than I can allow this eſſay.

SECT. VI.

THE firſt inſtance then, that I ſhall mention about vegetable ſubſtances, ſhall be taken from *lignum vitæ*, or *guaiacum* (for many artificers give them the ſame name, and uſe them promiſcuouſly for the ſame purpoſes) of which, though it ſeem to be the ſolideſt wood we know (inſomuch, that I as well as ſome others have ordinarily uſed it to pound ſolid bodies in) yet the ſkilfulleſt tradeſmen I have met with, have upon my inquiry informed me, that if it be wrought, before it be well ſeaſoned by length of time, it will ſhew itſelf very frangible; which an eminent turner told me he had often found to his loſs: for having turned divers fine pieces of work of *lignum vitæ*, before it was duly ſeaſoned, he found almoſt all of them by the heat of the ſun (which the uſes of many of them required they ſhould ſometimes be expoſed to) crack, and cleave aſunder, into I know not how many parts; whereby thoſe fine pieces of workmanſhip were quite ſpoiled. And I remember, that having inquired of an old experienced tradeſman, of whom I bought an excellent mortar of *lignum vitæ*, how long he had kept the wood in ſeaſoning before I had the mortar, he anſwered me (if I much miſ-remember not) twenty years, under which time it is not fully ſeaſoned for ſome purpoſes; of which opinion of his having occaſionally ſpoken to the lately-mentioned turner, this experienced workman much confirmed me in it; as he likewiſe did in an obſervation I not long ſince made about the ſlow and unperceived motion, that may be, not only in the more looſe and fugitive aqueous parts of *lignum vitæ*, but in far more unlikely ones. For he told me, that he had often found, in turning that wood, cavities of ſeveral ſizes in the very inward and ſolid part of the wood (which every way encompaſſed them) and in thoſe cavities conſiderable quantities of a certain gum, much cried up by ſome for an anti-venereal medicine.

THE uſe I would make of theſe examples is this, that ſince ſo ſolid a body as a trunk of *lignum vitæ* is, when the tree is newly felled, may require ſo long a time as twenty years, or upwards, to be ſeaſoned, *i. e.* brought to its full compactneſs and toughneſs: and ſince the account, upon which time ſeaſons wood, ſeems to be this, that by degrees the looſer aqueous, and more fugitive parts exhale into the air, whereby the remaining ſolid ones are brought into a cloſer order, and have leiſure to be ſo placed among themſelves, as is moſt convenient to make their texture firm and durable; it will follow, that even in the internal parts of this ſolid wood there muſt be, not only in the looſer and lighter corpuſcles, that extricate themſelves, and exale away, a true local motion, though much too ſlow to fall immediately under the diſcernment of our ſenſes. And, if the lately mentioned gum be either totally, or ſo much as in part generated, as to ſenſe, after the felling of the tree, as ſome analogous inſtances, that I have elſewhere taken notice of, make it probable; then the example will further be conſiderable to our preſent purpoſe, by ſhewing, that a ſubſtance ſo groſs, and ſo little volatile in compariſon of the aqueous parts, as is the brittle gum I ſpeak of, may permeate, to a great thickneſs, a very ſolid and inanimate ſubſtance; which cannot be done without an inteſtine, though inſenſible motion among the parts of the wood, and probably a marvellouſly ſlow motion of thoſe of the gum.

SECT. VII.

But it will poſſibly ſeem more ſtrange, that very thin pieces of wood, and thoſe ſawed off from a tree of a much looſer texture, ſhould be much longer in ſeaſoning, than that ſolid and ponderous wood we have been ſpeaking of. And indeed this diſcovery is not to be made, as in *lignum vitæ*, by the brittleneſs, or other obvious qualities in the wood, but by a ſubtiler way: and accordingly having purpoſely conſulted with the makers of muſical inſtruments, and with ſome antient muſicians, I was much confirmed by them in my opinion: and I remember, the laſt maker of viols, lutes, *&c.* of whom I inquired of what age he thought ſuch inſtruments, eſpecially lutes, ought to be, to attain their full and beſt ſeaſoning for ſonorouſneſs; he replied, that in ſome of them twenty years would be requiſite, in others forty, according to the nature and thickneſs of the wood, and other circumſtances. But an antient muſician, that was preſent at what was ſaid, informed me, that there were ſome famous lutes, one or two of which he named to me, that attained not their full ſeaſoning and beſt reſonance, till they were about fourſcore years old. And thus much for inanimate vegetable ſubſtances.

SECT. VIII.

As for the calcined ſtones made up into lime, and ſorts of raw ſtones, I have already obſerved from the credible relations of maſons and others, that the walls in ſome buildings attain not their hardneſs and ſolidity, till they are forty years old, or perhaps much antienter; and ſince in gradually proceeding to this degree of ſolidity, theſe walls reſemble the ſeaſoning of *lignum vitæ* formerly explicated, the motion of the internal parts may be argued from the change of texture as well in theſe as in that.

And, if I would rob other tracts (to which they more properly belong) I could here eaſily add ſome ſuch inſtances of the hardening and ſoftening of ſtones by time, as would much confirm what I have now been delivering; but I ſhall rather chuſe to confine myſelf here to the two examples following, not taken notice of in quarries or by maſons.

The firſt is, that there are marchaſites, conſiſting as well of a ſtony as of a metalline ſubſtance, which, though harder than many other ſorts of ſtones, and even than marble, have yet ſo great a motion in their internal parts, that if they be expoſed to the air, not only they will have a vitriolate effloreſcence, if I may ſo ſpeak, on their ſuperficies (as I have obſerved in divers other marchaſites) but they will in tract of time burſt the ſtone in pieces; of which ſort I had ſome time ſince, and I hope I have yet a bulky marchaſite, that I procured from a virtuoſo, that lives juſt by a vitriol-work, whither theſe among other vitriol-ſtones are brought, and where this ſtone being choſen for its largeneſs, was taken up, and carefully kept by that ingenious perſon, till it burſt of itſelf, and till I ſent for it. And to ſatisfy myſelf a little further, that the internal parts of marchaſites may be as well diſpoſed to be vitriolated as the external; I remember I broke a hard marchaſite, that I had from another place, and laying it ſome ſhort time in a chamber-window, I found the new ſuperficies made by the fracture about the middle of the ſtone to have acquired an effloreſcence of a vitriolate nature.

The other inſtance, which is very odd, and much talked of, is this: an ingenious gentleman of my acquaintance, caſually meeting me one day, told me, that he had a turquois-ſtone, which, if he were not miſtaken, had a wonderful property; for there being in it ſeveral ſpots of colours differing from the reſt of the gem, theſe ſpots ſeemed, though very ſlowly, to move from one part of the ſtone to the other. And this he thought himſelf to have taken notice of for very many months (perhaps a couple of years.) This relation ſeemed ſo ſtrange, that the relator was not at all ſurprized, when to aſcertain myſelf of the truth of it, I deſired to have the ring, this ſtone was ſet in, for a while in my own keeping; to which he readily aſſenting, beſides that I took very heedful notice of the ſituation of the ſpots, I employed a very ingenious youth, that then lived with me, and was ſkilled in drawing, to make the picture of the ſtone with the ſpots as they were then placed, and afterwards to have a watchful eye upon it, and from time to time (as once perhaps in two or three weeks) to draw a new picture of them; by comparing of ſeveral of which pictures, it was unanimouſly concluded, that the ſpots did ſhift places in the turquois, as if the matter they conſiſted of made its way through the ſubſtance of the ſtone; as we lately noted, that the gum of *lignum vitæ* ſeemed to do through the ſubſtance of the wood. And as far as we obſerved, the motion of theſe ſpots was exceeding ſlow and irregular, though perhaps it might have been reduced to a ſomewhat leſs uncertainty, were it not, that by an unwelcome accident we were deprived of the opportunity of continuing our obſervations long enough. And this brings into my mind, that the turquois being a ſtone, of which I had met ſtrange ſtories in good authors, I once aſked ſeveral queſtions about it of a noted jeweller; and inquiring, among other particulars, whether he had not obſerved ſome changes, that ſeemed ſpontaneous in the ſubſtance or colour of the ſtone? he replied, that in ſome few turquoiſes he had obſerved two differing blues in differing parts of the ſame ſtone, and that one of thoſe colours would by ſlow and unperceived degrees invade, and at length overſpread that part of the ſtone, which the other colour poſſeſſed before. I ſhall here add, that the ſame gentleman, that lent me the ſpotted turquois, ſhewed me afterward an agate haft of a knife, where was a certain cloud, which he told me an ingenious perſon had for ſome years obſerved to remove to and fro in the ſtone, and had a while ſince, to convince the relator, lent him the agate, of whoſe phænomena he promiſed me an account, when he ſhall have had the ſtone in his cuſtody for a competent time; till the expiration of which, it may ſuffice to have ſaid of this agate what I have now related.

SECT. IX.

But becauſe that diamonds and glaſs are generally looked upon, eſpecially by chymiſts, as bodies of the cloſeſt and firmeſt texture, that nature and art afford, if we could ſhew an inteſtine motion even in the parts of theſe, fitter inſtances for our purpoſe could not in reaſon be deſired; I ſhall venture to ſay ſomething of each of them, though what I have to ſay about diamonds, is propoſed rather to ground a ſuſpicion of what may be, than a demonſtration, that it muſt be.

In the firſt place then, to remove that prejudice, that may be entertained from the incomparable hardneſs of diamonds (which I confeſs experience has made me admire) as if bodies ſo hard and ſolid could not have their parts put into motion but by ſome extraordinary, not to ſay, prodigious force; I ſhall only repeat here, what I have elſewhere ſhewn, that diamonds are bodies, that eaſily enough become actually elec-

SECT. VII.

But it will poſſibly ſeem more ſtrange, that very thin pieces of wood, and thoſe ſawed off from a tree of a much looſer texture, ſhould be much longer in ſeaſoning, than that ſolid and ponderous wood we have been ſpeaking of. And indeed this diſcovery is not to be made, as in *lignum vitæ*, by the brittleneſs, or other obvious qualities in the wood, but by a ſubtiler way: and accordingly having purpoſely conſulted with the makers of muſical inſtruments, and with ſome antient muſicians, I was much confirmed by them in my opinion: and I remember, the laſt maker of viols, lutes, *&c.* of whom I inquired of what age he thought ſuch inſtruments, eſpecially lutes, ought to be, to attain their full and beſt ſeaſoning for ſonorouſneſs; he replied, that in ſome of them twenty years would be requiſite, in others forty, according to the nature and thickneſs of the wood, and other circumſtances. But an antient muſician, that was preſent at what was ſaid, informed me, that there were ſome famous lutes, one or two of which he named to me, that attained not their full ſeaſoning and beſt reſonance, till they were about fourſcore years old. And thus much for inanimate vegetable ſubſtances.

SECT. VIII.

As for the calcined ſtones made up into lime, and ſorts of raw ſtones, I have already obſerved from the credible relations of maſons and others, that the walls in ſome buildings attain not their hardneſs and ſolidity, till they are forty years old, or perhaps much antienter; and ſince in gradually proceeding to this degree of ſolidity, theſe walls reſemble the ſeaſoning of *lignum vitæ* formerly explicated, the motion of the internal parts may be argued from the change of texture as well in theſe as in that.

And, if I would rob other tracts (to which they more properly belong) I could here eaſily add ſome ſuch inſtances of the hardening and ſoftening of ſtones by time, as would much confirm what I have now been delivering; but I ſhall rather chuſe to confine myſelf here to the two examples following, not taken notice of in quarries or by maſons.

The firſt is, that there are marchaſites, conſiſting as well of a ſtony as of a metalline ſubſtance, which, though harder than many other ſorts of ſtones, and even than marble, have yet ſo great a motion in their internal parts, that if they be expoſed to the air, not only they will have a vitriolate effloreſcence, if I may ſo ſpeak, on their ſuperficies (as I have obſerved in divers other marchaſites) but they will in tract of time burſt the ſtone in pieces; of which ſort I had ſome time ſince, and I hope I have yet a bulky marchaſite, that I procured from a virtuoſo, that lives juſt by a vitriol-work, whither theſe among other vitriol-ſtones are brought, and where this ſtone being choſen for its largeneſs, was taken up, and carefully kept by that ingenious perſon, till it burſt of itſelf, and till I ſent for it. And to ſatisfy myſelf a little further, that the internal parts of marchaſites may be as well diſpoſed to be vitriolated as the external; I remember I broke a hard marchaſite, that I had from another place, and laying it ſome ſhort time in a chamber-window, I found the new ſuperficies made by the fracture about the middle of the ſtone to have acquired an effloreſcence of a vitriolate nature.

THE other inſtance, which is very odd, and much talked of, is this: an ingenious gentleman of my acquaintance, caſually meeting me one day, told me, that he had a turquois-ſtone, which, if he were not miſtaken, had a wonderful property; for there being in it ſeveral ſpots of colours differing from the reſt of the gem, theſe ſpots ſeemed, though very ſlowly, to move from one part of the ſtone to the other. And this he thought himſelf to have taken notice of for very many months (perhaps a couple of years.) This relation ſeemed ſo ſtrange, that the relator was not at all ſurprized, when to aſcertain myſelf of the truth of it, I deſired to have the ring, this ſtone was ſet in, for a while in my own keeping; to which he readily aſſenting, beſides that I took very heedful notice of the ſituation of the ſpots, I employed a very ingenious youth, that then lived with me, and was ſkilled in drawing, to make the picture of the ſtone with the ſpots as they were then placed, and afterwards to have a watchful eye upon it, and from time to time (as once perhaps in two or three weeks) to draw a new picture of them; by comparing of ſeveral of which pictures, it was unanimouſly concluded, that the ſpots did ſhift places in the turquois, as if the matter they conſiſted of made its way through the ſubſtance of the ſtone; as we lately noted, that the gum of *lignum vitæ* ſeemed to do through the ſubſtance of the wood. And as far as we obſerved, the motion of theſe ſpots was exceeding ſlow and irregular, though perhaps it might have been reduced to a ſomewhat leſs uncertainty, were it not, that by an unwelcome accident we were deprived of the opportunity of continuing our obſervations long enough. And this brings into my mind, that the turquois being a ſtone, of which I had met ſtrange ſtories in good authors, I once aſked ſeveral queſtions about it of a noted jeweller; and inquiring, among other particulars, whether he had not obſerved ſome changes, that ſeemed ſpontaneous in the ſubſtance or colour of the ſtone? he replied, that in ſome few turquoiſes he had obſerved two differing blues in differing parts of the ſame ſtone, and that one of thoſe colours would by ſlow and unperceived degrees invade, and at length overſpread that part of the ſtone, which the other colour poſſeſſed before. I ſhall here add, that the ſame gentleman, that lent me the ſpotted turquois, ſhewed me afterward an agate haft of a knife, where was a certain cloud, which he told me an ingenious perſon had for ſome years obſerved to remove to and fro in the ſtone, and had a while ſince, to convince the relator, lent him the agate, of whoſe phænomena he promiſed me an account, when he ſhall have had the ſtone in his cuſtody for a competent time; till the expiration of which, it may ſuffice to have ſaid of this agate what I have now related.

SECT. IX.

BUT becauſe that diamonds and glaſs are generally looked upon, eſpecially by chymiſts, as bodies of the cloſeſt and firmeſt texture, that nature and art afford, if we could ſhew an inteſtine motion even in the parts of theſe, fitter inſtances for our purpoſe could not in reaſon be deſired; I ſhall venture to ſay ſomething of each of them, though what I have to ſay about diamonds, is propoſed rather to ground a ſuſpicion of what may be, than a demonſtration, that it muſt be.

IN the firſt place then, to remove that prejudice, that may be entertained from the incomparable hardneſs of diamonds (which I confeſs experience has made me admire) as if bodies ſo hard and ſolid could not have their parts put into motion but by ſome extraordinary, not to ſay, prodigious force; I ſhall only repeat here, what I have elſewhere ſhewn, that diamonds are bodies, that eaſily enough become actually electrical, and that ſome diamonds (of which ſort I have a ſmall one by me) will by rubbing upon a cloth be brought to ſhine in the dark; the quiſt of both theſe tranſient qualities requiring a change of texture even in the internal parts; and the friction that produces that change, doing it immediately by putting the parts of the ſtone into local motion, it may be thence argued, that a very moderate force may ſuffice to beget an internal motion in the inward particles of diamonds themſelves.

AND I am not ſure, but that more hidden agents may make impreſſions upon theſe hardeſt bodies. For in a ring, that I am wont to wear on my little finger, which has no diamond, ſave one more than that ſhining one I lately mentioned, I have, I know not how often, ſeemed to myſelf to obſerve a manifeſtly greater clearneſs and ſparklingneſs at ſome times than at others, though I could not refer it to the clearneſs or dulneſs of the weather, the moiſture or dryneſs of the air, the ſuperficial clearneſs or foulneſs of the ſtone, or any other manifeſt cauſe I could think of. And in this I was the more inclined to think I might not be miſtaken, becauſe, beſides that the notice I took of it, was frequent, I have by me a rough diamond juſt as it came from the rock, in whoſe electrical faculty I have taken notice of changes as to the degree of ſtrength, wherewith it attracted, and that within the compaſs of a very little time, though I could not find any cauſe, whereto I could refer ſo ſurpriſing a phænomenon. And I muſt not here omit, that chancing one day to ſhew the newly mentioned diamond ring to a very ingenious lady, that uſed to wear in rings and bracelets ſtore of thoſe gems, and telling her what changes I had taken notice of in the diamonds; ſhe, who had obſerved more about gems than any lady I had yet met with, appeared but little ſurprized at what I told her, and affirmed to me, that ſhe had divers times obſerved the like alterations in ſome diamonds of hers, which ſometimes would look more ſparklingly than they were wont, and ſometimes far more dull than ordinary. And when I objected, that poſſibly that dulneſs might be imputed to the weather, or ſome caſual foulneſs of the ſurface of the ſtone, ſhe replied, that ſhe had been aware of thoſe circumſtances, rubbing the ſtones clean, and otherwiſe taking care to ſecure an obſervation, which ſhe had made too often to have deceived herſelf in it. If I remember aright, I have elſewhere mentioned how I ſaw a conſiderable, but cloudy, *Hungarian* diamond, which the owner would have preſented me, made clearer by lying for ſome time in a cold liquor; wherein he affirmed, that upon his keeping it longer the ſtone would loſe more and more of its cloudineſs: and what I myſelf ſaw ſufficed me to argue, that changes may be produced even in the inward parts of ſuch diamonds by agents, that act without any appearing violence.

SECT. X.

AND if it be true, that diamonds, as I elſewhere obſerve about many other ſtones, may be generated from time to time in the bowels of the earth, it may not perhaps be abſurd to imagine, that even true diamonds, that ſeem perfect, and are fit for rings, may long continue to have an inſenſible motion through the whole ſtone, whereby the corpuſcles it conſiſts of are diſpoſed into a more convenient texture for the conſtituting of an extremely hard body. For though it be taken for granted, that adamantine bodies, becauſe they are generally exceeding hard, are equally ſo, yet that ſuppoſition is not favoured by experience. And I remember, that to ſatisfy myſelf further about ſuch matters, I repaired to an antient artificer eminent in his trade, which was the cutting and ſetting of diamonds; and that having demanded of him, whether he found, that all diamonds were of equal hardneſs; he anſwered me, that having dealt in theſe gems near twenty years in *Amſterdam*, and divers years in *England*, he perceived, that there are of later years brought over worſe and worſe ſorts of diamonds; and that he finds ſeveral of the recent diamonds ſo ſoft and brittle in compariſon of thoſe he was antiently wont to ſet (and which he with other jewellers called diamonds of the old rock) that he is often afraid, and unwilling to meddle with them, when they are brought to him, leſt he ſhould ſpoil them in the cutting, or poliſhing. But this I only repeat hiſtorically, till further obſervation ſhall diſcover, whether theſe are diamonds not yet fully ripe, and capable of growing harder by further maturation; or whether they be of a peculiar ſort of diamonds, whoſe nature it is to be always ſofter than thoſe of the old rock.

SECT. XI.

THIS brings into my mind a confirmation of the unequal hardneſs of diamonds, whatever be the cauſe of it, which I met with in a little book, lately publiſhed in his own language by a Frenchman, who giving his reader an account of the eaſtern diamond-mines from the relation, as he affirms, of a late eye-witneſs, ſpeaks thus of the third and laſt mine called *Gazerpoli: They are very clear, and of a good water, but they cannot be ground by mutual attrition* (if I underſtand the term he uſes) *but with ſtones of the ſame mine; for if one ſhould employ for that purpoſe the ſtones of another mine, thoſe of* Gazerpoli *would be broken in pieces. They do alſo eaſily break upon the wheel, and thoſe, that are not verſed in the knowledge of ſtones, may eaſily be deceived* (in them.) Of which our author adds the example of a Portugueſe, who refuſing 1200 crowns for one of them at *Leghorn*, when he went to have it cut at *Venice*, it broke upon the wheel into fifteen or twenty pieces.

Egrezes, pag. m. 17, 18.

ANOTHER example, that ſeems to make more for our preſent purpoſe, is afforded by the ſame author, ſpeaking of the ſecond mine, which breeds the greateſt ſtones called gane or colonor; for he ſays, that *ſur la plus part, i. e.* upon the moſt of theſe ſtones after they are cut, there appears always, as it were, a kind of greaſineſs or unctuoſity, which invites you ever and anon to have recourſe to your handkerchief to wipe it off, which one would gueſs to proceed from ſome inſenſible effluvium, that exhaling out of the ſtone, comes to be checked and condenſed by the air on the ſuperficies of it, as it happens to ſweat on the ſkins of animals: the truth of which conjecture I would examine by very nice ſcales, if I could procure ſuch diamonds.

SECT. XII.

AND becauſe rubies, though inferior in hardneſs to diamonds, are yet harder than moſt other gems, and much more than marble and the like coarſer ſtones, I will not omit, on this occaſion, what was more than once affirmed to me by an obſerving lady, whom, if ſhe were not too nearly related to me, I could ſcarce mention without an elogy. For caſually caſting my eye upon a fair ruby ſhe wore upon her finger, and deſiring to conſider it more attentively, ſhe pulled off the rich ring it was ſet in, and reaching it me, told me it was worth my curioſity to conſider it. For beſides that it was ſo fine a ſtone, that it was thought worth being left her as a legacy by a great lady (her dear friend) that was famous, as I knew, for the variety of the rich jewels ſhe was miſtreſs of; this ruby would not unfrequently vary the degrees of its luſtre, ſhe knew not why. For ſometimes it ſeemed to be ennobled by a more vivid fire than ordinary, and at other times it would be manifeſtly more dull and cloudy than it is wont to be: and this not imputable, as ſhe expreſsly aſſured me upon repeated obſervations, to the cloudineſs of the weather, or any ſuperficial foulneſs of the ſtone. And, that I might be convinced as well as ſhe herſelf was, ſhe deſired me to rub it very clean, and then take notice of the preſent luſtre of it, of which ere long ſhe preſumed ſhe could ſhew me a manifeſt alteration (for I was then come to viſit her, and paſs ſome weeks with her in her houſe) but my occaſions calling me away within a few days after, I had not time to wait for the event of her promiſe.

How far what has been ſaid concerning diamonds may be allowed to be argumentative towards the ſcope of this diſcourſe, I ſhall willingly leave to the diſcovery of time, and further obſervation; the mention I have made of the foregoing particulars having been invited partly by the nobleneſs of the ſubject, which made me willing to add theſe relations to what I have elſewhere written about them; and partly becauſe thus much at leaſt ſeems probably deducible from what I noted about the exciting of diamonds by rubbing, both to attract, and to ſhine, that notwithſtanding their incomparable hardneſs, an inteſtine motion of their minute parts may be without any conſiderable violence quickly produced.

SECT. XIII.

And now it will be time to conſider the other body I promiſed to take notice of, namely, glaſs. For this being thought ſo compact and firm a body, that it is indeſtructable by art or nature, and being alſo of ſo cloſe a texture, that the ſubtleſt chymical ſpirits (that are yet known) cannot pervade it; and laſtly, having given ſuch proofs of the fixedneſs of its parts, as to have long indured the violence even of a glaſs-houſe-fire, we can ſcarce imagine a body more unlikely to have any motion amongſt its component particles: and yet, that they may not be always at perfect reſt among themſelves, I have been induced to think by the following, and the like obſervations.

First, having inquired of a famous and experienced maker of teleſcopes, as well as of thoſe, that uſe ſuch inſtruments, whether he did not obſerve, that the Venice-glaſſes he employed would ſometimes crack of themſelves, whilſt they were yet in plates, and ſometimes do the like after they were ground into convexes, and poliſhed up; he anſwered affirmatively. And though it ſeemed improbable, that glaſſes brought ſo far off as from *Venice*, and many of them kept a good while here in *England* before there be occaſion to grind them, and perhaps longer after their having been ground before they crack, ſhould after all this time retain an internal motion among their component particles; yet I have been induced to conjecture, that ſome ſaline corpuſcles, more numerous than the nature of the glaſs required, may by degrees, though ſlow and unperceived, ſo tend towards the ſuperficies of the glaſs, as either to get out of the pores of it, or crack, or burſt the glaſs in endeavouring to force their paſſage outward. For having purpoſely inquired of the abovenamed artificer, and ſome other obſerving men, that deal in optical glaſſes, whether it had not been taken notice of, that there would ſometimes be, eſpecially in winter and very moiſt weather, a kind of effloreſcence of a ſaltiſh taſte manifeſtly diſcernable upon the ſurfaces of their glaſſes; I was anſwered in the affirmative, eſpecially by the above-mentioned artificer, who having more occaſion and opportunity to take notice of ſuch things, told me, that he had by taſting found theſe exſudations ſenſibly ſaltiſh.

SECT. XIV.

AND I was the more apt to entertain the lately propoſed conjecture, becauſe of a thick glaſs cup, that I have yet by me, in the making of which, to render it the more diaphanous, I ſuppoſed an over-great proportion of ſalt had been employed. For this cup, though for a while it continued clear and intire, yet before the enſuing winter was ended, though it did not ſo crack as to fall to pieces, but ſtill retains its former ſhape, yet it was flawed with ſuch a multitude of little cracks, that at a diſtance it looks like a white, not like a cryſtalline cup.

SECT. XV.

I REMEMBER alſo, that I have ſometimes, though not often, had veſſels and other bodies of glaſs of a conſiderable thickneſs, which have of their own accord broken ſuddenly aſunder, with noiſe enough to make me take notice of it. And that this did not always happen for want of the glaſs's being gradually or ſlowly cooled, or, in the workmen's language, nealed; I was perſuaded, not only by the ſpontaneous cracking, not without a loud noiſe of a thick and empty glaſs veſſel, that had for I know not how many months been kept locked up in my ſtudy; but by the like accidents, which I after found had happened unto others. For inquiring of ſome, that made great ſtore of glaſs veſſels, as well as of others that ſold them; I learned from both, that they had ſometimes by their loſſes been made to take notice, that glaſſes, that had been long made, and kept unemployed, would break of themſelves, when there was no viſible outward agent near enough to be ſuſpected of the having broken them. And ſince this very page began to be written, I had a fair cryſtal phial, not too well ſtopped, which cracked at the thick bottom, in a glaſs-cabinet (that was fixed to a wall) where I kept that with other choice phials under lock and key; no other of the included glaſſes (full nor empty) nor yet of the external glaſſes appearing any way cracked or injured. Nay, even great and ſtrong looking-glaſſes are not quite exempted from theſe accidents. For I remember, that having purpoſely inquired of an honeſt man, that furniſhed the greateſt part of *London* with large looking glaſſes, whether he did not ſometimes find them crack, and that with noiſe; he ſhewed me divers large plates of excellent glaſs, and aſſured me, that ſometimes after they had been a good while in his ſhop, ſome of them would of themſelves, not only crack with a loud noiſe, but now and then alſo (though rarely) fly aſunder with that violence, as to break ſome of the neighbouring plates, though thick and ſtrong.

SECT. XVI.

AND having alſo a mind to inquire further, whether this diſpoſition to break, in ſome ſorts of glaſs, might not continue much longer than I had opportunity to obſerve, I addreſſed myſelf to an ingenious maſter of a glaſs-houſe, and demanded of him, how long he had taken notice of glaſs to continue ſound and whole, and yet afterwards to break of itſelf. He replied, that he had once a great parcel of glaſſes packed up, which not having the occaſion he expected to vend and make uſe of, lay by him for a great while; and yet when afterwards he had unpacked them, and ranged them, in a ſhort time a great many of them, amounting to about a fourth or third part of the whole number, cracked of themſelves; and when I aſked, how long the parcels had lain by, before they were opened, he replied, that it was, as he remembered, between four and five years.

SECT. XVII.

These inſtances (to which I could add divers others) I have therefore mentioned, becauſe either of the two hypotheſes, in congruity whereunto they ſeem likelieſt to be intelligibly explicable, will favour the doctrine hitherto patronized. For according to the atomical theory, it may be conceived, that there is a conſtant inteſtine motion of the ſmall parts of the glaſs upon the ſcore of their conſtituent atoms, which endeavour or tend to extricate themſelves and get away, which at laſt they do, by breaking the glaſs in ſome brittle, or other fit place; where (after a multitude of encounters and evolutions) a competent number of them may happen to be got together, and find their motion (outwards) withſtood: whence may enſue ſo unequal an agitation there of the formerly coherent parts of the glaſs, as to make the more agitated ones part from them, that are leſs ſo; and conſequently crack the glaſs. To which agrees what I have often obſerved in chymical and mechanical trials made with glaſs-veſſels, that if there be any grain of ſand or gravel, or any little lump of the alkalizate matter glaſs is made of, conſpicuouſly incloſed in the ſubſtance of the veſſel, it will both be much the more apt to break, and, if it do, will almoſt always begin to crack at that place (whence uſually as from a center ſeveral cracks go ſeveral ways;) the part of the glaſs, where the blemiſh is, being commonly of a differing texture from the reſt (as is often manifeſt to the very eye) and being by that incongruous texture diſpoſed to be put into a motion differing from, and perhaps very diſproportionate to that of the neighbouring parts.

SECT. XVIII.

I must not here ſtay to examine, whether or no this motion of the internal parts may not (in divers caſes) be made more efficacious by the penetration of ſome ſubtile and moiſt matter into the glaſs's pores (eſpecially the more ſuperficial ones of ſome glaſs of a looſer ſort) and ſo by degrees vitiate the texture of the body, and promoting the agitation and ſwelling of the ſaline corpuſcles, enable them to burſt the glaſs, after ſome ſuch manner, as the marchaſites I lately mentioned, came to have a vitriolate efflореſcence, and even to be burſt by the operation of the air; this, I ſay, I muſt not now ſtay to examine, becauſe I would haſten to propoſe the ſecond hypotheſis, and tell you, that (elſe) we may, congruouſly to what we elſewhere diſcourſe, imagine, that in tract of time, there is produced in ſome parts of a glaſs a texture, that makes it reſiſt more than it did formerly the free paſſage of the æther, or ſome other ſubtile matter, that was accuſtomed (perhaps ſtream like) to permeate it before; which tranſient matter now finding its paſſage obſtructed (and, perchance, almoſt quite hindered) by the vitiating of the pores of the glaſs, or ſome other (inconvenient) change of texture in it, and endeavouring to continue its wonted motion through it, does ſo ſtretch the pores, or otherwiſe offer violence to the texture of the body, that it cauſes a divulſion in the parts, which, according as it is more or leſs forcibly or ſuddenly made, does either barely crack the glaſs, or make it fly aſunder. To the precedent doctrine theſe two things agree not ill: the firſt, that glaſs is a body eaſily made electrical by rubbing, which makes it probable, that its particles may eaſily be put into motion. And the ſecond, that ſuch a divulſion may be made in glaſs by but an inequal motion between the neighbouring parts; as may appear by the chymical practice of cracking glaſſes, which they often think fit to do, only by applying a red-hot iron to the place, till it be ſufficiently heated, and whilſt it is very hot, moiſtening it with cold water (or even ſpittle) which by cooling the part, that it touched, and conſequently checking the agitation of the corpuſcles it meets there, whilſt the contiguous parts retain their former vehement agitation, occaſions a diſcontinuity or divulſion in the glaſs, ſome of whoſe parts are in ſo ſwift, and others are in ſo ſlow a motion.

SECT. XIX.

And on this occaſion I ſhall add chiefly, becauſe I would not pretermit ſo conſiderable a phænomenon, that even when glaſs ſeems to have loſt the degree of heat, that one would think neceſſary to have its ſhape or bigneſs ſenſibly altered, there may remain yet ſo much agitation in the minute parts, as, when they are modified by the air, by the cold, or by ſome other inviſible agent, to make them alter the bulk or ſhape of the whole veſſel they compoſe; and that (which one would not expect) by enlarging the veſſel, at leaſt if we allow not in the caſe any change of figure: for it has often been obſerved in thoſe glaſs-houſes, where they work white glaſſes, as they call thoſe, that are pure and clear, that when they have blown glaſſes into a mold, to give them more exactly the deſired ſhape and ſize, theſe glaſſes, when they are cold, cannot well be put again into the ſame mold they were blown in, and require, that the cells of garde-phials, that are to receive them, be made a little larger: which obſervation an eminent artificer of my acquaintance, that gets conſiderably by fitting fine glaſſes to caſes, has much confirmed to me by his complaints of the inconvenience, eaſy to be incurred, by the not knowing, or not remembering ſo unlikely an effect of the cooling of glaſs. But I muſt not proſecute the conſideration of theſe, and the like phænomena, nor examine, which of the preceding explications is preferable, having mentioned them, as I was ſaying, chiefly to ſhew, that, which of them ſoever we pitch upon, it will argue an inteſtine motion in the corpuſcles of the glaſs, which motion we ſhall think may be very ſlow, if we conſider how long a time it is on ſome occaſions, in producing its effects, before it brings them to be diſcernable to our ſenſes.

SECT. XX.

Having thus made it probable, that among the parts of ſuch ſolid bodies, as I have hitherto inſtanced in, there may not be ſuch a perfect reſt, as is generally believed; it will, I ſuppoſe, be expected, that I ſhould now draw this conſequence from what has been ſaid, that there is no ſuch thing as abſolute reſt in nature. But ſince at my firſt mentioning this paradox to you, I propoſed it but as problematical, and ſince I conſider, that we are not yet ſure, but that though many of the parts of ſolid bodies may not be always moveleſs, yet ſome others of them may ſometimes, for a while at leaſt, be at perfect reſt: I ſhall conclude, as I began, and without reſolutely denying, that there can be any ſuch thing *in rerum naturâ*, as abſolute reſt, I ſhall content myſelf to ſay, that it is not either abſurd to doubt, whether there be or no; nor improbable to think, that there is not, ſince we have not found it in thoſe very bodies, where, with the greateſt likelihood, it might have been expected.

THE
SCEPTICAL CHYMIST:
OR
Chymico-Phyſical Doubts and Paradoxes,
TOUCHING THE
EXPERIMENTS,
Whereby vulgar Spagyriſts are wont to endeavour to evince their Salt, Sulphur and Mercury, to be the true Principles of Things.

To which, in this Edition, are ſubjoined divers Experiments and Notes about the Producibleneſs of Chymical Principles.

A PREFACE INTRODUCTORY to the following Treatiſe.

TO give the reader an account, why the following treatiſe is ſuffered to paſs abroad ſo maimed and imperfect, I muſt inform him, that it is now long ſince, that to gratify an ingenious gentleman, I ſet down ſome of the reaſons, that kept me from fully acquieſcing either in the peripatetical, or in the chymical doctrine of the material principles of mixed bodies. This diſcourſe, ſome years after, falling into the hands of ſome learned men, had the good luck to be ſo favourably received, and advantageouſly ſpoken of by them, that having had more than ordinary invitations given me to make it public, I thought fit to review it, that I might retrench ſome things, that ſeemed not ſo fit to be ſhewn to every reader, and ſubſtitute ſome of thoſe other things, that occurred to me of the trials and obſervations I had ſince made. What became of my papers, I elſewhere mention in a preface, where I complain of it; but ſince I writ that, I found many ſheets, that belonged to the ſubjects I am now about to diſcourſe of. Wherefore, ſeeing that I had then in my hands as much of the firſt Dialogue, as was requiſite to ſtate the caſe, and ſerve for an Introduction as well to the Conference betwixt *Carneadès* and *Eleutherius*, as to ſome other Dialogues, which for certain reaſons are not herewith publiſhed, I reſolved to ſupply, as well as I could, the contents of a paper belonging to the ſecond of the following diſcourſes, which I could not poſſibly retrieve, though it were the chief of them all. And having once more tried the opinion of friends, but not the ſame, about this imperfect work, I found it ſuch, that I was content, in compliance with their deſires, that not only it ſhould be publiſhed, but that it ſhould be publiſhed as ſoon as conveniently might be. I had, indeed, all along the Dialogues ſpoken of myſelf, as of a third perſon; for they containing diſcourſes, which were among the firſt treatiſes, that I ventured long ago to write of matters philoſophical, I had reaſon to deſire, with the painter, to *latere pone tabulam*, and hear what men would ſay of them, before I owned myſelf to be their author. But, beſides, that now I find, it is not unknown to many, who it is, that writ them, I am made to believe, that it is not inexpedient, they ſhould be known to come from a perſon altogether a ſtranger to chymical affairs. And I made the leſs ſcruple to let them come abroad uncompleated, partly, becauſe my affairs and pre-ingagements to publiſh divers other treatiſes, allowed me ſmall hopes of being able in a great while to compleat thoſe Dialogues. And partly becauſe I am not unapt to think, that they may come abroad ſeaſonably enough, though not for the author's reputation, yet for other purpoſes. For I obſerve, that of late chymiſtry begins, as indeed it deſerves, to be cultivated by learned men, who before deſpiſed it; and to be pretended to by many, who never cultivated it, that they may be thought not to be ignorant of it: whence it is come to paſs, that divers chymical notions about matters philoſophical are taken for granted and employed, and ſo adopted by very eminent writers both naturaliſts and phyſicians. Now this, I fear, may prove ſomewhat prejudicial to the advancement of ſolid philoſophy: for though I am a great lover of chymical experiments, and though I have no mean eſteem of divers chymical remedies, yet I diſtinguiſh theſe from their notions about the cauſes of things and their manner of generation. And, for aught I can hitherto diſcern, there are a thouſand phænomena in nature, beſides a multitude of accidents relating to the human body, which will ſcarcely be clearly and ſatisfactorily made out by them, that confine themſelves to deduce things from ſalt, ſulphur and mercury, and the other notions peculiar to the chymiſts, without taking much more notice than they are wont to do, of the motions and figures of the ſmall parts of matter, and the other more catholic and fruitful affections of bodies. Wherefore it will not, perhaps, be now unſeaſonable to let our *Carneades* warn men, not to ſubſcribe to the grand doctrine of the chymiſts, touching their three hypoſtatical principles, till they have a little examined it, and conſidered, how they can clear it from his objections, divers of which, it is like, they may never have thought on; ſince a chymiſt ſcarce would, and none but a chymiſt could propoſe them. I hope alſo, it will not be unacceptable to ſeveral ingenious perſons, who are unwilling to determine of any important controverſy, without a previous conſideration of what may be ſaid on both ſides, and yet have greater deſires to underſtand chymical matters, than opportunities of learning them, to find here together, beſides ſeveral experiments of my own, purpoſely made to illuſtrate the doctrine of the elements, divers others, ſcarce to be met with, otherwiſe than ſcattered among many chymical books; and to find theſe aſſociated experiments ſo delivered, as that an ordinary reader, if he be but acquainted with the uſual chymical terms, may eaſily enough underſtand them; and even a wary one may ſafely rely on them. Theſe things I add, becauſe a perſon, any thing verſed in the writings of chymiſts cannot but diſcern by their obſcure, ambiguous, and almoſt ænigmatical way of expreſſing what they pretend to teach, that they have no mind to be underſtood at all, but by the ſons of art (as they call them) nor to be underſtood even by theſe, without difficulty and hazardous trials. Inſomuch that ſome of them ſcarce ever ſpeak ſo candidly, as when they make uſe of that known chymical ſentence; *ubi palam locuti ſumus, ibi nihil diximus.* And, as the obſcurity of what ſome writers deliver makes it very difficult to be underſtood; ſo the unfaithfulneſs of too many others makes it unfit to be relied on. For though unwillingly, yet I muſt for the truth's ſake, and the reader's, warn him not to be forward to believe chymical experiments, when they are ſet down only by way of preſcriptions, and not of relations; that is, unleſs he, that delivers them, mentions his doing it upon his own particular knowledge, or upon the relation of ſome credible perſon, avowing it upon his own experience. For I am troubled, I muſt complain, that even eminent writers, both phyſicians and philoſophers, whom I can eaſily name, if it be required, have of late ſuffered themſelves to be ſo far impoſed upon, as to publiſh and build upon chymical experiments, which queſtionleſs they never tried; for if they had, they would, as well as I, have found them not to be true. And indeed, it were to be wiſhed, that now, that thoſe begin to quote chymical experiments, that are not themſelves acquainted with chymical operations, men would leave off that indefinite way of vouching, the chymiſts ſay this, or the chymiſts affirm that; and would rather, for each experiment they allege, name the author or authors, upon whoſe credit they relate it: for by this means they would ſecure themſelves from the ſuſpicion of falſhood (to which the other practice expoſes them) and they would leave the reader to judge of what is fit for him to believe of what is delivered, whilſt they employ not their own great names to countenance doubtful relations; and they will alſo do juſtice to the inventors or publiſhers of the true experiments, as well as upon the obtruders of falſe ones. Whereas, by that general way of quoting the chymiſts, the candid writer is defrauded of the particular praiſe, and the impoſtor eſcapes the perſonal diſgrace, that is due to him.

THE remaining part of this Preface muſt be imployed in ſaying ſomething for *Carneades*, and ſomething for myſelf.

AND firſt, *Carneades* hopes, that he will be thought to have diſputed civilly and modeſtly enough for one, that was to play the antagoniſt and the ſceptic: and if he any where ſeems to ſlight his adverſaries tenets and arguments, he is willing to have it looked upon as what he was induced to, not ſo much by his opinion of them, as the examples of *Themiſtius* and *Philoponus*, and the cuſtom of ſuch kind of diſputes.

NEXT, in caſe that ſome of his arguments ſhall not be thought of the moſt cogent ſort, that may be, he hopes it will be conſidered, that it ought not to be expected, that they ſhould be ſo. For his part being chiefly but to propoſe doubts and ſcruples, he does enough, if he ſhews, that his adverſaries arguments are not ſtrongly concluding, though his own be not ſo neither. And if there ſhould appear any diſagreement betwixt the things he delivers in divers paſſages, he hopes it will be conſidered, that it is not neceſſary, that all the things a ſceptic propoſes ſhould be conſonant; ſince it being his work to ſuggeſt doubts againſt the opinion he queſtions, it is allowable for him to propoſe two or more ſeveral hypotheſes about the ſame thing; and to ſay, that it may be accounted for this way, or that way, or the other way, though theſe ways be perhaps inconſiſtent among themſelves. Becauſe it is enough for him, if either of the propoſed hypotheſes be but as probable as that he calls in queſtion. And if he propoſe many, that are each of them probable, he does the more ratify his doubts, by making it appear the more difficult to be ſure, that that way, which they all differ from, is the true. And our *Carneades*, by holding the negative, has this advantage, that if among all the inſtances he brings to invalidate the vulgar doctrine of thoſe he diſputes with, any one be irrefragable, that alone is ſufficient to overthrow a doctrine, which univerſally aſſerts what he oppoſes. For it cannot be true that all bodies whatſoever, that are reckoned among the perfectly mixt ones, are compounded of ſuch a determinate number of ſuch or ſuch ingredients, in caſe any one ſuch body can be produced, that is not ſo compounded; and he hopes too, that accurateneſs will be the leſs expected from him, becauſe his undertaking obliges him to maintain ſuch opinions in chymiſtry, and that chiefly by chymical arguments, as are contrary to the very principles of the chymiſts, from whoſe writings it is not therefore like he ſhould receive any intentional aſſiſtance, except from ſome paſſages of the bold and ingenious *Helmont*, with whom he yet diſagrees in many things (which reduce him to explicate divers chymical phænomena, according to other notions;) and of whoſe ratiocinations, not only ſome ſeem very extravagant, but even the reſt are not wont to be as conſiderable as his experiments. And though it be true indeed, that ſome Ariſtotelians have occaſionally written againſt the chymical doctrine he oppugns, yet ſince they have done it according to their principles, and ſince our *Carneades* muſt as well oppoſe their hypotheſis as that of the Spagyriſt, he was fain to fight his adverſaries with his own weapons, thoſe of the Peripateric being improper, if not hurtful, for a perſon of his tenets; beſides that thoſe Ariſtotelians (at leaſt thoſe he met with) that have written againſt the chymiſts, ſeem to have had ſo little experimental knowledge in chymical matters, that by their frequent miſtakes and unſkilful way of oppugning, they have too often expoſed themſelves to the deriſion of their adverſaries, for writing ſo confidently againſt what they appear ſo little to underſtand.

And laſtly, *Carneades* hopes, he ſhall do the ingenious this piece of ſervice, that by having thus drawn the chymiſts doctrine out of their dark and ſmoky laboratories, and both brought it into the open light, and ſhewn the weakneſs of their proofs, that have hitherto been wont to be brought for it, either judicious men ſhall henceforth be allowed calmly and after due information to diſbelieve it, or thoſe abler chymiſts, that are zealous for the reputation of it, will be obliged to ſpeak plainer than hitherto has been done, and maintain it by better experiments and arguments than thoſe *Carneades* hath examined: ſo that he hopes the curious will one way or other derive either ſatisfaction or inſtruction from his endeavours. And as he is ready to make good the profeſſion he makes in the cloſe of his diſcourſe, of being ready to be better informed, ſo he expects either to be indeed informed, or to be let alone. For though, if any truly knowing chymiſts ſhall think fit in a civil and rational way to ſhew him any truth, touching the matter in diſpute, that he yet diſcerns not, *Carneades* will not refuſe either to admit, or to own a conviction; yet if any impertinent perſon ſhall, either to get himſelf a name, or for what other end ſoever, wilfully or careleſly miſtake the ſtate of the controverſy, or the ſenſe of his arguments, or ſhall rail inſtead of arguing, as hath been done of late, in print, by divers chymiſts *; or laſtly, ſhall write againſt them in a canting way, I mean, ſhall expreſs himſelf in ambiguous or obſcure terms, or argue from experiments not intelligibly enough delivered, *Carneades* profeſſes, that he values his time ſo much, as not to think the anſwering ſuch trifles worth the loſs of it.

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* *G.* and *F.* and *H.* and others, in their books againſt one another.

And now having ſaid thus much for *Carneades*, I hope the reader will give me leave to ſay ſomething for myſelf.

And firſt, if ſome moroſe readers ſhall find fault with my having made the interlocutors upon occaſion compliment with one another, and that I have almoſt all along written theſe Dialogues, in a ſtyle more faſhionable than that of meer ſcholars is wont to be, I hope I ſhall be excuſed by them, that ſhall conſider, that to keep a due decorum in the diſcourſes, it was fit, that in a book written by a gentleman, and wherein only gentlemen are introduced as ſpeakers, the language ſhould be more ſmooth, and the expreſſions more civil, than is uſual in the more ſcholaſtic way of writing. And indeed, I am not ſorry to have this opportunity of giving an example, how to manage even diſputes with civility; whence, perhaps, ſome readers will be aſſiſted to diſcern a difference betwixt bluntneſs of ſpeech and ſtrength of reaſon, and find, that a man may be a champion for truth, without being an enemy to civility; and may confute an opinion without railing at them that hold it; to whom, he that deſires to convince and not to provoke them, muſt make ſome amends by his civility to their perſons for his ſeverity to their miſtakes; and muſt ſay as little elſe as he can, to diſpleaſe them, when he ſays, that they are in an error.

But, perhaps, other readers will be leſs apt to find fault with the civility of my diſputants, than the chymiſts will be, upon the reading of ſome paſſages of the following Dialogue, to accuſe *Carneades* of aſperity. But, if I have made my Sceptic ſometimes ſpeak ſlightingly of the opinions he oppoſes, I hope it will not be found, that I have done any more, than became the part he was to act, of an opponent: eſpecially, if what I have made him ſay, be compared with what the prince of the Roman orators himſelf makes both great perſons and friends ſay of one another's opinions, in his excellent Dialogues, *de naturâ Deorum:* and I ſhall ſcarce be ſuſpected of partiality in the caſe, by them, that take notice, that there is full as much (if not far more) liberty of ſlighting their adverſaries tenets, to be met with in the diſcourſes of thoſe, with whom *Carneades* diſputes. Nor need I make the interlocutors ſpeak otherwiſe than freely in a Dialogue, wherein it was ſufficiently intimated, that I meant not to declare my own opinion of the arguments propoſed, much leſs of the whole controverſy itſelf, otherwiſe than as it may by an attentive reader be gueſſed at by ſome paſſages of *Carneades*; (I ſay, ſome paſſages, becauſe I make not all, that he ſays, eſpecially in the heat of diſputation, mine) partly in this diſcourſe, and partly in ſome other * Dialogues betwixt the ſame ſpeakers (though they treat not immediately of the elements) which have long lain by me, and expect the entertainment, that theſe preſent diſcourſes will meet with. And indeed they will much miſtake me, that ſhall conclude from what I now publiſh, that I am at defiance with chymiſtry, or would make my readers ſo. I hope the *ſpecimina* I have lately publiſhed of an attempt to ſhew the uſefulneſs of chymical experiments to contemplative philoſophers, will give thoſe, that read them, other thoughts of me, and I had a deſign (but wanted opportunity) to publiſh with theſe papers an eſſay I have lying by me, the greater part of which is apologetical for one ſort of chymiſts. And at leaſt, as for thoſe, that know me, I hope the pains I have taken in the fire will both convince them, that I am far from being an enemy to the chymiſts art (though I am no friend to many, that diſgrace it by profeſſing it) and perſuade them to believe me, when I declare, that I diſtinguiſh betwixt thoſe chymiſts, that are either cheats, or but laborants, and the true *adepti*; by whom, could I enjoy their converſation, I would both willingly and thankfully be inſtructed; eſpecially concerning the nature and generation of metals; and poſſibly, thoſe, that know how little I have remitted of my former addictedneſs to make chymical experiments, will eaſily believe, that one of the chief deſigns of this ſceptical diſcourse was, not ſo much to diſcredit chymiſtry, as to give an occaſion and a kind of neceſſity to the more knowing artiſts, to lay aſide a little of their over-great reſervedneſs, and either explicate or prove the chymical theory better than ordinary chymiſts have done, or, by enriching us with ſome of their nobler ſecrets, to evince, that their art is able to make amends even for the deficiencies of their theory. And thus much I ſhall make bold to add, that we ſhall much undervalue chymiſtry, if we imagine, that it cannot teach us things far more uſeful, not only to phyſic, but to philoſophy, than thoſe, that are hitherto known to vulgar chymiſts. And yet, as for inferior Spagyriſts themſelves, they have, by their labours, deſerved ſo well of the common wealth of learning, that methinks it is pity they ſhould ever miſs the truth, which they have ſo induſtriouſly ſought. And though I be no admirer of the theorical part of their art, yet my conjectures will much deceive me, if the practical part be not hereafter much more cultivated, than hitherto it has been, and do not both employ philoſophy and philoſophers, and hope to make men ſuch. Nor would I, that have been diverted by other ſtudies as well as affairs, be thought to pretend being a profound Spagyriſt, by finding ſo many faults in the doctrine, wherein the generality of chymiſts ſcruple not to acquieſce: for beſides that it is moſt commonly far eaſier to frame objections againſt any propoſed hypotheſis, than to propoſe an hypotheſis not liable to objections (beſides this, I ſay) it is no ſuch great matter, if whereas beginners in chymiſtry are commonly at once imbued with the theory and operations of their profeſſions, I, who had the good fortune to learn the operations from illiterate perſons, upon whoſe credit I was not tempted to take up any opinion about them, ſhould conſider things with leſs prejudice, and conſequently with other eyes than the generality of learners; and ſhould be more diſpoſed to accommodate the phænomena, that occurred to me, to other notions than to thoſe of the Spagyriſts. And having at firſt entertained a ſuſpicion, that the vulgar principles were leſs general and comprehenſive, or leſs conſiderately deduced from chymical operations, than was believed, it was not uneaſy for me both to take notice of divers phænomena, over-looked by prepoſſeſt perſons, that ſeemed not to ſuit ſo well with the hermetical doctrine; and to deviſe ſome experiments likely to furniſh me with objections againſt it, not known to many, that having practiſed chymiſtry longer, perchance, than I have yet lived, may have far more experience, than I, of particular proceſſes.

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* The Dialogues here meant are thoſe about heat, fire, flame, *&c.* (ſeen by two Secretaries of the Royal Society) that the author ſomewhere complains to have been miſſing, with other things of his, preſently after the haſty removal of his goods by night in the great fire of *London*.

To conclude, whether the notions I have propoſed, and the experiments I have communicated, be conſiderable, or not, I willingly leave others to judge; and this only I ſhall ſay for myſelf, that I have endeavoured to deliver matters of fact ſo faithfully, that I may as well aſſiſt the leſs ſkilful readers to examine the chymical hypotheſis, as provoke the ſpagyrical philoſophers to illuſtrate it: which if they do, and that either the chymical opinion, or the peripatetic, or any other theory of the elements differing from that I am moſt inclined to, ſhall be intelligibly explicated, and duly proved to me; what I have hitherto diſcourſed will not hinder it from making a proſelyte of a perſon, that loves fluctuation of judgment little enough to be willing to be eaſed of it by any thing but error.

Advertiſement.

THE reader is deſired to take notice, that as the date of the licence witneſſes, this book ſhould have been printed long ago, and there has been a miſtake in the bottom of the title-page, where the year 1680, has been put inſtead of the year 1679, in which it was really printed off, though not publickly expoſed to ſale, till the beginning of this month of *January* 1678-79.

PHYSIOLOGICAL CONSIDERATIONS

TOUCHING

The Experiments wont to be employed to evince, either the four Peripatetick Elements, or the three chymical Principles of mixt Bodies.

Part of the firſt Dialogue.

I PERCEIVE, that divers of my friends have thought it very ſtrange to hear me ſpeak ſo irreſolvedly, as I have been wont to do, concerning thoſe things, which ſome take to be the elements, and others to be the principles of all mixed bodies. But I bluſh not to acknowledge, that I much leſs ſcruple to confeſs that I doubt, when I do ſo, than to profeſs, that I know what I do not: and I ſhould have much ſtronger expectations than I dare yet entertain, to ſee philoſophy ſolidly eſtabliſhed, if men would more carefully diſtinguiſh thoſe things, that they know, from thoſe, that they ignore or do but think, and then explicate clearly the things they conceive they underſtand, acknowledge ingenuouſly what it is they ignore, and profeſs ſo candidly their doubts, that the induſtry of intelligent perſons might be ſet on work to make further enquiries, and the eaſineſs of leſs diſcerning men might not be impoſed on. But becauſe a more particular account will probably be expected of my unſatisfiedneſs, not only with the peripatetick, but with the chymical doctrine of the primitive ingredients of bodies; it may poſſibly ſerve to ſatisfy others of the excuſeableneſs of my diſſatisfaction, to peruſe the enſuing relation of what paſſed a while ſince at a meeting of perſons of ſeveral opinions, in a place that need not here be named; where the ſubject, whereof we have been ſpeaking, was amply and variouſly diſcourſed of.

IT was on one of the faireſt days of this ſummer, that the inquiſitive *Eleutherius* came to invite me to make a viſit with him to his friend *Carneades.* I readily conſented to this motion, telling him, that if he would but permit me to go firſt and make an excuſe at a place not far off, where I had at that hour appointed to meet, but not about a buſineſs either of moment, or that could not well admit of a delay, I would preſently wait on him, becauſe of my knowing *Carneades* to be ſo converſant with nature and with furnaces, and ſo unconfined to vulgar opinions, that he would probably, by ſome ingenious paradox or other, give our minds at leaſt a pleaſing exerciſe, and perhaps enrich them with ſome ſolid inſtruction. *Eleutherius* then firſt going with me to the place where my apology was to be made, I accompanied him to the lodging of *Carneades*, where when we were come, we were told by the ſervants, that he was retired with a couple of friends (whoſe names they alſo told us) to one of the arbours in his garden, to enjoy under its cool ſhades a delightful protection from the yet troubleſome heat of the ſun.

ELEUTHERIUS being perfectly acquainted with that garden, immediately led me to the arbour, and relying on the intimate familiarity, that had been long cheriſhed betwixt him and *Carneades*; in ſpight of my reluctancy to what might look like an intruſion upon his privacy, drawing me by the hand, he abruptly entered the arbour, where we found *Carneades*, *Philoponus*, and *Themiſtius*, ſitting cloſe about a little round table, on which, beſides paper, pen, and ink, there lay two or three open books. *Carneades* appeared not at all troubled at this ſurpriſe, but riſing from the table, received his friend with open looks and arms, and welcoming me alſo with his wonted freedom and civility, invited us to reſt ourſelves by him; which, as ſoon as we had exchanged with his two friends (who were ours alſo) the civilities accuſtomed on ſuch occaſions, we did. And he, preſently after we had ſeated ourſelves, ſhutting the books that lay open, and turning to us with a ſmiling countenance, ſeemed ready to begin ſome ſuch unconcerning diſcourſe, as is wont to paſs, or rather waſte the time in promiſcuous companies.

BUT *Eleutherius* gueſſing at what he meant to do, prevented him by telling him, I perceive, *Carneades*, by the books, that you have been now ſhutting, and much more by the poſture, wherein I found perſons ſo qualified to diſcourſe of ſerious matters, and ſo accuſtomed to do it, that you three were, before our coming, engaged in ſome philoſophical conference; which I hope you will either proſecute, and allow us to be partakers of, in recompence of the freedom we have uſed in preſuming to ſurpriſe you, or elſe give us leave to repair the injury we ſhould otherwiſe do you, by leaving you to the freedom we have interrupted, and puniſhing ourſelves for our boldneſs, by depriving ourſelves of the happineſs of your company. With theſe laſt words he and I roſe up, as if we meant to be gone; but *Carneades* ſuddenly laying hold on his arm, and ſtopping him by it, ſmilingly told him, we are not ſo forward to loſe good company as you ſeem to imagine; eſpecially ſince you are pleaſed to be preſent at what we ſhall ſay, about ſuch a ſubject as that you found us conſidering. For that being the number of the elements, principles, or material ingredients of bodies, is an enquiry, whoſe truth is of that importance, and of that difficulty, that it may as well deſerve, as require, to be ſearched into by ſuch ſkilful indagators of nature, as yourſelves. And therefore we ſent to invite the bold and acute *Leucippus* to lend us ſome light by his atomical paradox, upon which we expected ſuch pregnant hints, that it was not without a great deal of trouble, that we had lately word brought us, that he was not to be found; and we had likewiſe begged the aſſiſtance of your preſence and thoughts, had not the meſſenger we employed to *Leucippus* informed us, that as he was going, he ſaw you both paſs by towards another part of the town; and this fruſtrated expectation of *Leucippus* his company, who told me but laſt night, that he would be ready to give me a meeting where I pleaſed to-day, having very long ſuſpended our conference about the freſhly-mentioned ſubject, it was ſo newly begun when you came in, that we ſhall ſcarce need to repeat any thing, to acquaint you with what had paſſed betwixt us before your arrival; ſo that I cannot but look upon it as a fortunate accident, that you ſhould come ſo ſeaſonably, to be not hearers alone, but, we hope, interlocutors at our conference. For we ſhall not only allow of your preſence at it, but deſire your aſſiſtance, in it; which I add, both for other reaſons, and becauſe though theſe learned gentlemen (ſays he, turning to his two friends) need not fear to diſcourſe before any auditory, provided it be intelligent enough to underſtand them, yet for my part (continues he with a new ſmile) I ſhall not dare to vent my unpremeditated thoughts before two ſuch critics, unleſs, by promiſing to take your turns of ſpeaking, you will allow me mine of quarrelling with what has been ſaid. He and his friends added divers things to convince us, that they were both deſirous, that we ſhould hear them, and reſolved againſt our doing ſo, unleſs we allowed them ſometimes to hear us. *Eleutherius*, after having a while fruitleſsly endeavoured to obtain leave to be ſilent, promiſed he would not be ſo always, provided, that he were permitted, according to the freedom of his genius and principles, to ſide with one of them in the managing of one argument, and, if he ſaw cauſe, with his antagoniſt, in the proſecution of another, without being confined to ſtick to any one party or opinion, which was after ſome debate accorded him. But I, conſcious to my own diſabilities, told them reſolutely, that I was as much more willing, as more fit, to be a hearer than a ſpeaker, among ſuch knowing perſons, and on ſo abſtruſe a ſubject. And that therefore I beſeeched them, without neceſſitating me to proclaim my weakneſſes, to allow me to leſſen them, by being a ſilent auditor of their diſcourſes: to ſuffer me to be at which, I could preſent them no motive, ſave that their inſtructions would make them in me a more intelligent admirer. I added, that I deſired not to be idle, whilſt they were employed, but would, if they pleaſed, by writing down in ſhort hand what ſhould be delivered, preſerve diſcourſes, that I knew would merit to be laſting. At firſt, *Carneades* and his two friends utterly rejected this motion; and all that my reſoluteneſs to make uſe of my ears, not tongue, at their debates, could do, was to make them acquieſce in the propoſition of *Eleutherius*, who thinking himſelf concerned, becauſe he brought me thither, to afford me ſome faint aſſiſtance, was content, that I ſhould regiſter their arguments, that I might be the better able, after the concluſion of their conference, to give them my ſenſe upon the ſubject of it (the number of elements or principles) which he promiſed I ſhould do at the end of the preſent debates, if time would permit, or elſe at our next meeting. And this being by him undertaken in my name, though without my conſent, the company would by no means receive my proteſtation againſt it, but caſting, all at once, their eyes on *Carneades*, they did by that, and their unanimous ſilence, invite him to begin; which (after a ſhort pauſe, during which he turned himſelf to *Eleutherius* and me) he did in this manner.

NOTWITHSTANDING the ſubtile reaſonings I have met with in the books of the Peripateticks, and the pretty experiments, that have been ſhewed me in the laboratories of chymiſts, I am of ſo diffident, or dull a nature, as to think, that if neither of them can bring more cogent arguments to evince the truth of their aſſertion than are wont to be brought; a man may rationally enough retain ſome doubts concerning the very number of thoſe material ingredients of mixt bodies, which ſome would have us call elements, and others principles. Indeed, when I conſidered, that the tenets concerning the elements are as conſiderable amongſt the doctrines of natural philoſophy, as the elements themſelves are among the bodies of the univerſe, I expected to find thoſe opinions ſolidly eſtabliſhed, upon which ſo many others are ſuperſtructed. But when I took the pains impartially to examine the bodies themſelves, that are ſaid to reſult from the blended elements, and to torture them into a confeſſion of their conſtituent principles, I was quickly induced to think, that the number of the elements has been contended about by philoſophers with more earneſtneſs than ſucceſs. This unſatisfiedneſs of mine has been much wondered at by theſe two gentlemen (at which words he pointed at *Themiſtius* and *Philoponus*) who, though they differ almoſt as much betwixt themſelves about the queſtion we are to conſider, as I do from either of them, yet they both agree very well in this, that there is a determinate number of ſuch ingredients as I was juſt now ſpeaking of, and that what that number is, I ſay not, may be (for what may not ſuch as they perſuade?) but is wont to be clearly enough demonſtrated both by reaſon and experience. This has occaſioned our preſent conference. For our diſcourſe this afternoon having fallen from one ſubject to another, and at length ſettled on this, they proffered to demonſtrate to me, each of them the truth of his opinion, out of both the topics, that I have freſhly named. But on the former (that of reaſon ſtrictly ſo taken) we declined inſiſting at the preſent, leſt we ſhould not have time enough before ſupper to go through the reaſons and experiments too. The latter of which we unanimouſly thought the moſt requiſite to be ſeriouſly examined. I muſt deſire you then to take notice gentlemen (continued *Carneades*) that my preſent buſineſs doth not oblige me ſo to declare my own opinion on the ſubject in queſtion, as to aſſert or deny the truth either of the peripatetic, or the chymical doctrine, concerning the number of the elements; but only to ſhew you, that neither of theſe doctrines hath been ſatisfactorily proved by the arguments commonly alledged on its behalf. So that if I really diſcern (as, perhaps, I think I do) that there may be a more rational account than ordinary given of one of theſe opinions, I am left free to declare myſelf of it, notwithſtanding my preſent engagement; it being obvious to all your obſervation, that a ſolid truth may be generally maintained by no other, than incompetent arguments. And to this declaration I hope it will be needleſs to add, that my taſk obliges me not to anſwer the arguments, that may be drawn either for *Themiſtius* or *Philoponus*'s opinion from the topic of reaſon, as expoſed to experiments; ſince it is theſe only, that I am to examine, and not all theſe neither, but ſuch of them alone, as either of them ſhall think fit to inſiſt on, and as have hitherto been wont to be brought either to prove, that it is the four peripatetick elements, or that it is the three chymical principles, that all compounded bodies conſiſt of. Theſe things (adds *Carneades*) I thought myſelf obliged to premiſe, partly leſt you ſhould do theſe gentlemen (pointing at *Themiſtius* and *Philoponus*, and ſmiling on them) the injury of meaſuring their parts by the arguments they are ready to propoſe, the laws of our conference confining them to make uſe of thoſe, that the vulgar of philoſophers (for even of them there is a vulgar) has drawn up to their hands; and partly, that you ſhould not condemn me of preſumption for diſputing againſt perſons over whom I can hope for no advantage, that I muſt not derive from the nature, or rules of our controverſy, wherein I have but a negative to defend, and wherein too I am like on ſeveral occaſions to have the aſſiſtance of one of my diſagreeing adverſaries againſt the other.

PHILOPONUS and *Themiſtius* ſoon returned this compliment with civilities of the like nature, in which *Eleutherius* perceiving them engaged, to prevent the further loſs of that time, of which they were not like to have very much to ſpare, he minded them, that their preſent buſineſs was not to exchange compliments, but arguments: and then addreſſing his ſpeech to *Carneades*, I eſteemed it no ſmall happineſs (ſays he) that I am come here ſo luckily this evening. For I have been long diſquieted with doubts concerning this very ſubject, which you are now ready to debate. And ſince a queſtion of this importance is to be now diſcuſſed by perſons, that maintain ſuch variety of opinions concerning it, and are both ſo able to inquire after truth, and ſo ready to imbrace it by whomſoever and on what occaſion ſoever it is preſented them; I cannot but promiſe myſelf, that I ſhall, before we part, either loſe my doubts or the hopes of ever finding them reſolved. *Eleutherius* pauſed not here; but, to prevent their anſwer, added almoſt in the ſame breath. And I am not a little pleaſed to find, that you are reſolved, on this occaſion, to inſiſt rather on experiments than ſyllogiſms. For I, and no doubt you, have long obſerved, that thoſe dialectical ſubtleties, that the ſchoolmen too often employ about phyſiological myſteries, are wont much more to declare the wit of him, that uſes them, than increaſe the knowledge or remove the doubts of ſober lovers of truth. And ſuch captious ſubtleties do indeed often puzzle, and ſometimes ſilence men, but rarely ſatisfy them; being like the tricks of jugglers, whereby men doubt not but that they are cheated, though oftentimes they cannot declare by what ſlights they are impoſed on. And therefore I think you have done very wiſely to make it your buſineſs to conſider the phænomena relating to the preſent queſtion, which have been afforded by experiments, eſpecially ſince it might ſeem injurious to our ſenſes, by whoſe mediation we acquire ſo much of the knowledge we have of things corporal, to have recourſe to far-fetched and abſtracted ratiocinations, to know what are the ſenſible ingredients of thoſe ſenſible things, that we daily ſee and handle, and are ſuppoſed to have the liberty to untwiſt (if I may ſo ſpeak) into the primitive bodies they conſiſt of. He annexed, that he wiſhed therefore they would no longer delay his expected ſatisfaction, if they had not, as he feared they had, forgotten ſomething preparatory to their debate; and that was to lay down, what ſhould be all along underſtood by the word principle or element. *Carneades* thanked him for his admonition, but told him, that they had not been unmindful of ſo requiſite a thing. But that being gentlemen, and very far from the litigious humour of loving to wrangle about words, or terms, or notions as empty, they had, before his coming in, readily agreed promiſcuouſly to uſe, when they pleaſed, elements and principles, as terms equivalent; and to underſtand both by the one and the other thoſe primitive and ſimple bodies, of which the mixt ones are ſaid to be compoſed, and into which they are ultimately reſolved. And upon the ſame account (he added) we agreed to diſcourſe of the opinions to be debated, as we have found them maintained by the generality of the aſſertors of the four elements of the one party, and of thoſe, that receive the three principles on the other, without tying ourſelves to inquire ſcrupulouſly, what notion either *Ariſtotle* or *Paracelſus*, or this or that interpreter, or follower of either of thoſe great perſons, framed of elements or principles; our deſign being to examine, not what theſe or thoſe writers thought or taught, but what we find to be the obvious and moſt general opinion of thoſe, who are willing to be accounted favourers of the peripatetic or chymical doctrine, concerning this ſubject.

I SEE not (ſays *Eleutherius*) why you might not immediately begin to argue, if you were but agreed which of your two friendly adverſaries ſhall be firſt heard. And it being quickly reſolved on, that *Themiſtius* ſhould firſt propoſe the proofs for his opinion, becauſe it was the ancienter, and the more general, he made not the company expect long before he thus addreſſed himſelf to *Eleutherius*, as to the perſon leaſt intereſted in the diſpute.

If you have taken ſufficient notice of the late confeſſion, which was made by *Carneades*, and which (though his civility dreſſed it up in complemental expreſſions) was exacted of him by his juſtice, I ſuppoſe you will be eaſily made ſenſible, that I engage in this controverſy with great and peculiar diſadvantages, beſides thoſe, which his parts and my perſonal diſabilities would bring to any other cauſe, to be maintained by me againſt him. For he juſtly apprehending the force of truth, though ſpeaking by no better a tongue than mine, has made it the chief condition of our duel, that I ſhould lay aſide the beſt weapons I have, and thoſe I can beſt handle; whereas if I were allowed the freedom, in pleading for the four elements, to employ the arguments ſuggeſted to me by reaſon to demonſtrate them, I ſhould almoſt as little doubt of making you a proſelyte to thoſe unſevered teachers, Truth and *Ariſtotle*, as I do of your candour and your judgment. And I hope you will however conſider, that that great favourite and interpreter of nature, *Ariſtotle*, who was (as his *Organum* witneſſes) the greateſt maſter of logic, that ever lived, diſclaimed the courſe taken by other petty philoſophers (antient and modern) who not attending the coherence and conſequences of their opinions, are more ſollicitous to make each particular opinion plauſible independently upon the reſt, than to frame them all ſo, as not only to be conſiſtent together, but to ſupport each other. For that great man, in his vaſt and comprehenſive intellect, ſo framed each of his notions, that being curiouſly adapted into one ſyſtem, they need not each of them any other defence than that, which their mutual coherence gives them; as it is in an arch, where each ſingle ſtone, which, if ſevered from the reſt, would be perhaps defenceleſs, is ſufficiently ſecured by the ſolidity and entireneſs of the whole fabric, of which it is a part. How juſtly this may be applied to the preſent caſe, I could eaſily ſhew you, if I were permitted to declare to you, how harmonious *Ariſtotle*'s doctrine of the elements is with his other principles of philoſophy; and how rationally he has deduced their number from that of the combinations of the four firſt qualities from the kinds of ſimple motion belonging to ſimple bodies, and from I know not how many other principles and phænomena of nature, which ſo conſpire with his doctrine of the elements, that they mutually ſtrengthen and ſupport each other. But ſince it is forbidden me to inſiſt on reflections of this kind, I muſt proceed to tell you, that though the aſſertors of the four elements, value reaſon ſo highly, and are furniſhed with arguments enough drawn from thence, to be ſatisfied, that there muſt be four elements, though no man had ever yet made any ſenſible trial to diſcover their number, yet they are not deſtitute of experience to ſatisfy others, that are wont to be more ſwayed by their ſenſes, than their reaſon. And I ſhall proceed to conſider the teſtimony of experience, when I ſhall have firſt advertiſed you, that if men were as perfectly rational, as it is to be wiſhed they were, this ſenſible way of probation would be as needleſs, as it is wont to be imperfect. For it is much more high and philoſophical to diſcover things *à priori*, than *à poſteriori*. And therefore the Peripatetics have not been very ſollicitous to gather experiments to prove their doctrines, contenting themſelves with a few only, to ſatisfy thoſe, that are not capable of a nobler conviction. And indeed they employ experiments rather to illuſtrate than to demonſtrate their doctrines, as aſtronomers uſe ſpheres of paſteboard, to deſcend to the capacities of ſuch, as muſt be taught by their ſenſes, for want of being arrived to a clear apprehenſion of purely mathematical notions and truths. I ſpeak thus *Eleutherius* (adds *Themiſtius*) only to do right to reaſon, and not out of diffidence of the experimental proof I am to alledge. For though I ſhall name but one, yet it is ſuch a one, as will make all others appear as needleſs, as itſelf will be found ſatisfactory. For if you but conſider a piece of green wood burning in a chimney, you will readily diſcern in the diſbanded parts of it the four elements, of which we teach it and other mixt bodies to be compoſed. The fire diſcovers itſelf in the flame by its own light; the ſmoke, by aſcending to the top of the chimney, and there readily vaniſhing into air, like a river loſing itſelf in the ſea, ſufficiently manifeſts to what element it belongs, and gladly returns. The water in its own form, boiling and hiſſing at the ends of the burning wood, betrays itſelf to more than one of our ſenſes; and the aſhes by their weight, their fiérineſs, and their dryneſs, put it paſt doubt, that they belong to the elements of earth. If I ſpoke (continues *Themiſtius*) to leſs knowing perſons, I would perhaps make ſome excuſe for building upon ſuch an obvious and eaſy analyſis; but it would be, I fear, injurious, not to think ſuch an apology needleſs to you, who are too judicious either to think it neceſſary, that experiments to prove obvious truths ſhould be far-fetched, or to wonder, that among ſo many mixt bodies, that are compounded of the four elements, ſome of them ſhould, upon a ſlight analyſis, manifeſtly exhibit the ingredients they conſiſt of. Eſpecially ſince it is very agreeable to the goodneſs of nature, to diſcloſe, even in ſome of the moſt obvious experiments, that men make, a truth ſo important and ſo requiſite to be taken notice of by them. Beſides, that our analyſis, by how much the more obvious we make it, by ſo much the more ſuitable it will be to the nature of that doctrine, which it is alledged to prove; which being as clear and intelligible to the underſtanding as obvious to the ſenſe, it is no marvel the learned part of mankind ſhould ſo long and ſo generally imbrace it. For this doctrine is very different from the whimſies of chymiſts and other modern innovators; of whoſe hypotheſes we may obſerve, as naturaliſts do of leſs perfect animals, that as they are haſtily formed, ſo they are commonly ſhort lived. For ſo theſe, as they are often framed in one week, are, perhaps, thought fit to be laughed at the next; and being built, perchance, but upon two or three experiments, are deſtroyed by a third or fourth, whereas the doctrine of the four elements was framed by *Ariſtotle*, after he had leiſurely conſidered thoſe theories of former philoſophers, which are now with great applauſe revived, as diſcovered by theſe later ages; and had ſo judiciouſly detected and ſupplied the errors and defect of former hypotheſes concerning the elements, that his doctrine of them has been ever ſince deſervedly embraced by the lettered part of mankind: all the philoſophers, that preceded him, having, in their ſeveral ages, contributed to the completeneſs of this doctrine, as thoſe of ſucceeding times have acquieſced in it. Nor has an hypotheſis, ſo deliberately and maturely eſtabliſhed, been called in queſtion, till in the laſt century *Paracelſus* and ſome few other ſooty empirics, rather than (as they are fain to call themſelves) philoſophers, having their eyes darkened, and their brains troubled with the ſmoke of their own furnaces, began to rail at the Peripatetick doctrine, which they were too illiterate to underſtand, and to tell the credulous world, that they could ſee but three ingredients in mixed bodies; which, to gain themſelves the repute of inventors, they endeavoured to diſguiſe, by calling them, inſtead of earth, and fire, and vapour, ſalt, ſulphur, and mercury; to which they gave the canting title of hypoſtatical principles. But when they came to deſcribe them, they ſhewed how little they underſtood what they meant by them, by diſagreeing as much from one another, as from the truth they agreed in oppoſing: for they deliver their hypotheſes as darkly as their proceſſes; and it is almoſt as impoſſible for any ſober man to find their meaning, as it is for them to find their elixir. And, indeed, nothing has ſpread their philoſophy, but their great brags and undertakings; notwithſtanding all which (ſays *Themiſtius* ſmiling) I ſcarce know any thing they have performed worth wondering at, ſave that they have been able to draw *Philoponus* to their party, and to engage him to the defence of an unintelligible hypotheſis, who knows ſo well as he does, that principles ought to be like diamonds, as well very clear, as perfectly ſolid.

THEMISTIUS having after theſe laſt words declared by his ſilence, that he had finiſhed his diſcourſe, *Carneades* addreſſing himſelf, as his adverſary had done, to *Eleutherius*, returned this anſwer to it: I hoped for a demonſtration, but I perceive *Themiſtius* hopes to put me off with an harangue, wherein he cannot have given me a greater opinion of his parts, than he has given me diſtruſt for his hypotheſis, ſince for it even a man of ſuch learning can bring no better arguments. The rhetorical part of his diſcourſe, though it make not the leaſt part of it, I ſhall ſay nothing to, deſigning to examine only the argumentative part, and leaving it to *Philoponus* to anſwer thoſe paſſages, wherein either *Paracelſus* or chymiſts are concerned. I ſhall obſerve to you, that in what he has ſaid beſides, he makes it his buſineſs to do theſe two things. The one to propoſe and make out an experiment to demonſtrate the common opinion about the four elements; and the other, to inſinuate divers things, which he thinks may repair the weakneſs of his argument, from experience, and upon other accounts bring ſome credit to the otherwiſe defenceleſs doctrine he maintains.

To begin then with his experiment of the burning wood, it ſeems to me to be obnoxious to not a few conſiderable exceptions.

AND firſt, if I would now deal rigidly with my adverſary, I might here make a great queſtion of the very way of probation, which he and others employ, without the leaſt ſcruple, to evince, that the bodies commonly called mixt are made up of earth, air, water, and fire, which they are pleaſed alſo to call elements; namely, that upon the ſuppoſed analyſis made by the fire of the former ſort of concretes, there are wont to emerge bodies reſembling thoſe, which they take for the elements. For, not to anticipate here, what I foreſee I ſhall have occaſion to inſiſt on, when I come to diſcourſe with *Philoponus* concerning the right, that fire has to paſs for the proper and univerſal inſtrument of analyzing mixed bodies; not to anticipate that, I ſay, if I were diſpoſed to wrangle, I might alledge, that by *Themiſtius* his experiment it would appear rather, that thoſe he calls elements, are made of thoſe he calls mixed bodies, than mixed bodies of the elements. For, in *Themiſtius*'s analyzed wood, and in other bodies diſſipated and altered by the fire, it appears, and he confeſſes, that, which he takes for elementary fire and water, are made out of the concrete; but it appears not, that the concrete was made up of fire and water. Nor has either he, or any man, for aught I know, of his perſuaſion, yet proved, that nothing can be obtained from a body by the fire, that was not pre-exiſtent in it.

AT this unexpected objection, not only *Themiſtius*, but the reſt of the company appeared not a little ſurprized; but after a while, *Philoponus* conceiving his opinion, as well as that of *Ariſtotle*, concerned in that objection: you cannot ſure (ſays he, to *Carneades*) propoſe this difficulty, not to call it cavil, otherwiſe than as an exerciſe of wit, and not as laying any weight upon it. For how can that be ſeparated from a thing, that was not exiſtent in it? When, for inſtance, a refiner mingles gold and lead, and expoſing this mixture upon a cupel to the violence of the fire, thereby ſeparates it into pure and refulgent gold and lead (which driven off together with the droſs of the gold, is thence called *lithargyrum auri)* can any man doubt, that ſees theſe two ſo differing ſubſtances ſeparated from the maſs, that they were exiſtent in it, before it was committed to the fire?

I SHOULD (replies *Carneades)* allow your argument to prove ſomething, if, as men ſee the refiners commonly take beforehand both lead and gold to make the maſs you ſpeak of, ſo we did ſee nature pull down a parcel of the element of fire, that is fancied to be placed I know not how many thouſand leagues off, contiguous to the orb of the moon, and to blend it with a quantity of each of the three elements, to compoſe every mixed body, upon whoſe reſolution the fire preſents us with fire, and earth, and the reſt. And let me add, *Philoponus*, that to make your reaſoning cogent, it muſt be firſt proved, that the fire does only take the elementary ingredients aſunder, without otherwiſe altering them. For elſe it is obvious, that bodies may afford ſubſtances, which were not pre-exiſtent in them; as fleſh too long kept produces maggots; and old cheeſe, mites; which I ſuppoſe you will not affirm to be ingredients of thoſe bodies. Now that fire does not always barely ſeparate the elementary parts, but, ſometimes at leaſt, alter alſo the ingredients of bodies, if I did not expect ere long a better occaſion to prove it, I might make probable out of your very inſtance, wherein there is nothing elementary ſeparated by the great violence of the refiner's fire: the gold and lead, which are the two ingredients ſeparated upon the analyſis being confeſſedly yet perfectly mixed bodies, and the litharge being lead indeed, but ſuch lead, as is differing in conſiſtence and other qualities from what it was before. To which I muſt add, that I have ſometimes ſeen, and ſo queſtionleſs have you, much oftener, ſome parcels of glaſs adhering to the teſt or cupel, and this glaſs, though emergent, as well as the gold or litharge, upon your analyſis, you will not, I hope, allow to have been a third ingredient of the maſs, out of which the fire produced it.

BOTH *Philoponus* and *Themiſtius* were about to reply, when *Eleutherius* apprehending, that the proſecution of this diſpute would take up time, which might be better employed, thought fit to prevent them, by ſaying to *Carneades*, You made at leaſt half a promiſe, when you firſt propoſed this objection, that you would not (now at leaſt) inſiſt on it; nor indeed does it ſeem to be of abſolute neceſſity to your cauſe, that you ſhould. For though you ſhould grant, that there are elements, it would not follow, that there muſt be preciſely four; and therefore I hope you will proceed to acquaint us with your other and more conſiderable objections againſt *Themiſtius*'s opinion, eſpecially ſince there is ſo great a diſproportion in bulk betwixt the earth, water and air, on the one part, and thoſe little parcels of reſembling ſubſtances, that the fire ſeparates from concretes, on the other part, that I can ſcarce think, that you are ſerious, when to loſe no advantage againſt your adverſary, you ſeem to deny it to be rational, to conclude theſe great ſimple bodies to be the elements, and not the products of compounded ones.

WHAT you alledge (replies *Carneades)* of the vaſtneſs of the earth and water, has long ſince made me willing to allow them to be the greateſt and chief maſſes of matter to be met with here below: but I think I could ſhew you, if you would give me leave, that this will prove only, that the elements, as you call them, are the chief bodies, that make up the neighbouring part of the world, but not that they are ſuch ingredients as every mixt body muſt conſiſt of. But ſince you challenge me of ſomething of a promiſe, though it be not an entire one, yet I ſhall willingly perform it. And indeed I intended not, when I firſt mentioned this objection, to inſiſt on it at preſent againſt *Themiſtius* (as I plainly intimated in my way of propoſing it) being only deſirous to let you ſee, that though I diſcerned my advantages, yet I was willing to forego ſome of them, rather than appear a rigid adverſary of a cauſe ſo weak, that it may with ſafety be favourably dealt with. But I muſt here profeſs, and deſire you to take notice of it, that though I paſs on to another argument, it is not becauſe I think this firſt invalid. For you will find, in the progreſs of our diſpute, that I had ſome reaſon to queſtion the very way of probation imployed both by Peripatetics and Chymiſts, to evince the being and number of the elements. For that there are ſuch, and that they are wont to be ſeparated by the analyſis made by fire, is indeed taken for granted by both parties, but has not (for aught I know) been ſo much as plauſibly attempted to be proved by either. Hoping then, that when we come to that part of our debate, wherein conſiderations relating to this matter are to be treated of, you will remember what I have now ſaid, and that I do rather for a while ſuppoſe, than abſolutely grant the truth of what I have queſtioned, I will proceed to another objection.

AND hereupon *Eleutherius* having promiſed him not to be unmindful, when time ſhould ſerve, of what he had declared;

I CONSIDER then (ſays *Carneades*) in the next place, that there are divers bodies, out of which *Themiſtius* will not prove in haſte, that there can be ſo many elements as four extracted by the fire. And I ſhould, perchance, trouble him, if I ſhould aſk him, what Peripatetic can ſhew us (I ſay not, all the four elements, for that would be too rigid a queſtion, but) any one of them extracted out of gold by any degree of fire whatſoever. Nor is gold the only body in nature, that would puzzle an Ariſtotelian (that is no more) to analyze by the fire into elementary bodies; ſince, for aught I have yet obſerved, both ſilver and calcined Venetian talc, and ſome other concretes, not neceſſary here to be named, are ſo fixt, that to reduce any of them into four heterogeneous ſubſtances has hitherto proved a taſk much too hard, not only for the diſciples of *Ariſtotle*, but thoſe of *Vulcan*, at leaſt, whilſt the latter have employed only fire to make the analyſis.

THE next argument (continues *Carneades*) that I ſhall urge againſt *Themiſtius*'s opinion ſhall be this; that as there are divers bodies, whoſe analyſis by fire cannot reduce them into ſo many heterogeneous ſubſtances or ingredients as four, ſo there are others, which may be reduced into more, as the blood (and divers other parts) of men and other animals; which yield when analyzed five diſtinct ſubſtances, phlegm, ſpirit, oil, ſalt and earth, as experience has ſhewn us in diſtilling man's blood, hartſhorn, and divers other bodies, that, belonging to the animal-kingdom, abound with a not uneaſily ſequeſtrable ſalt.

THE SCEPTICAL CHYMIST:

PART I.

I AM (ſays *Carneades*) ſo unwilling to deny *Eleutherius* any thing, that though, before the reſt of my company, I am reſolved to make good the part I have undertaken of a ſceptic; yet I ſhall readily, ſince you will have it ſo, lay aſide for a while the perſon of an adverſary to the Peripatetics and Chymiſts; and before I acquaint you with my objections againſt their opinions, acknowledge to you, what may be (whether truly or not) tolerably enough added, in favour of a certain number of principles of mixt bodies, to that grand and known argument from the analyſis of compound bodies, which I may poſſibly hereafter be able to confute.

And that you may the more eaſily examine, and the better judge of what I have to ſay, I ſhall caſt it into a pretty number of diſtinct propoſitions, to which I ſhall not premiſe any thing; becauſe I take it for granted, that you need not be advertiſed, that much of what I am to deliver, whether for or againſt a determinate number of ingredients of mixt bodies, may be indifferently applied to the four Peripatetic elements, and the three chymical principles, though divers of my objections will more peculiarly belong to theſe laſt named; becauſe the chymical hypotheſis ſeeming to be much more countenanced by experience than the other, it will be expedient to inſiſt chiefly upon the diſproving of that; eſpecially ſince moſt of the arguments, that are employed againſt it, may, by a little variation, be made to conclude, at leaſt as ſtrongly againſt the leſs plauſible Ariſtotelian doctrine.

To proceed then to my propoſitions, I ſhall begin with this, that

Prop. I. *It ſeems not abſurd to conceive, that at the firſt production of mixt bodies, the univerſal matter, whereof they among other parts of the univerſe conſiſted, was actually divided, into little particles, of ſeveral ſizes and ſhapes, variouſly moved.*

This (ſays *Carneades*) I ſuppoſe you will eaſily enough allow. For beſides that, which happens in the generation, corruption, nutrition, and waſting of bodies, that, which we diſcover partly by our microſcopes of the extream littleneſs of even the ſcarce ſenſible parts of concretes, and partly by the chymical reſolutions of mixt bodies, and by divers other operations of ſpagyrical fires upon them, ſeems ſufficiently to manifeſt their conſiſting of parts very minute, and of differing figures. And that there does alſo intervene a various local motion of ſuch ſmall bodies, will ſcarce be denied; whether we chuſe to grant the origin of concretions aſſigned by *Epicurus*, or that related by *Moſes*. For the firſt, as you well know, ſuppoſes not only all mixt bodies, but all others to be produced by the various and caſual occurſions of atoms, moving themſelves to and fro by an internal principle in the immenſe or rather infinite vacuum. And as for the inſpired hiſtorian, he, informing us, that the great and wiſe author of things did not immediately create plants, beaſts, birds, *&c.* but produced them out of thoſe portions of the pre-exiſtent, though created, matter, that he calls water and earth, allows us to conceive, that the conſtituent particles, whereof theſe new concretes were to conſiſt, were variouſly moved, in order to their being connected into the bodies they were, by their various coalitions and textures, to compoſe.

But (continues *Carneades*) preſuming, that the firſt propoſition needs not be longer inſiſted on, I will paſs on to the ſecond, and tell you that

Prop. II. *Neither is it impoſſible that of theſe minute particles divers of the ſmalleſt and neighbouring ones were here and there aſſociated into minute maſſes or cluſters, and did by their coalitions conſtitute great ſtore of ſuch little primary concretions or maſſes, as were not eaſily diſſipable into ſuch particles, as compoſed them.*

To what may be deduced, in favour of this aſſertion from the nature of the thing itſelf, I will add ſomething out of experience; which, though I have not known it uſed to ſuch a purpoſe, ſeems to me more fairly to make out, that there may be elementary bodies, than the more queſtionable experiments of Peripatetics and Chymiſts prove, that there are ſuch. I conſider then, that gold will mix and be colliquated not only with ſilver, copper, tin and lead, but with antimony, *Regulus Martis*, and many other minerals, with which it will compoſe bodies very differing both from gold, and the other ingredients of the reſulting concretes. And the ſame gold will alſo by common aqua regis, and (I ſpeak it knowingly) by divers other menſtruums be reduced into a ſeeming liquor, inſomuch, that the corpuſcles of gold will, with thoſe of the menſtruum, paſs through cap-paper, and with them alſo coagulate into a cryſtalline ſalt. And I have further tried, that with a ſmall quantity of a certain ſaline ſubſtance I prepared, I can eaſily enough ſublime gold into the form of red cryſtals, of a conſiderable length; and many other ways may gold be diſguiſed, and help to conſtitute bodies of very differing natures both from it and from one another, and nevertheleſs be afterward reduced to the ſelf-ſame numerical, yellow, fixt, ponderous and malleable gold it was before its commixture. Nor is it only the fixedeſt of metals, but the moſt fugitive, that I may employ in favour of our propoſition: for quickſilver will, with divers metals, compoſe an amalgam; with divers menſtruums, it ſeems to be turned into a liquor; with aqua fortis, it will be brought into either a red or white powder, or precipitate; with oil of vitriol, into a pale yellow one; with ſulphur, it will compoſe a blood-red and volatile cinabar; with ſome ſaline bodies, it will aſcend in form of a ſalt, which will be diſſoluble in water; with regulus of antimony and ſilver, I have ſeen it ſublimed into a kind of cryſtals; with another mixture, I reduced it into a malleable body, into a hard and brittle ſubſtance by another: and ſome there are, who affirm, that by proper additaments they can reduce quickſilver into oil, nay into glaſs, to mention no more. And yet out of all theſe exotic compounds, we may recover the very ſame running mercury, that was the main ingredient of them, and was ſo diſguiſed in them. Now the reaſon (proceeds *Carneades*) that I have repreſented theſe things concerning gold and quickſilver, is, that it may not appear abſurd to conceive, that ſuch little primary maſſes or cluſters, as our propoſition mentions, may remain undiſſipated, notwithſtanding their entering into the compoſition of various concretions, ſince the corpuſcles of gold and mercury, though they be not primary concretions of the moſt minute particles of matter, but confeſſedly mixed bodies, are able to concur plentifully to the compoſition of ſeveral very differing bodies, without loſing their own nature or texture, or having their coheſion violated by the divorce of their aſſociated parts or ingredients.

GIVE me leave to add (ſays *Eleutherius*) on this occaſion, to what you now obſerved, that as confidently as ſome chymiſts, and other modern innovators in philoſophy are wont to object againſt the Peripatetics, that from the mixture of their four elements there could ariſe but an inconſiderable variety of compound bodies; yet, if the Ariſtotelians were but half as well verſed in the works of nature, as they are in the writings of their maſter, the propoſed objection would not ſo calmly triumph, as for want of experiments they are fain to ſuffer it to do. For if we aſſign to the corpuſcles, whereof each element conſiſts, a peculiar ſize and ſhape, it may eaſily enough be manifeſted, that ſuch differingly-figured corpuſcles may be mingled in ſuch various proportions, and may be connected ſo many ſeveral ways, that an almoſt incredible number of variouſly qualified concretes may be compoſed of them. Eſpecially ſince the corpuſcles of one element may barely, by being aſſociated among themſelves, make up little maſſes of differing ſize and figure from their conſtituent parts: and ſince alſo, to the ſtrict union of ſuch minute bodies there ſeems oftentimes nothing requiſite, beſides the bare contact of a great part of their ſurfaces. And how great a variety of phænomena the ſame matter, without the addition of any other, and only ſeveral ways diſpoſed or contexed, is able to exhibit, may partly appear by the multitude of differing engines, which by the contrivances of ſkilful mechanicians, and the dexterity of expert workmen, may be made of iron alone. But, in our preſent caſe, being allowed to deduce compound bodies from four very differently qualified ſorts of matter, he, who ſhall but conſider, what you freſhly took notice of, concerning the new concretes reſulting from the mixture of incorporated minerals, will ſcarce doubt, but that the four elements, managed by nature's ſkill, may afford a multitude of differing compounds.

I AM thus far of your mind (ſays *Carneades*) that the Ariſtotelians might with probability deduce a much greater number of compound bodies from the mixture of their four elements, than according to their preſent hypotheſis they can, if inſtead of vainly attempting to deduce the variety and proprieties of all mixed bodies from the combinations and temperaments of the four elements, as they are (among them) endowed with the four firſt qualities, they had endeavoured to do it by the bulk and figure of the ſmalleſt parts of thoſe ſuppoſed elements. For from theſe more catholic and fruitful accidents of the elementary matter may ſpring a great variety of textures, upon whoſe account a multitude of compound bodies may very much differ from one another. And what I now obſerve touching the four Peripatetic elements, may be alſo applied, *mutatis mutandis* (as they ſpeak) to the chymical principles. But (to take notice of that by the by) both the one and the other muſt, I fear, call into their aſſiſtance ſomething, that is not elementary, to excite or regulate the motion of the parts of the matter, and diſpoſe them after the manner requiſite to the conſtitution of particular concretes. For that otherwiſe they are like to give us but a very imperfect account of the origin of very many mixed bodies, it would, I think, be no hard matter to perſuade you, if it would not ſpend time, and were no digreſſion, to examine, what they are wont to alledge of the origin of the textures and qualities of mixed bodies, from a certain ſubſtantial form, whoſe origination they leave more obſcure than what it is aſſumed to explicate.

BUT to proceed to a new propoſition.

I ſhall not peremptorily deny, that from moſt of ſuch mixed bodies, as partake either of animal or vegetable nature, there may, by the help of the fire, be actually obtained a determinate number (whether three, four, or five, or fewer or more) of ſubſtances, worthy of differing denominations. Prop. III.

OF the experiments, that induce me to make this conceſſion, I am like to have occaſion enough to mention ſeveral in the proſecution of my diſcourſe. And therefore, that I may not hereafter be obliged to trouble you and myſelf with needleſs repetitions, I ſhall now only deſire you to take notice of ſuch experiments, when they ſhall be mentioned, and in your thoughts refer them hither.

To theſe three conceſſions, I have but this fourth to add, that

It may likewiſe be granted, that thoſe diſtinct ſubſtances, which concretes generally either afford, or are made up of, may, without very much inconvenience, be called the elements or principles of them. Prop. IV.

WHEN I ſaid, without very much inconvenience, I had in my thoughts that ſober admonition of *Galen, cum de re conſtat, de verbis non eſt litigandum.* And therefore alſo I ſcruple not to ſay, elements or principles, partly becauſe the chymiſts are wont to call the ingredients of mixed bodies, principles, as the Ariſtotelians name them, elements; I would here exclude neither: and, partly becauſe it ſeems doubtful, whether the ſame ingredients may not be called principles; as not being compounded of any more primary bodies; and elements, in regard that all mixed bodies are compounded of them. But I thought it requiſite to limit my conceſſion, by premiſing the words, *very much*, to the word *inconvenience*; becauſe, that though the inconvenience of calling the diſtinct ſubſtances, mentioned in the propoſition, elements or principles, be not very great, yet that it is impropriety of ſpeech, and conſequently in a matter of this moment not to be altogether overlooked, you will perhaps think, as well as I, by that time you ſhall have heard the following part of my diſcourſe, by which you will beſt diſcern what conſtruction to put upon the former propoſitions, and how far they may be looked upon, as things that I concede as true, and how far as things I only repreſent as ſpecious enough to be fit to be conſidered.

AND now *Eleutherius* (continues *Carneades)* I muſt reſume the perſon of a ſceptic, and, as ſuch, propoſe ſome part of what may be either diſliked, or at leaſt doubted of, in the common hypotheſis of the chymiſts; which if I examine with a little the more freedom, I hope I need not deſire you (a perſon to whom I have the happineſs of being ſo well known) to look upon it as ſomething more ſuitable to the employment, whereto the company has, for this meeting, doomed me, than either to my humour or my cuſtom.

Now though I might preſent you many things againſt the vulgar chymical opinion of the three principles, and the experiments wont to be alledged as demonſtrations of it; yet thoſe I ſhall at preſent offer you, may be conveniently enough comprehended in four capital conſiderations: touching all which I ſhall only premiſe this in general, that ſince it is not my preſent taſk ſo much to aſſert an hypotheſis of my own, as to give an account wherefore I ſuſpect the truth of that of the chymiſts, it ought not to be expected, that all my objections ſhould be of the moſt cogent ſort, ſince it is reaſon enough to doubt of a propoſed opinion, that there appears no cogent reaſon for it.

To come then to the objections themſelves; I conſider in the firſt place, that notwithſtanding what common chymiſts have proved or taught, it may reaſonably enough be doubted, how far, and in what ſenſe, fire ought to be eſteemed the genuine and univerſal inſtrument of analyzing mixed bodies.

THIS doubt, you may remember, was formerly mentioned, but ſo tranſiently diſcourſed of, that it will now be fit to inſiſt upon it; and manifeſt, that it was not ſo inconſiderately propoſed as our adverſaries then imagined.

BUT, before I enter any further into this diſquiſition, I cannot but here take notice, that it were to be wiſhed, our chymiſts had clearly informed us, what kind of diviſion of bodies by fire muſt determine the number of the elements: for it is nothing near ſo eaſy, as many ſeem to think, to determine diſtinctly the effects of heat, as I could eaſily manifeſt, if I had leiſure to ſhew you, how much the operations of fire may be diverſified by circumſtances. But not wholly to paſs by a matter of this importance, I will firſt take notice to you, that guaiacum (for inſtance) burnt with an open fire in a chimney, is ſequeſtered into aſhes and ſoot, whereas the ſame wood, diſtilled in a retort does yield far other heterogeneities (to uſe the Helmontian expreſſion) and is reſolved into oil, ſpirit, vinegar, water and charcoal; the laſt of which, to be reduced into aſhes, requires the being farther calcined than it can be in a cloſe veſſel. Beſides, having kindled amber, and held a clean ſilver ſpoon, or ſome other concave and ſmooth veſſel, over the ſmoke of its flame, I obſerved the ſoot, into which that fume condenſed, to be very differing from any thing, that I had obſerved to proceed from the ſteam of amber purpoſely (for that is not uſual) diſtilled *per ſe* in cloſe veſſels. Thus having, for trial's ſake, kindled camphire, and catched the ſmoke, that copiouſly aſcended out of the flame, it condenſed into a black and unctuous ſoot, which would not have been gueſſed by the ſmell or other properties to have proceeded from camphire: whereas having (as I ſhall otherwhere more fully declare) expoſed a quantity of that fugitive concrete to a gentle heat in a cloſe glaſs-veſſel, it ſublimed up without ſeeming to have loſt any thing of its whiteneſs, or its nature; both which it retained, though afterwards I ſo increaſed the fire, as to bring it to fuſion. And, beſides camphire, there are divers other bodies (that I elſewhere name) in which the heat in cloſe veſſels is not wont to make any ſeparation of heterogeneities, but only a comminution of parts, thoſe that riſe firſt being homogeneal with the others, though ſubdivided into ſmaller particles: whence ſublimations have been ſtyled, *The Peſtles of the Chymiſts*. But not here to mention what I elſewhere take notice of, concerning common brimſtone once or twice ſublimed, that, expoſed to a moderate fire in ſubliming-pots, it riſes all into dry, and almoſt taſteleſs, flowers: whereas being expoſed to a naked fire, it affords ſtore of a ſaline and fretting liquor; not to mention this, I ſay, I will further obſerve to you, that as it is conſiderable in the analyſis of mixt bodies, whether the fire act on them, when they are expoſed to the open air, or ſhut up in cloſe veſſels, ſo is the degree of fire, by which the analyſis is attempted, of no ſmall moment. For a mild balneum will ſever unfermented blood (for inſtance) but into phlegm and *caput mortuum*, the latter whereof (which I have ſometimes had) hard, brittle, and of divers colours (tranſparent almoſt like tortoiſe-ſhell) preſſed by a good fire in a retort yields a ſpirit, an oil or two, and a volatile ſalt, beſides another *caput mortuum*. It may be alſo pertinent to our preſent deſign, to take notice of what happens in the making and diſtilling of ſoap; for by one degree of fire, the ſalt, the water, and the oil or greaſe, whereof that factitious concrete is made up, being boiled up together, are eaſily brought to mingle and incorporate into one maſs; but by another and further degree of heat, the ſame maſs may be again divided into an oleaginous and aqueous, a ſaline and an earthy part. And ſo we may obſerve, that impure ſilver and lead being expoſed together to a moderate fire, will thereby be colliquated into one maſs, and mingle *per minima*, as they ſpeak; whereas a much vehementer fire will drive or carry off the baſer metals (I mean the lead, and the copper or other alloy) from the ſilver, though not, for aught appears, ſeparate them from one another. Beſides, when a vegetable abounding in fixed ſalt is analyzed by a naked fire, as one degree of heat will reduce it into aſhes, (as the chymiſts themſelves teach us) ſo, by only a further degree of fire, thoſe aſhes may be vitrified and turned into glaſs. I will not ſtay to examine, how far a mere chymiſt might on this occaſion demand, if it be lawful for an Ariſtotelian to make aſhes (which he miſtakes for mere earth) paſs for an element, becauſe by one degree of fire it may be produced, why a chymiſt may not upon the like principle argue, that glaſs is one of the elements of many bodies, becauſe that alſo may be obtained from them, barely by the fire? I will not, I ſay, loſe time to examine this, but obſerve, that by a method of applying the fire, ſuch ſimilar bodies may be obtained from a concrete, as chymiſts have not been able to ſeparate; either by barely burning it in an open fire, or by barely diſtilling it in cloſe veſſels. For to me it ſeems very conſiderable, and I wonder, that men have taken ſo little notice of it, that I have not, by any of the common ways of diſtillation in cloſe veſſels, ſeen any ſeparation made of ſuch a volatile ſalt as is afforded us by wood, when that is firſt by an open fire divided into aſhes and ſoot, and that ſoot is afterwards placed in a ſtrong retort, and compelled by an urgent fire to part with its ſpirit, oil and ſalt. For though I dare not peremptorily deny, that in the liquors of guaiacum and other woods diſtilled in retorts after the common manner, there may be ſaline parts, which by reaſon of the analogy may pretend to the name of ſome kind of volatile ſalts; yet queſtionleſs there is a great diſparity betwixt ſuch ſalts, and that which we have ſometimes obtained upon the firſt diſtillation of ſoot (though for the moſt part it has not been ſeparated from the firſt or ſecond rectification, and ſometimes not till the third). 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But it is time, that I proceed to mention the particular reaſons, that incline me to doubt, whether the fire be the true and univerſal analyzer of mixt bodies; of which reaſons, what has been already objected may paſs for one.

IN the next place, I obſerve, that there are ſome mixt bodies, from which it has not been yet made appear, that any degree of fire can ſeparate either ſalt, or ſulphur, or mercury, much leſs all the three. The moſt obvious inſtance of this truth is gold, which is a body ſo fixed, and wherein the elementary ingredients (if it have any) are ſo firmly united to each other, that we find not in the operations, wherein gold is expoſed to the fire, how violent ſoever, that it does diſcernably ſo much as loſe of its fixedneſs or weight, ſo far is it from being diſſipated into thoſe principles, whereof one at leaſt is acknowledged to be fugitive enough; and ſo juſtly did the ſpagyrical poet ſomewhere exclaim,

Cuncta adeo miris illic compagibus hærent.

Gaſto Claveus *Apolog. Argur. & Cryſopera.*

AND I muſt not omit on this occaſion to mention to you, *Eleutherius*, the memorable experiment, that I remember I met with in *Gaſto Claveus*, who, though a lawyer by profeſſion, ſeems to have had no ſmall curioſity and experience in chymical affairs. He relates then, that having put into one ſmall earthen veſſel, an ounce of the moſt pure gold, and into another the like weight of pure ſilver, he placed them both in that part of a glaſs-houſe furnace, wherein the workmen keep their metal (as our Engliſh artificers call their liquid glaſs) continually melted; and, that having there kept both the gold and the ſilver in conſtant fuſion for two months together, he afterwards took them out of the furnace and the veſſels, and weighing both of them again, found, that the ſilver had not loſt above a 12th part of its weight, but the gold had not of his loſt any thing at all. And though our author endeavours to give us of this a ſcholaſtic reaſon, which I ſuppoſe you would be as little ſatisfied with, as I was when I read it; yet for the matter of fact, which will ſerve our preſent turn, he aſſures us, that though it be ſtrange, yet experience itſelf taught it him to be moſt true.

AND though there be not perhaps any other body to be found ſo perfectly fixed as gold, yet there are divers others ſo fixed or compoſed, at leaſt of ſo ſtrictly united parts, that I have not yet obſerved the fire to ſeparate from them any one of the chymiſts principles. I need not tell you, what complaints the more candid and judicious of the chymiſts themſelves are wont to make of thoſe boaſters, that confidently pretend, that they have extracted the ſalt or ſulphur of quickſilver, when they have diſguiſed it by additaments, wherewith it reſembles the concretes, whoſe names are given it; whereas by a ſkilful and rigid examen, it may be eaſily enough ſtripped of its diſguiſes, and made to appear again in the priſtine form of running mercury. The pretended ſalts and ſulphurs being ſo far from being elementary parts extracted out of the body of mercury, that they are rather (to borrow a term of the grammarians) de-compound bodies, made up of the whole metal and the menſtruum, or other additaments employed to diſguiſe it. And as for ſilver, I never could ſee any degree of fire make it part with any of its three principles. And though the experiment lately mentioned from *Claveus* may beget a ſuſpicion, that ſilver may be diſſipated by fire, provided it be extremely violent and very laſting; yet it will not neceſſarily follow, that becauſe the fire was able at length to make the ſilver loſe a little of its weight, it was therefore able to diſſipate it into its principles. For firſt I might allege, that I have obſerved little grains of ſilver to lie hid in the ſmall cavities (perhaps glaſſed over by a vitrifying heat) in crucibles, wherein ſilver has been long kept in fuſion, whence ſome goldſmiths of my acquaintance make a benefit by grinding ſuch crucibles to powder, to recover out of them the latent particles of ſilver. And hence I might argue, that perhaps *Claveus* was miſtaken, and imagined, that ſilver to have been driven away by the fire, that indeed lay in minute parts hid in his crucible, in whoſe pores ſo ſmall a quantity, as he miſſed of ſo ponderous a body, might very well lie concealed.

But ſecondly, admitting, that ſome parts of the ſilver were driven away by the violence of the fire, what proof is there, that it was either the ſalt, the ſulphur, or the mercury of the metal, and not rather a part of it homogeneous to what remained? for beſides that the ſilver, that was left, ſeemed not ſenſibly altered, which probably would have appeared, had ſo much of any one of its principles been ſeparated from it; we find in other mineral bodies of a leſs permanent nature than ſilver, that the fire may divide them into ſuch minute parts, as to be able to carry them away with itſelf, without at all deſtroying their nature. Thus we ſee, that in the refining of ſilver, the lead, that is mixed with it (to carry away the copper or other ignoble mineral that embaſſes the ſilver) will, if it be let alone, in time evaporate away upon the teſt; but if (as is moſt uſual amongſt thoſe, that refine great quantities of metals together) the lead be blown off from the ſilver by bellows, that, which would elſe have gone away in the form of unheeded ſteams, will, in great part, be collected not far from the ſilver, in the form of a darkiſh powder or calx; which, becauſe it is blown off from ſilver, they call litharge of ſilver. And thus *Agricola* in divers places informs us, when copper, or the ore of it is colliquated by the violence of the fire with Cadmia, the ſparks, that in great multitudes do fly upwards, do ſome of them ſtick to the vaulted roofs of the furnaces, in the form of little and (for the moſt part) white bubbles; which therefore the Greeks, and, in imitation of them, our drugſters call pompholyx: and others more heavy partly adhere to the ſides of the furnace, and partly (eſpecially if the covers be not kept upon the pots) fall to the ground, and by reaſon of their aſhy colour, as well as weight, were called by the ſame Greeks σποδὸς, which, I need not tell you, in their language ſignifies aſhes. I might add, that I have not found, that from Venetian talc (I ſay Venetian, becauſe I have found other kinds of that mineral more open) from the lapis oſſifragus (which the ſhops call oſtiocolla) from *Muſcovia* glaſs, from pure and fuſible ſand (to mention now no other concretes) thoſe of my acquaintance, that have tried, have been able by the fire to ſeparate any one of the hypoſtatical principles: which you will the leſs ſcruple to believe, if you conſider, that glaſs may be made by the bare colliquation of the ſalt and earth remaining in the aſhes of a burnt plant, and that yet common glaſs, once made, does ſo far reſiſt the violence of the fire, that moſt chymiſts think it a body more undeſtroyable than gold itſelf. For if the artificer can ſo firmly unite ſuch comparative groſs particles as thoſe of earth and ſalt, that make up common aſhes, into a body indiſſoluble by fire, why may not nature aſſociate in divers bodies the more minute elementary corpuſcles, ſhe has at hand, too firmly to let them be ſeparable by the fire? And on this occaſion, *Eleutherius*, give me leave to mention to you two or three ſlight experiments, which will, I hope, be found more pertinent to our preſent diſcourſe, than at firſt perhaps they will appear. The firſt is, that, having (for trial's ſake) put a quantity of that fugitive concrete, camphire, into a glaſs veſſel, and placed it in a gentle heat, I found it (not leaving behind, according to my eſtimate, not ſo much as one grain) to ſublime to the top of the veſſel into flowers; which in whiteneſs, ſmell, &c. ſeemed not to differ from the camphire itſelf. Another experiment is that of *Helmont*, who in ſeveral places affirms, that a coal

Agricola *de Natura Foſſil.* Lib. 9. cap. 11, *and* 12.

kept in a glaſs exactly cloſed will never be calcined to aſhes, though kept never ſo cloſe in a ſtrong fire. To countenance which, I ſhall tell you this trial of my own, that having ſometimes diſtilled ſome woods, as particularly box, whilſt our *caput mortuum* remained in the retort, it continued black like charcoal, though the retort were earthen, and kept red-hot in a vehement fire: but as ſoon as ever it was brought out of the candent veſſel into the open air, the burning coals did haſtily degenerate or fall aſunder, without the aſſiſtance of any new calcination, into pure white aſhes. And to theſe two I ſhall add but this obvious and known obſervation, that common ſulphur (if it be pure and freed from its vinegar) being leiſurely ſublimed in cloſe veſſels, riſes into dry flowers, which may be preſently melted into a body of the ſame nature with that which afforded them. Though, if brimſtone be burnt in the open air, it gives, you know, a penetrating fume, which being caught in a glaſs-bell, condenſes into that acid liquor called oil of ſulphur *per campanam*. The uſe I would make of theſe experiments, collated with what I lately told you out of *Agricola*, is this, that even among the bodies, that are not fixt, there are divers of ſuch a texture, that it will be hard to make it appear, how the fire, as chymiſts are wont to imploy it, can reſolve them into elementary ſubſtances. For ſome bodies being of ſuch a texture, that the fire can drive them into the cooler and leſs hot part of the veſſels, wherein they are included, and, if need be, remove them from place to place, to fly the greateſt heat, more eaſily than it can divorce their elements (eſpecially without the aſſiſtance of the air) we ſee that our chymiſts cannot analyze them in cloſe veſſels, and of other compound bodies the open fire can as little ſeparate the elements. For what can a naked fire do to analyze a mixt body, if its component principles be ſo minute, and ſo ſtrictly united, that the corpuſcles of it need leſs heat to carry them up, than is requiſite to divide them into their principles? So that of ſome bodies the fire cannot in cloſe veſſels make any analyſis at all; and others will in the open air fly away in the forms of flowers or liquors, before the heat can prove able to divide them into their principles. And this may hold, whether the various ſimilar parts of a concrete be combined by nature or by art; for in factitious ſal armoniac we find the common and the urinous ſalts ſo well mingled, that both in the open fire, and in ſubliming veſſels they riſe together, as one ſalt, which ſeems in ſuch veſſels irreſoluble by fire alone. For I can ſhew you ſal armoniac, which after the ninth ſublimation does ſtill retain its compounded nature. And indeed I ſcarce know any one mineral, from which by fire alone chymiſts are wont to ſever any ſubſtance ſimple enough to deſerve the name of an element or principle. For though out of native cinnabar they diſtil quickſilver, and though from many of thoſe ſtones, that the antients called *Pyrites*, they ſublime brimſtone, yet both that quickſilver and this ſulphur, being very often the ſame with the common minerals, that are ſold in the ſhops under thoſe names, are themſelves too much compounded bodies to paſs for the elements of ſuch. And thus much, *Eleutherius*, for the ſecond argument, that belongs to my firſt conſideration; the others I ſhall the leſs inſiſt on, becauſe I have dwelt ſo long upon this.

PROCEED we then, in the next place, to conſider that there are divers ſeparations to be made by other means, which either cannot at all, or elſe cannot ſo well be made by the fire alone. When gold and ſilver are melted into one maſs, it would lay a great obligation upon refiners and goldſmiths, to teach them the art of ſeparating them by the fire, without the trouble and charge they are fain to be at to ſever them. Whereas they may be very eaſily parted by the affuſion of ſpirit of nitre or aqua fortis; which the French therefore call *eau de depart*. So likewiſe the metalline part of vitriol will not be ſo eaſily and conveniently ſeparated from the ſaline part, even by a violent fire, as by the affuſion of certain alkalizate ſalts, in a liquid form, upon the ſolution of vitriol made in common water. For thereby the acid ſalt of the vitriol leaving the copper, it had corroded, to join with the added ſalts, the metalline part will be precipitated to the bottom almoſt like mud. And that I may not give inſtances only in decompound bodies, I will add a not uſeleſs one of another kind. Not only chymiſts have not been able (for aught is vulgarly known) by fire alone to ſeparate true ſulphur from antimony; but though you may find in their books many plauſible proceſſes of extracting it, yet he, that ſhall make as many fruitleſs trials as I have done to obtain it, by moſt of them will, I ſuppoſe, be eaſily perſuaded, that the productions of ſuch proceſſes are antimonial ſulphurs rather in name than nature. But though antimony ſublimed by itſelf is reduced but to a volatile powder, or antimonial flowers, of a compounded nature like the mineral, that affords them; yet I remember, that ſome years ago I ſublimed out of antimony a ſulphur, and that in greater plenty than ever I ſaw obtained from that mineral, by a method, which I ſhall therefore acquaint you with, becauſe chymiſts ſeem not to have taken notice, of what importance ſuch experiments may be in the indagation of the nature, and eſpecially of the number of the elements. Having then purpoſely for trial ſake digeſted eight ounces of good and well powdered antimony with twelve ounces of oil of vitriol, in a well ſtopped glaſs-veſſel, for about ſix or ſeven weeks; and having cauſed the maſs (grown hard and brittle) to be diſtilled in a retort placed in ſand, with a ſtrong fire; we found the antimony to be ſo opened, or altered by the menſtruum, wherewith it had been digeſted, that whereas crude antimony forced up by the fire, ariſes only in flowers, our antimony thus handled afforded us partly in the receiver, and partly in the neck and at the top of the retort, about an ounce of ſulphur, yellow and brittle like common brimſtone, and of ſo ſulphureous a ſmell, that upon the unluting the veſſels it infected the room with a ſcarce ſupportable ſtink. And this ſulphur, beſides the colour and ſmell, had the perfect inflammability of common brimſtone, and would immediately kindle (at the flame of a candle) and burn blue like it. And though it ſeemed, that the long digeſtion, wherein our antimony and menſtruum were detained, did conduce to the better unlocking of the mineral, yet if you have not the leiſure to make ſo long a digeſtion, you may, by incorporating with powdered antimony a convenient quantity of oil of vitriol, and committing them immediately to diſtillation, obtain a little ſulphur like unto the common one, and more combuſtible than, perhaps, you will at firſt take notice of. For I have obſerved, that though (after its being firſt kindled) the flame would ſometimes go out too ſoon of itſelf, if the ſame lump of ſulphur were held again to the flame of a candle, it would be rekindled and burn a pretty while, not only after the ſecond, but after the third or fourth accenſion. You, to whom I think I ſhewed my way of diſcovering ſomething of ſulphureous in oil of vitriol, may perchance ſuſpect, *Eleutherius*, either that this ſubſtance was ſome venereal ſulphur, that lay hid in that liquor, and was by this operation only reduced into a manifeſt body; or elſe, that it was a compound of the unctuous parts of the antimony, and the ſaline ones of the vitriol, in regard, that (as *Gunther* informs us) divers learned men would have ſulphur to be nothing but a mixture, made in the bowels of the earth, of vitriolate ſpirits and a certain combuſtible ſubſtance: but the quantity of ſulphur we obtained by digeſtion was much too great to have been latent in the oil of vitriol. And that vitriolate ſpirits are not neceſſary to the conſtruction of ſuch a ſulphur as ours, I could eaſily manifeſt, if I would acquaint you with the ſeveral ways, by which I have obtained, though not in ſuch plenty, a ſulphur of antimony, coloured and combuſtible like common brimſtone.

Lib. 1. *Obſervat.* cap. 6.

And

And though I am not now minded to diſcover them, yet I ſhall tell you, that to ſatisfy ſome ingenious men, that diſtilled vitriolate ſpirits are not neceſſary to the obtaining of ſuch a ſulphur as we have been conſidering, I did by the bare diſtillation of only ſpirit of nitre, from its weight of crude antimony, ſeparate, in a ſhort time, a yellow and very inflammable ſulphur; which, for aught I know, deſerves as much the name of an element, as any thing, that chymiſts are wont to ſeparate from any mineral by the fire. I could perhaps tell you of other operations upon antimony, whereby that may be extracted from it, which cannot be forced out of it by the fire; but I ſhall reſerve them for a fitter opportunity, and only annex at preſent this ſlight but not impertinent experiment: that whereas I lately obſerved to you, that the urinous and common ſalts, whereof ſal armoniac conſiſts, remained unſevered by the fire in many ſucceſſive ſublimations, they may be eaſily ſeparated, and partly without any fire at all, by pouring upon the concrete finely powdered, a ſolution of ſalt of tartar, or of the ſalt of wood-aſhes: for upon your diligently mixing of theſe, you will find your noſe invaded with a very ſtrong ſmell of urine, and perhaps too your eyes forced to water, by the ſame ſubtile and piercing body, that produces the ſtink; both theſe effects proceeding from hence, that by the alcalizate ſalt, the ſea ſalt, that entered the compoſition of the ſal armoniac, is mortified and made more fixt, and thereby a divorce is made between it and the volatile urinous ſalt; which being at once ſet at liberty, and put into motion, begins preſently to fly away, and to offend the noſtrils and eyes it meets with by the way. And if the operation of theſe ſalts be in convenient glaſſes promoted by warmth, though but by that of a bath, the aſcending ſteams may eaſily be caught and reduced into a penetrant ſpirit, abounding with a ſalt, which I have ſometimes found to be ſeparable in a cryſtalline form. I might add to theſe inſtances, that whereas ſublimate, conſiſting, as you know, of ſalts and quickſilver combined and carried up together by heat, may be ſublimed, I know not how often, by a like degree of fire, without ſuffering any divorce of the component bodies, the mercury may be eaſily ſevered from the adhering ſalts, if the ſublimate be diſtilled from ſalt of tartar, quick lime, or ſuch alkalizate bodies. But I will rather obſerve to you, *Eleutherius*, what divers ingenious men have thought ſomewhat ſtrange; that by ſuch an additament, that ſeems but only to promote the ſeparation, there may be eaſily obtained from a concrete, that by the fire alone is eaſily diviſible into all the elements, that vegetables are ſuppoſed to conſiſt of, ſuch a ſimilar ſubſtance, as differs in many reſpects from them all, and conſequently has by many of the moſt intelligent chymiſts been denied to be contained in the mixt body. For I know a way, and have practiſed it, whereby common tartar, without the addition of any thing, that is not perfectly a mineral, except ſalt-petre, may by one diſtillation in an earthern retort be made to afford good ſtore of real ſalt readily diſſoluble in water; which I found to be neither acid, nor of the ſmell of tartar, and to be almoſt as volatile as ſpirit of wine itſelf, and to be indeed of ſo differing a nature from all, that is wont to be ſeparated by fire from tartar; that divers learned men, with whom I diſcourſed of it, could hardly be brought to believe, that ſo fugitive a ſalt could be afforded by tartar, till I aſſured it them upon my own knowledge. And if I did not think you apt to ſuſpect me to be rather too backward than too forward to credit or affirm unlikely things, I could convince you, by what I have yet lying by me, of that anomalous ſalt.

THE fourth thing, that I ſhall allege to countenance my firſt conſideration, is, that the fire, even when it divides a body into ſubſtances of divers conſiſtencies, does not moſt commonly analyze it into hypoſtatical principles, but only diſpoſes its parts into new textures, and thereby produces concretes of a new indeed, but yet of a compound nature. This argument it will be requiſite for me to proſecute ſo fully hereafter, that I hope you will then confeſs, that it is not for want of good proofs, that I deſire leave to ſuſpend my proofs, till the ſeries of my diſcourſe ſhall make it more proper and ſeaſonable to propoſe them.

IT may be further alleged on the behalf of my firſt conſideration, that ſome ſuch diſtinct ſubſtances may be obtained from ſome concretes without fire, as deſerve no leſs the name of elementary, than many, that chymiſts extort by the violence of the fire.

WE ſee, that the inflammable ſpirit, or, as the chymiſts eſteem it, the ſulphur of wine, may not only be ſeparated from it by the gentle heat of a bath, but may be diſtilled either by the help of the ſun-beams, or even of a dunghill, being indeed of ſo fugitive a nature, that it is not eaſy to keep it from flying away, even without the application of external heat. I have likewiſe obſerved, that a veſſel full of urine being placed in a dunghill, the putrefaction is wont, after ſome weeks, ſo to open the body, that the parts diſbanding the ſaline ſpirit, will within no very long time, if the veſſel be not ſtopped, fly away of itſelf; inſomuch, that from ſuch urine I have been able to diſtil little or nothing elſe than a nauſeous phlegm, inſtead of the active and piercing ſalt and ſpirit, that it would have afforded, when firſt expoſed to the fire, if the veſſel had been carefully ſtopped.

AND this leads me to conſider, in the fifth place, that it will be very hard to prove, that there can no other body or way be given, which will, as well as the fire, divide concretes into ſeveral homogeneous ſubſtances, which may conſequently be called their elements or principles, as well as thoſe ſeparated or produced by the fire. For ſince we have lately ſeen, that nature can ſucceſsfully employ other inſtruments than the fire to ſeparate diſtinct ſubſtances from mixed bodies, how know we, but that nature has made, or art may make, ſome ſuch ſubſtance, as may be a fit inſtrument to analyze mixt bodies, or that ſome ſuch method may be found by human induſtry or luck, by whoſe means compound bodies may be reſolved into other ſubſtances, than ſuch as they are wont to be divided into by the fire? And why the products of ſuch an analyſis may not as juſtly be called the component principles of the bodies, that afford them, it will not be eaſy to ſhew; eſpecially ſince I ſhall hereafter make it evident, that the ſubſtances, which chymiſts are wont to call the ſalts, and ſulphurs, and mercuries of bodies, are not ſo pure and elementary as they preſume, and as their hypotheſis requires. And this may therefore be the more freely preſſed upon the chymiſts, becauſe neither the Paracelſians, nor the Helmontians, can reject it without apparent injury to their reſpective maſters. For *Helmont* does more than once inform his readers, that both *Paracelſus* and himſelf were poſſeſſors of the famous liquor, alkaheſt, which for its great power in reſolving bodies irreſoluble by vulgar fires, he ſomewhere ſeems to call *Ignis Gehennæ*. To this liquor he aſcribes (and that in great part upon his own experience) ſuch wonders, that if we ſuppoſe them all true, I am ſo much the more a friend to knowledge than to wealth, that I ſhould think the alkaheſt a nobler and more deſirable ſecret, than the philoſophers ſtone itſelf. Of this univerſal diſſolvent he relates, that having digeſted with it for a competent time a piece of oaken charcoal, it was thereby reduced into a couple of new and diſtinct liquors, diſcriminated from each other by their colour and ſituation, and that the whole body of the coal was reduced into thoſe liquors, both of them ſeparable from his immortal menſtruum, which remained as fit for ſuch operations as before. And he moreover tells us in divers places of his writings, that by his powerful, and unwearied

wearied agent, he could diſſolve metals, marchaſites, ſtones, vegetable and animal bodies of what kind ſoever, and even glaſs itſelf (firſt reduced to powder) and in a word, all kind of mixt bodies in the world, into their ſeveral ſimilar ſubſtances without any reſidence or *caput mortuum.* And laſtly, we may gather this further from his informations, that the homogeneous ſubſtances obtainable from compound bodies by his piercing liquor, were oftentimes different enough both as to number and as to nature, from thoſe into which the ſame bodies are wont to be divided by common fire. Of which I ſhall need in this place to mention no other proof, than that whereas we know, that in our common analyſis of a mixt body, there remains a terreſtrial and very fixed ſubſtance, oftentimes aſſociated with a ſalt as fixed; our author tells us, that by his way he could diſtil over all concretes without any *caput mortuum*, and conſequently could make thoſe parts of the concrete volatile, which in the vulgar analyſis would have been fixt. So that if our chymiſts will not reject the ſolemn and repeated teſtimony of a perſon, who cannot but be acknowledged for one of the greateſt ſpagyriſts, that they can boaſt of, they muſt not deny, that there is to be found in nature another agent able to analyze compound bodies leſs violently, and both more genuinely and more univerſally than the fire. And for my own part, though I cannot but ſay on this occaſion what (you know) our friend Mr. *Boyle* is wont to ſay, when he is aſked his opinion of any ſtrange experiment, *That he, that hath ſeen it, hath more reaſon to believe it, than he that hath not*; yet I have found *Helmont* ſo faithful a writer, even in divers of his improbable experiments (I always except that extravagant treatiſe *De Magnetica Vulnerum Curatione*, which ſome of his friends affirm to have been firſt publiſhed by his enemies) that I think it ſomewhat harſh to give him the lye, eſpecially to what he delivers upon his own proper trial. And I have heard from very credible eye-witneſſes ſome things, and ſeen ſome others myſelf, which argue ſo ſtrongly, that a circulated ſalt, or a menſtruum (ſuch as it may be) may, by being abſtracted from compound bodies, whether mineral, animal, or vegetable, leave them more unlocked than a wary naturaliſt would eaſily believe; that I dare not confidently meaſure the power of nature and art by that of the menſtruums, and other inſtruments that eminent chymiſts themſelves are as yet wont to employ about the analyzing of bodies; nor deny, that a menſtruum may at leaſt from this or that particular concrete obtain ſome apparently ſimilar ſubſtance, differing from any obtainable from the ſame body by any degree or manner of application of the fire. And I am the more backward to deny peremptorily, that there may be ſuch openers of compound bodies, becauſe among the experiments, that make me ſpeak thus warily, there wanted not ſome, in which it appeared not, that one of the ſubſtances, not ſeparable by common fires and menſtruums, could retain any thing of the ſalt, by which the ſeparation was made.

AND here, *Eleutherius* (ſays *Carneades*) I ſhould conclude as much of my diſcourſe as belongs to the firſt conſideration I propoſed, but that I foreſee, that what I have delivered will appear liable to two ſuch ſpecious objections, that I cannot ſafely proceed any further, till I have examined them.

AND firſt, one ſort of oppoſers will be forward to tell me, that they do not pretend by fire alone to ſeparate out of all compound bodies their hypoſtatical principles; it being ſufficient, that the fire divides them into ſuch, though afterwards they employ other bodies to collect the ſimilar parts of the compound; as it is known, that though they make uſe of water, to collect the ſaline parts of aſhes from the terreſtrial, wherewith they are blended, yet it is the fire only, that incinerates bodies, and reduced the fixed part of them into the ſalt and earth, whereof aſhes are made up.

This objection is not, I confeſs, inconſiderable, and I might in great part allow of it, without granting it to make againſt me, if I would content myſelf to anſwer, that it is not againſt thoſe, that make it, that I have been diſputing, but againſt thoſe vulgar chymiſts, who themſelves believe, and would fain make others do ſo, that the fire is not only an univerſal, but an adequate and ſufficient inſtrument to analyze mixt bodies with. For as to their practice of extracting the fixed ſalt out of aſhes by the affuſion of water, it is obvious to allege, that the water does only aſſemble together the ſalt the fire had before divided from the earth: as a ſieve does not further break the corn, but only bring together into two diſtinct heaps the flower and the bran, whoſe corpuſcles before lay promiſcuouſly blended together in the meal. This I ſay I might allege, and thereby exempt myſelf from the need of taking any farther notice of the propoſed objection. But not to loſe the riſe it may afford me of illuſtrating the matter under conſideration, I am content briefly to conſider it, as far forth as my preſent diſquiſition may be concerned in it.

NOT to repeat then what has been already anſwered, I ſay further, that though I am ſo civil an adverſary, that I will allow the chymiſts, after the fire has done all its work, the uſe of fair water to make their extractions with, in ſuch caſes wherein the water does not co-operate with the fire to make the analyſis; yet ſince I grant this but upon ſuppoſition, that the water does only waſh off the ſaline particles, which the fire alone has before extricated in the analyzed body, it will not be reaſonable, that this conceſſion ſhould extend to other liquors, that may add to what they diſſolve, nor ſo much as to other caſes than thoſe newly mentioned: which limitation I deſire you would be pleaſed to bear in mind, till I ſhall anon have occaſion to make uſe of it. And this being thus premiſed, I ſhall proceed to obſerve,

FIRST, that many of the inſtances I propoſed in the preceding diſcourſe are ſuch, that the objection we are conſidering will not at all reach them. For fire can no more with the aſſiſtance of water, than without it, ſeparate any of the three principles, either from gold, ſilver, mercury, or ſome other of the concretes named above.

HENCE we may infer, that fire is not an univerſal analyzer of all mixt bodies, ſince of metals and minerals, wherein chymiſts have moſt exerciſed themſelves, there appear ſcarce any, which they are able to analyze by fire, nay, from which they can unqueſtionably ſeparate ſo much as any one of their hypoſtatical principles; which may well appear no ſmall diſparagement, as well to their hypotheſis, as to their pretenſions.

IT will alſo remain true, notwithſtanding the objection, that there may be other ways, than the wonted analyſis by fire, to ſeparate from a compound body ſubſtances as homogeneous as thoſe, that chymiſts ſcruple not to reckon among their *tria prima*, (as ſome of them, for brevity ſake, call their three principles.)

AND it appears, that by convenient additaments ſuch ſubſtances may be ſeparated by the help of the fire, as could not be ſo by the fire alone: witneſs the ſulphur of antimony.

AND laſtly, I muſt repreſent, that ſince it appears too, that the fire is but one of the inſtruments, that muſt be employed in the reſolution of bodies, we may reaſonably challenge the liberty of doing two things. For whenever any menſtruum or other additament is employed, together with the fire, to obtain a ſulphur or a ſalt from a body, we may well take the freedom to examine, whether or no that menſtruum do barely help to ſeparate the principle obtained by it, or whether there intervene not a coalition of the parts of the body wrought upon with thoſe of the menſtruum, whereby the produced concrete may be judged to reſult from the union of both. And it will be farther allowable for us to conſider, how far any ſubſtance, ſeparated by the help of ſuch additaments, ought to paſs for one of the *tria prima*; ſince by one way of handling the ſame mixt body, it may, according to the nature of the additaments, and the method of working upon it, be made to afford differing ſubſtances from it by other additaments, and another method, nay and (as may appear by what I formerly told you about tartar) differing from any of the ſubſtances, into which a concrete is diviſible by the fire without additaments, though, perhaps, thoſe additaments do not, as ingredients, enter the compoſition of the obtained body, but only diverſify the operation of the fire upon the concrete; and though that concrete by the fire alone may be divided into a number of differing ſubſtances, as great as any of the chymiſts, that I have met with, teach us, that of the elements to be. And having ſaid thus much (ſays *Carneades*) to the objection likely to be propoſed by ſome chymiſts, I am now to examine that, which I foreſee will be confidently preſſed by divers Peripatetics, who, to prove fire to be the true analyzer of bodies, will plead, That it is the very definition of heat given by *Ariſtotle*, and generally received, *congregare homogenea, & ſegregare heterogenea*, 'to aſſemble things of a reſembling, and 'disjoin thoſe of a differing nature.' To this I anſwer, that this effect is far from being ſo eſſential to heat, as it is generally imagined; for it rather ſeems, that the true and genuine property of heat is, to ſet a moving, and thereby to diſſociate the parts of bodies, and ſubdivide them into minute particles, without regard to their being homogeneous or heterogeneous; as is apparent in the boiling of water, the diſtillation of quickſilver, or the expoſing of bodies to the action of the fire, whoſe parts either are not (at leaſt in that degree of heat appear not) diſſimilar, where, all that the fire can do, is to divide the body into very minute parts, which are of the ſame nature with one another, and with their totum, as their reduction by condenſation evinces. And even when the fire ſeems moſt ſo *congregare homogenea, & ſegregare heterogenea*, it produces that effect but by accident; for the fire does but diſſolve the cement, or rather ſhatter the frame, or ſtructure, that kept the heterogeneous parts of bodies together, under the common form; upon which diſſolution the component particles of the mixt, being freed and ſet at liberty, do naturally, and oftentimes without any operation of the fire, aſſociate themſelves each with its like, or rather do take thoſe places, which their ſeveral degrees of gravity and levity, fixedneſs or volatility (either natural, or adventitious from the impreſſion of the fire) aſſign them. Thus in the diſtillation (for inſtance) of man's blood, the fire does firſt begin to diſſolve the nexus or cement of the body; and then the water, being the moſt volatile, and eaſy to be extracted, is either by the igneous atoms, or the agitation they are put into by the fire, firſt carried up, till forſaken by what carried it up, its weight ſinks it down, into the receiver: but all this while the other principles of the concrete remain unſevered, and require a ſtronger degree of heat to make a ſeparation of its more fixt elements; and therefore the fire muſt be increaſed, which carries over the volatile ſalt and the ſpirit, they being, though believed to be differing principles, and though really of different conſiſtency, yet of an almoſt equal volatility. After them, as leſs fugitive, comes over the oil, and leaves behind the earth and the alcali, which being of an equal fixedneſs, the fire ſevers them not, for all the definition of the ſchools. And if into a red-hot earthen or iron retort you caſt the matter to be diſtilled, you may obſerve, as I have often done, that the predominant fire will carry up all the volatile elements confuſedly in one fume, which will afterwards take their places in the receiver, either according to the degree of their gravity, or according to the exigency of their reſpective textures; the ſalt adhering, for the moſt part, to the ſides and top, and the phlegm faſtening itſelf there too in great drops, the oil and ſpirit placing themſelves under, or above one another, according as their ponderouſneſs makes them ſwim or ſink. For it is obſervable, that though oil or liquid ſulphur be one of the elements ſeparated by this fiery analyſis, yet the heat, which accidentally unites the particles of the other volatile principles, has not always the ſame operation on this, there being divers bodies, which yield two oils, whereof the one ſinks to the bottom of that ſpirit, on which the other ſwims; as I can ſhew you in ſome oils of the ſame deer's blood, which are yet by me: nay, I can ſhew you two oils carefully made of the ſame parcel of human blood, which not only differ extremely in colour, but ſwim upon one another without mixture, and, if by agitation confounded, will of themſelves divorce again.

AND that the fire doth oftentimes divide bodies, upon the account, that ſome of their parts are more fixt, and ſome more volatile, how far ſoever either of theſe two may be from a pure elementary nature, is obvious enough, if men would but heed it in the burning of wood, which the fire diſſipates into ſmoke and aſhes: for not only the latter of theſe is confeſſedly made up of two ſuch differing bodies as earth and ſalt; but the former being condenſed into that ſoot, which adheres to our chimneys, diſcovers itſelf to contain both ſalt and oil, and ſpirit and earth (and ſome portion of phlegm too) which being, all almoſt, equally volatile to that degree of fire, which forces them up, (the more volatile parts helping, perhaps, as well as the urgency of the fire, to carry up the more fixt ones, as I have often tried in dulcified colcothar, ſublimed by ſal armoniac blended with it) are carried up together, but may afterwards be ſeparated by other degrees of fire, whoſe orderly gradation allows the diſparity of their volatileneſs to diſcover itſelf. Beſides, if differing bodies united into one maſs be both ſufficiently fixt, the fire finding no parts volatile enough to be expelled or carried up, makes no ſeparation at all; as may appear by a mixture of colliquated ſilver and gold, whoſe component metals may be eaſily ſever'd by aqua fortis or aqua regis (according to the predominancy of the ſilver and gold) but in the fire alone, though vehement, the metals remain unſever'd, the fire only dividing the body into ſmaller particles (whoſe littleneſs may be argued from their fluidity) in which either the little nimble atoms of fire, or its briſk and numberleſs ſtrokes upon the veſſels, hinder reſt and continuity, without any ſequeſtration of elementary principles. Moreover, the fire ſometimes does not ſeparate, ſo much as unite, bodies of a differing nature; provided they be of an almoſt reſembling fixedneſs, and have in the figure of their parts an aptneſs to coalition, as we ſee in the making of many plaiſters, ointments, *&c.* And in ſuch metalline mixtures as that made by melting together two parts of clean braſs with one of pure copper, of which ſome ingenious tradeſmen caſt ſuch curious patterns (for gold and ſilver works) as I have ſometimes taken great pleaſure to look upon. Sometimes the bodies mingled by the fire are differing enough as to fixidity and volatility, and yet are ſo combined by the firſt operation of the fire, that itſelf does ſcarce afterwards ſeparate them, but only pulverize them; whereof an inſtance is afforded us by the common preparation of mercurius dulcis, where the ſaline particles of the vitriol, ſea-ſalt, and ſometimes nitre, employed to make the ſublimate, do ſo unite themſelves with the mercurial particles made uſe of, firſt to make ſublimate, and then to dulcify it, that the ſaline and metalline parts ariſe together in many ſucceſſive ſublimations, as if they all made but one body. And ſometimes too, the fire does not only not ſever the differing elements of a body, but combine them ſo firmly, that nature herſelf does very ſeldom, if ever,

ever, make unions leſs diſſoluble. For the fire meeting with ſome bodies exceedingly and almoſt equally fixt, inſtead of making a ſeparation, makes an union ſo ſtrict, that itſelf, alone, is unable to diſſolve it; as we ſee, when an alcalizate ſalt and the terreſtrial reſidue of the aſhes are incorporated with pure ſand, and by vitrification made one permanent body, (I mean the coarſe or greeniſh ſort of glaſs) that mocks the greateſt violence of the fire, which though able to marry the ingredients of it, yet is not able to divorce them. I can ſhew you ſome pieces of glaſs, which I ſaw flow down from an earthen crucible purpoſely expoſed for a good while, with ſilver in it, to a very vehement fire. And ſome that deal much in the fuſion of metals inform me, that the melting of a great part of a crucible into glaſs is no great wonder in their furnaces. I remember, I have obſerved too in the melting of great quantities of iron out of the ore, by the help of ſtore of charcoal (for they affirm that ſea-coal will not yield a flame ſtrong enough) that by the prodigious vehemence of the fire, excited by vaſt bellows (made to play by great wheels turned about by water) part of the materials expoſed to it, was, inſtead of being analized, colliquated, and turned into a dark, ſolid, and very ponderous glaſs, and that in ſuch quantity, that in ſome places I have ſeen the very high-ways, near ſuch iron-works, mended with heaps of ſuch lumps of glaſs, inſtead of ſtones and gravel. And I have alſo obſerved, that ſome kind of fire-ſtone itſelf, having been employed in furnaces wherein it was expoſed to very ſtrong and laſting fires, has had all its fixed parts ſo wrought on by the fire, as to be perfectly vitrified, which I have tried by forcing from it pretty large pieces of perfect and tranſparent glaſs. And leſt you might think *Eleutherius*, that the queſtioned definition of heat may be demonſtrated, by the definition, which is wont to be given and acquieſced in, of its contrary quality, cold; whoſe property is taught to be *tam homogenea, quam heterogenea congregare*, give me leave to repreſent to you, that neither is this definition unqueſtionable; for not to mention the exceptions, which a logician, as ſuch, may take at it, I conſider, that the union of heterogeneous bodies, which is ſuppoſed to be the genuine production of cold, is not performed by every degree of cold. For we ſee, for inſtance, that in the urine of healthy men, when the liquor has been ſuffered a while to ſtand, the cold makes a ſeparation of the thinner part from the groſſer, which ſubſides to the bottom, and grows opacous there; whereas, if the urinal be warm, theſe parts readily mingle again, and the whole liquor becomes tranſparent as before. And when, by glaciation, wood, ſtraw, duſt, water, *&c.* are ſuppoſed to be united into one lump of ice, the cold does not cauſe any real union or adonation (if I may ſo ſpeak) of theſe bodies, but only hardening the aqueous parts of the liquor into ice, the other bodies, being accidentally preſent in that liquor, are frozen up in it, but not really united. And accordingly, if we expoſe a heap of money, conſiſting of gold, ſilver and copper coins, or any other bodies of differing natures, which are deſtitute of aqueous moiſture, capable of congelation, to never ſo intenſe a cold, we find not, that theſe differing bodies are at all thereby ſo much as compacted, much leſs united together; and even in liquors themſelves we find phænomena, which induce us to queſtion the definition, which we are examining. If *Paracelſus* his authority were to be looked upon as a ſufficient proof in matters of this nature, I might here inſiſt on that proceſs of his, whereby he teaches, that the eſſence of wine may be ſever'd from the phlegm and ignoble part by the aſſiſtance of congelation. And becauſe much weight has been laid upon this proceſs, not only by Paracelſians, but other writers, ſome of whom ſeem not to have peruſed it themſelves, I ſhall give you the intire paſſage in the author's own words, as I lately found them in the ſixth book of his

Archidoxis, an extract whereof I have yet about me; and it ſounds thus: *De vino ſciendum eſt, fæcem phlegmaque ejus eſſe mineram, & vini ſubſtantiam eſſe corpus, in quo conſervatur eſſentia, prout auri in auro latet eſſentia. Juxta quod practicam nobis ad memoriam ponimus, ut non obliviſcamur, ad hunc modum: Recipe Vinum vetuſtiſſimum & optimum, quod habere poteris, calore ſaporeque ad placitum; hoc in vas vitreum infundas, ut tertiam ejus partem impleat, & ſigillo Hermetis occluſum in equino ventre menſibus quatuor, & in continuato calore teneatur, qui non deficiat. Quo peracto, hyeme, cum frigus & gelu maxime ſæviunt, his permenſem exponatur, ut congeletur. Ad hunc modum frigus vini ſpiritum una cum ejus ſubſtantia protrudit in vini centrum, ac ſeparat a phlegmate: congelatum abjice; quod vero congelatum non eſt, id ſpiritum cum ſubſtantia eſſe judicato. Hunc in pelicanum poſitum in arenæ digeſtione non adeo calida per aliquod tempus manere ſinito; poſtmodum eximito vini magiſterium, de quo locuti ſumus.*

BUT I dare not, *Eleutherius*, lay much weight upon this proceſs, becauſe I have have found, that, if it were true, it would be but ſeldom practicable in this country upon the beſt wine: for though this preſent winter hath been extraordinary cold, yet in very keen froſts, accompanied with laſting ſnows, I have not been able in any meaſure to freeze a thin phial full of ſack; and even with ſnow and ſalt I could freeze little more than the ſurface of it; and I ſuppoſe, *Eleutherius*, that it is not every degree of cold that is capable of congealing liquors, which is able to make ſuch an analyſis (if I may ſo call it) of them by ſeparating their aqueous and ſpirituous parts: for I have ſometimes, though not often, frozen ſeverally red-wine, urine and milk, but could not obſerve the expected ſeparation. And the Dutchmen, that were forced to winter in that icy region near the arctick circle, called *Nova Zembla*, although they relate, as we ſhall ſee below, that there was a ſeparation of parts made in their frozen beer about the middle of *November*, yet of the freezing of their ſack in *December* following they give but this account: *Yea and our ſack, which is ſo hot, was frozen very hard, ſo that when we were every man to have his part, we were forced to melt it in the fire; which we ſhared every ſecond day, about half a pint for a man, wherewith we were forced to ſuſtain ourſelves.* In which words they imply not, that their ſack was divided by the froſt into differing ſubſtances, after ſuch manner as their beer had been. All which notwithſtanding, *Eleutherius*, ſuppoſe that it may be made to appear, that even cold ſometimes may *congregare homogenea, & heterogenea ſegregare*; and to manifeſt this, I may tell you, that I did once, purpoſely, cauſe to be decocted in fair water a plant abounding with ſulphureous and ſpirituous parts; and having expoſed the decoction to a keen North-wind in a very froſty night, I obſerved, that the more aqueous parts of it were turned by the next morning into ice, towards the innermoſt part of which, the more agile and ſpirituous parts, as I then conjectured, having retreated, to ſhun, as much as might be, their invironing enemy, they had there preſerved themſelves unfrozen in the form of a high coloured liquor; the aqueous and ſpirituous parts having been ſo ſlightly (blended rather than) united in the decoction, that they were eaſily ſeparable by ſuch a degree of cold, as would not have been able to have divorced the parts of urine or wine, which by fermentation or digeſtion are wont, as trial has informed me, to be more intimately aſſociated each with other. But I have already intimated, *Eleutherius*, that I ſhall not inſiſt on this experiment, not only becauſe, having made it but once, I may poſſibly have been miſtaken in it; but alſo (and that principally) becauſe of that much more full and eminent experiment of the ſeparative virtue of extreme cold, that was made againſt their wills, by the formentioned Dutchmen that wintered in *Nova Zembla*; the relation of whoſe voyage being a very ſcarce book, it will not be amiſs to give you that memorable part of it, which concerns our preſent theme, as I cauſed the paſſage to be extracted out of the Engliſhed voyage itſelf.

'GERARD DE VEER, *John Cornelyſon* and others, ſent out of *Amſterdam*, *Anno* 'Dom. 1596, being forced by unſeaſonable weather to winter in *Nova Zembla*, near '*Ice-haven*; on the thirteenth of *October*, three of us (ſays the relation) went aboard 'the ſhip, and laded a ſled with beer; but when we had laden it, thinking to go to 'our houſe with it, ſuddenly there aroſe ſuch a wind, and ſo great a ſtorm and cold, 'that we were forced to go into the ſhip again, becauſe we were not able to ſtay 'without; and we could not get the beer into the ſhip again, but were forced to let 'it ſtand without upon the ſled. The fourteenth, as we came out of the ſhip, we 'found the barrel of beer ſtanding upon the ſled, but it was faſt frozen at the heads; 'yet by reaſon of the great cold, the beer, that purged out, froze as hard upon the 'ſide of the barrel, as if it had been glued thereon: and in that ſort we drew it to 'our houſe, and ſet the barrel an end, and drank it up; but firſt we were forced 'to melt the beer, for there was ſcarce any unfrozen beer in the barrel: but in that 'thick yeaſt, that was unfrozen, lay the ſtrength of the beer, ſo that it was too 'ſtrong to drink alone, and, that which was frozen taſted like water; and being 'melted, we mixed one with the other, and ſo drank it; but it had neither ſtrength 'nor taſte.'

AND on this occaſion I remember, that having, the laſt very ſharp winter, purpoſely tried to freeze, among other liquors, ſome beer moderately ſtrong, in glaſs veſſels, with ſnow and ſalt, I obſerved, that there came out of the neck a certain thick ſubſtance, which, it ſeems, was much better able than the reſt of the liquor (that I found turned into ice) to reſiſt a froſt; and which, by its colour and conſiſtence, ſeemed manifeſtly enough to be yeaſt; whereat, I confeſs, I ſomewhat marvelled, becauſe I did not either diſcern by the taſte, or find by enquiry, that the beer was at all too new, to be very fit to be drank. I might confirm the Dutchmen's relation, by what happened a while ſince to a near friend of mine, who complained to me, that having brewed ſome beer or ale for his own drinking in *Holland* (where he then dwelt) the keenneſs of the late bitter winter froze the drink, ſo as to reduce it into ice, and a ſmall proportion of a very ſtrong and ſpirituous liquor. But I muſt not entertain you any longer concerning cold, not only becauſe you may think I have but loſt my way into a theme, which does not directly belong to my preſent undertaking; but becauſe I have already enlarged myſelf too much, upon the firſt conſideration I propoſed, though it appears ſo much a paradox, that it ſeemed to require, that I ſhould ſay much to keep it from being thought a meer extravagance: yet ſince I undertook but to make the common aſſumption of our Chymiſts and Ariſtotelians appear queſtionable, I hope I have ſo performed that taſk, that I may now proceed to my following conſiderations, and inſiſt leſs on them than I have done on the firſt.

THE

SCEPTICAL CHYMIST:

PART II.

THE ſecond conſideration I deſire to have notice taken of, is this; that it is not ſo ſure, as both Chymiſts and Ariſtotelians are wont to think it, that every ſeemingly ſimilar or diſtinct ſubſtance, that is ſeparated from a body by the help of the fire, was pre-exiſtent in it, as a principle or element of it.

THAT I may not make this paradox a greater than needs muſt, I will firſt briefly explain what the propoſition means, before I proceed to argue for it.

AND, I ſuppoſe, you will eaſily believe, that I do not mean, that any thing is ſeparable from a body by fire, that was not materially pre-exiſtent in it: for it far exceeds the power of meerly natural agents, and conſequently of the fire, to produce anew ſo much as one atom of matter, which they can but modify and alter, not create; which is ſo obvious a truth, that almoſt all ſects of philoſophers have denied the power of producing matter to ſecond cauſes, and the Epicureans and ſome others have done the like, in reference to their gods themſelves.

NOR does the propoſition peremptorily deny, but that ſome things obtained by the fire from a mixed body may have been more than barely materially pre-exiſtent in it; ſince there are concretes, which before they be expoſed to the fire, afford us ſeveral documents of their abounding, ſome with ſalt, and others with ſulphur. For it will ſerve the preſent turn, if it appear, that divers things obtained from a mixed body expoſed to the fire were not its ingredients before; for, if this be made to appear, it will be rational enough to ſuſpect, that chymiſts may deceive themſelves, and others, in concluding reſolutely and univerſally, thoſe ſubſtances to be the elementary ingredients of bodies barely ſeparated by the fire, of which it yet may be doubted, whether there be ſuch or no; at leaſt till ſome other argument, than that drawn from the analyſis, be brought to reſolve the doubt.

THAT then, which I mean by the propoſition I am explaining, is, that it may without abſurdity be doubted, whether or no the differing ſubſtances obtainable from a concrete diſſipated by the fire were ſo exiſtent in it, in that form (at leaſt, as to their minute parts) wherein we find them, when the analyſis is over, that the fire did only disjoin and extricate the corpuſcles of one principle from thoſe of the other, wherewith before they were blended.

HAVING thus explained my propoſition, I ſhall endeavour to do two things, to prove it; the firſt of which is to ſhew, that ſuch ſubſtances, as chymiſts call principles, may be produced *de novo* (as they ſpeak.) And the other is to make it probable, that by the fire we may actually obtain from ſome mixed bodies ſuch ſubſtances, as were not, in the newly-expounded ſenſe, pre-exiſtent in them.

To begin then with the firſt of theſe, I conſider, that if it be as true, as it is probable, that compounded bodies differ from one another but in the various textures,

tures reſulting from the bigneſs, ſhape, motion, and contrivance of their ſmall parts, it will not be irrational to conceive, that one and the ſame parcel of the univerſal matter may, by various alterations and contextures, be brought to deſerve the name, ſometimes of ſulphureous, and ſometimes of a terrene, or aqueous body. And this I could more largely explicate, but that our friend Mr. *Boyle* has promiſed us ſomething about qualities, wherein the theme I now willingly reſign him, will, I queſtion not, be ſtudiouſly inquired into. Wherefore, what I ſhall now advance in favour of what I have lately delivered, ſhall be deduced from experiments made divers years ſince. The firſt of which would have been much more conſiderable, but that by ſome intervening accidents I was neceſſitated to loſe the beſt time of the year, for a trial of the nature of that I deſigned; it being about the middle of *May*, before I was able to begin an experiment, which ſhould have then been two months old; but ſuch as it was, it will not, perhaps, be impertinent to give you this narrative of it. At the time newly mentioned, I cauſed my gardener (being by urgent occaſions hindered from being preſent myſelf) to dig out a convenient quantity of good earth, and dry it well in an oven, to weigh it, to put in an earthen pot almoſt level with the ſurface of the ground, and to ſet in it a ſelected ſeed, he had before received from me, for that purpoſe, of ſquaſh, which is an Indian kind of pompion, that grows apace; this ſeed I ordered him to water only with rain or ſpring water. I did not (when my occaſions permitted me to viſit it) without delight behold how faſt it grew, though unſeaſonably ſown; but the haſtening winter hindered it from attaining any thing near its due and wonted magnitude, (for I found the ſame autumn, in my garden, ſome of thoſe plants, by meaſure, as big about as my middle) and made me order the having it taken up; which about the middle of *October* was carefully done by the ſame gardener, who a while after ſent me this account of it: *I have weighed the pompion with the ſtalk and leaves, all which weighed three pound wanting a quarter; then I took the earth, baked it as formerly, and found it juſt as much as I did at firſt, which made me think, I had not dried it ſufficiently. Then I put it into the oven twice more, after the bread was drawn, and weighed it the ſecond time, but found it ſhrink little or nothing.*

BUT to deal candidly with you, *Eleutherius*, I muſt not conceal from you the event of another experiment of this kind made this preſent ſummer, wherein the earth ſeems to have been much more waſted; as may appear by the following account, lately ſent me by the ſame gardener, in theſe words: *To give you an account of your cucumbers, I have gained two indifferent fair ones, the weight of them is ten pound and a half, the branches with the roots weighed four pounds wanting two ounces; and when I had weighed them, I took the earth, and baked it in ſeveral ſmall earthen diſhes in an oven; and when I had ſo done, I found the earth wanted a pound and a half of what it was formerly; yet I was not ſatisfied, doubting the earth was not dry: I put it into an oven the ſecond time, (after the bread was drawn) and after I had taken it out and weighed it, I found it to be the ſame weight. So I ſuppoſe there was no moiſture left in the earth. Neither do I think, that the pound and half, that was wanting, was drawn away by the cucumber, but a great part of it, in the ordering, was in duſt (and the like) waſted: (the cucumbers are kept by themſelves, leſt you ſhould ſend for them.)* But yet in this trial, *Eleutherius*, it appears, that though ſome of the earth, or rather the diſſoluble ſalt harboured in it, were waſted, the main body of the plant conſiſted of tranſmuted water. And I might add, that a year after I cauſed the formerly mentioned experiment, touching large pompions, to be reiterated, with ſo good ſucceſs, that if my memory does not much miſ-inform me, it did not only much ſurpaſs any, that

I made before, but ſeemed ſtrangely to conclude what I am pleading for; though (by reaſon, I have unhappily loſt the particular account my gardener writ me up of the circumſtances) I dare not inſiſt upon them. The like experiment may be as conveniently tried with the ſeeds of any plant, whoſe growth is haſty, and its ſize bulky. If tobacco will in theſe cold climates grow well in earth undunged, it would not be amiſs to make a trial with it; for it is an annual plant, that ariſes where it proſpers, ſometimes as high as a tall man, and I have had leaves of it in my garden, near a foot and a half broad. But the next time I try this experiment, it ſhall be with ſeveral ſeeds of the ſame ſort, in the ſame pot of earth, that ſo the event may be the more conſpicuous. But, becauſe every body has not conveniency of time and place for this experiment neither, I made, in my chamber, ſome ſhorter and more expeditious trials. I took a top of ſpearment, about an inch long, and put it into a good phial full of ſpring water, ſo as the upper part of the mint was above the neck of the glaſs, and the lower part immerſed in the water; within a few days this mint began to ſhoot forth roots into the water, and to diſplay its leaves, and aſpire upwards; and in a ſhort time it had numerous roots and leaves, and theſe very ſtrong and fragrant of the odour of the mint; but the heat of my chamber, as I ſuppoſe, killed the plant, when it was grown to have a pretty thick ſtalk, which with the various and ramified roots, which it ſhot into the water as if it had been earth, preſented in its tranſparent flower-pot a ſpectacle not unpleaſant to behold. The like I tried with ſweet-marjoram; and I found the experiment ſucceeded alſo, though ſomewhat more ſlowly, with balm and peniroyal, to name now no other plants. And one of theſe vegetables, cheriſhed only by water, having obtained a competent growth, I did, for trial's ſake, cauſe to be diſtilled in a ſmall retort, and thereby obtained ſome phlegm, a little empyreumatical ſpirit, a ſmall quantity of aduſt oil, and a *caput mortuum*; which appearing to be a coal, I concluded it to conſiſt of ſalt and earth: but the quantity of it was ſo ſmall, that I forbore to calcine it. The water I uſed to nouriſh this plant was not ſhifted nor renewed; and I choſe ſpring-water rather than rain-water, becauſe the latter is more diſcernably a kind of ϖανσπερμία, which, though it be granted to be freed from groſſer mixtures, ſeems yet to contain in it, beſides the ſteams of ſeveral bodies wandering in the air, which may be ſuppoſed to impregnate it, a certain ſpirituous ſubſtance, which may be extracted out of it, and is by ſome miſtaken for the ſpirit of the world corporified, upon what grounds, and with what probability, I may elſewhere, perchance, but muſt not now, diſcourſe to you.

BUT perhaps I might have ſaved a great part of my labour. For I find, that *Helmont* (an author more conſiderable for his experiments, than many learned men are pleaſed to think him) having had an opportunity to proſecute an experiment much of the ſame nature with thoſe I have been now ſpeaking of, for five years together, obtained at the end of that time ſo notable a quantity of tranſmuted water, that I ſhould ſcarce think it fit to have his experiment, and mine mentioned together, were it not, that the length of time requiſite to this may deter the curioſity of ſome, and exceed the leiſure of others; and partly, that ſo paradoxical a truth, as that, which theſe experiments ſeem to hold forth, needs to be confirmed by more witneſſes than one, eſpecially ſince the extravagancies and untruths, to be met with in *Helmont*'s treatiſe of the magnetic cure of wounds, have made his teſtimonies ſuſpected in his other writings; though as to ſome of the unlikely matters of fact he delivers in them, I might ſafely undertake to be his compurgator. But that experiment of his, which I was mentioning to you, he ſays, was this: he took 200 pound of earth dried in an oven, and having put it into an earthen veſſel, and moiſtened it with rain-water, he planted in it the trunk of a willow-tree of five pound weight; this he watered, as need required, with rain or diſtilled water; and to keep the neighbouring earth from getting into the veſſel, he employed a plate of iron tinned over and perforated with many holes. Five years being effluxed, he took out the tree, and weighed it, and (with computing the leaves that fell during four autumns) he found it to weigh 169 pound, and about three ounces. And having again dryed the earth it grew in, he found it to want of its former weight of 200 pound, about a couple only of ounces; ſo that 164 pound of the roots, wood, and bark, which conſtituted the tree, ſeem to have ſprung from the water. And though it appears not, that *Helmont* had the curioſity to make any analyſis of this plant, yet what I lately told you I did to one of the vegetables I nouriſhed with water only, will I ſuppoſe keep you from doubting, that if he had diſtilled this tree, it would have afforded him the like diſtinct ſubſtances as another vegetable of the ſame kind. I need not ſubjoin, that I had it alſo in my thoughts to try, how experiments, to the ſame purpoſe with thoſe I related to you, would ſucceed in other bodies than vegetables, becauſe importunate avocations having hitherto hindered me from putting my deſign in practice, I can yet ſpeak but conjecturally of the ſucceſs: but the beſt is, that the experiments already made and mentioned to you need not the aſſiſtance of new ones, to verify as much, as my preſent taſk makes it concern me to prove by experiments of this nature.

ONE would ſuſpect (ſays *Eleutherius* after his long ſilence) by what you have been diſcourſing, that you are not far from *Helmont*'s opinion about the origination of compound bodies, and perhaps too diſlike not the arguments, which he employs to prove it.

WHAT Helmontian opinion, and what arguments do you mean? (aſks *Carneades.*)

WHAT you have been newly diſcourſing (replies *Eleutherius*) tells us, that you cannot but know, that this bold and acute Spagyriſt ſcruples not to aſſert, that all mixt bodies ſpring from one element; and that vegetables, animals, marchaſites, ſtones, metals, *&c.* are materially but ſimple water diſguiſed into theſe various forms, by the plaſtick or formative vertue of their ſeeds. And as for his reaſons, you may find divers of them ſcattered up and down his writings; the conſiderableſt of which ſeem to be theſe three; the ultimate reduction of mixt bodies into inſipid water, the viciſſitude of the ſuppoſed elements, and the production of perfectly mixt bodies out of ſimple water. And firſt he affirms, that the *ſal circulatus Paracelſi*, or his liquor alkaheſt, does adequately reſolve plants, animals, and minerals into one liquor or more, according to their ſeveral internal diſparities of parts, (without *caput mortuum*, or the deſtruction of their ſeminal vertues;) and that the alkaheſt being abſtracted from theſe liquors in the ſame weight and virtue wherewith it diſſolved them, the liquors may by frequent cohobations from chalk or ſome other idoneous matter, be totally deprived of their ſeminal endowments, and return at laſt to their firſt matter, inſipid water: ſome other ways he propoſes here and there to diveſt ſome particular bodies of their borrowed ſhapes, and make them remigrate to their firſt ſimplicity. The ſecond topick, whence *Helmont* draws his arguments, to prove water to be the material cauſe of mixt bodies, I told you was this, that the other ſuppoſed elements may be tranſmuted into one another. But the experiments by him here and there produced on this occaſion are ſo uneaſy to be made and to be judged of, that I ſhall not inſiſt on them; not to mention, that if they were granted to be true, his inference from them is ſomewhat diſputable. And therefore I ſhall paſs on to tell you, that as, in his firſt argument, our paradoxical author endeavours to prove water the ſole element of mixt bodies, by their ultimate reſolution, when by his alkaheſt, or ſome other conquering agent, the ſeeds have been deſtroyed, which diſguiſed them; or when by time thoſe ſeeds are wearied, or exantlated, or unable to act their parts upon the ſtage of the univerſe any longer; ſo in his third argument he endeavours to evince the ſame concluſion, by the conſtitution of bodies, which he aſſerts to be nothing but water ſubdued by ſeveral virtues. Of this he gives here and there in his writings ſeveral inſtances, as to plants and animals; but divers of them being difficult either to be tried or to be underſtood, and others of them being not altogether unobnoxious to exceptions, I think you have ſingled out the principal and leſs queſtionable experiment, when you lately mentioned that of the willow-tree. And having thus, continues *Eleutherius*, to anſwer your queſtion, given you a ſummary account of what, I am confident, you know better than I do, I ſhall be very glad to receive your ſenſe of it, if the giving it me will not too much divert you from the proſecution of your diſcourſe.

THAT *if* (replies *Carneades*) was not needleſsly annexed: for thoroughly to examine ſuch an hypotheſis and ſuch arguments, would require ſo many conſiderations, and conſequently ſo much time, that I ſhould not now have the leiſure to perfect ſuch a digreſſion, and much leſs to finiſh my principal diſcourſe. Yet thus much I ſhall tell you at preſent, that you need not fear my rejecting this opinion for its novelty; ſince, however the Helmontians may, in compliment to their maſter, pretend it to be a new diſcovery, yet though the arguments be for the moſt part his, the opinion itſelf is very antient: for *Diogenes Laertius* and divers other authors ſpeak of *Thales*, as the firſt among the *Grecians*, that made diſquiſitions upon nature. And of this *Thales*, I remember, *Tully* informs us, that he taught all things were at firſt made of water. And it ſeems by *Plutarch* and *Juſtin Martyr*, that the opinion was ancienter than he: for they tell us, that he uſed to defend his tenet by the teſtimony of *Homer*. And a Greek author, the ſcholiaſt of *Apollonius*, upon theſe words,

De naturâ Deorum.

'Εξ ἰλύος ἐβλάστησε χθὼν αὔτη.
The earth of ſlime was made, Argonaut. 4.

affirms (out of *Zeno*) that the chaos, whereof all things were made, was, according to *Heſiod*, water; which, ſettling firſt, became ſlime, and then condenſed into ſolid earth. And the ſame opinion about the generation of ſlime ſeems to have been entertained by *Orpheus*, out of whom one of the ancients cites this teſtimony,

Athenagoras.

'Εκ τοῦ ὕδατος ἰλὺς κατέστη.
Of water ſlime was made.

It ſeems alſo by what is delivered in * *Strabo* out of another author concerning the Indians, that they likewiſe held, that all things had differing beginnings, but that, of which the world was made, was water. And the like opinion has been by ſome of the ancients aſcribed to the Phenicians, from whom *Thales* himſelf is conceived to have borrowed it; as probably the Greeks did much of theology, and, as I am apt to think, of their philoſophy too; ſince the deviſing of the atomical hypotheſis commonly aſcribed to *Leucippus* and his diſciple *Democritus* is by learned men attributed to one *Moſchus* a Phenician. And poſſibly the opinion is yet ancienter than ſo; for it is known, that the Phenicians borrowed moſt of their learning from the Hebrews. And among thoſe, that acknowledge the books of *Moſes*, many have been inclined to think water to have been the primitive and univerſal matter, by peruſing the beginning of *Geneſis*, where the waters ſeem to be mentioned as the material cauſe, not only of ſublunary compound bodies, but of all thoſe, that make up the univerſe; whoſe component parts did orderly, as it were, emerge out of that vaſt abyſs, by the operation of the ſpirit of God, who is ſaid to have been moving himſelf, as hatching females do, (as the original מרחפת, *Merahephet*, is ſaid to import, and it ſeems to ſignify, in one of the two other places, wherein alone I have met with it in the Hebrew bible) upon the face of the waters; which being, as may be ſuppoſed, divinely impregnated with the ſeeds of all things, were by that productive incubation qualified to produce them. But you, I preſume, expect, that I ſhould diſcourſe of this matter like a naturaliſt, not a philologer. Wherefore I ſhall add, to countenance *Helmont*'s opinion, that whereas he gives not, that I remember, any inſtance of any mineral body, nor ſcarce of any animal, generated of water, a French chymiſt, Monſieur *de Rochas*, has preſented his readers an experiment, which if it were punctually ſuch as he has delivered it, is very notable. He then diſcourſing of the generation of things according to certain chymical and metaphorical notions (which I confeſs are not to me intelligible) ſets down, among divers ſpeculations not pertinent to our ſubject, the following narrative, which I ſhall repeat to you the ſenſe of in Engliſh, with as little variation from the literal ſenſe of the French words, as my memory will enable me. *Having* (ſays he) *diſcerned ſuch great wonders by the natural operation of water, I would know what may be done with it by art imitating nature. Wherefore I took water, which I well knew not to be compounded, nor to be mixed with any other thing than that ſpirit of life* (whereof he had ſpoken before) *and with a heat artificial, continual and proportionate, I prepared and diſpoſed it by the above mentioned graduations of coagulation, congelation, and fixation, until it was turned into earth, which earth produced animals, vegetables and minerals. I tell not what animals, vegetables and minerals, for that is reſerved for another occaſion: but the animals did move of themſelves, eat,* &c.—*and by the true anatomy I made of them, I found, that they were compoſed of much ſulphur, little mercury, and leſs ſalt.—The minerals began to grow and increaſe by converting into their own nature one part of the earth thereunto diſpoſed; they were ſolid and heavy. And by this truly demonſtrative ſcience, namely chymiſtry, I found, that they were compoſed of much ſalt, little ſulphur, and leſs mercury.*

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* *Univerſarum rerum primordia diverſa eſſe, faciendi autem mundi initium aquam.* Strabo Geograph. Lib. 15. *circa medium.*

Deuter. xxxii. 11. Jerm. xxiii. 9.

BUT (ſays *Carneades)* I have ſome ſuſpicions concerning this ſtrange relation, which make me unwilling to declare an opinion of it, unleſs I were ſatisfied concerning divers material circumſtances, that our author has left unmentioned; though, as for the generation of living creatures, both vegetable and ſenſitive, it needs not ſeem incredible, ſince we find, that our common water (which indeed is often impregnated with variety of ſeminal principles and rudiments) being long kept in a quiet place, will putrify and ſtink, and then perhaps too, produce moſs and little worms, or other inſects, according to the nature of the ſeeds that were lurking in it. I muſt likewiſe deſire you to take notice, that as *Helmont* gives us no inſtance of the production of minerals out of water, ſo the main argument, that he employs to prove, that they and other bodies may be reſolved into water, is drawn from the operations of his alkaheſt, and conſequently cannot be ſatisfactorily examined by you and me.

YET certainly (ſays *Eleutherius)* you cannot but have ſomewhat wondered as well as I, to obſerve, how great a ſhare of water goes to the making up of divers bodies,
whoſe

whoſe diſguiſes promiſe nothing near ſo much. The diſtillation of eels, though it yielded me ſome oil, and ſpirit, and volatile ſalt, beſides the *caput mortuum*, yet were all theſe ſo diſproportionate to the phlegm, that came from them, (and in which at firſt they boiled as in a pot of water) that they ſeemed to have been nothing but coagulated phlegm; which does likewiſe ſtrangely abound in vipers, though they are eſteemed very hot in operation, and will in a convenient air ſurvive ſome days the loſs of their heads and hearts, ſo vigorous is their vivacity. Man's blood itſelf, as ſpirituous and as elaborate a liquor as it is reputed, does ſo abound in phlegm, that, the other day, diſtilling ſome of it on purpoſe to try the experiment (as I had formerly done in deer's blood) out of about ſeven ounces and a half of pure blood we drew near ſix ounces of phlegm, before any of the more operative principles began to ariſe and invite us to change the receiver. And to ſatisfy myſelf, that ſome of theſe animal phlegms were void enough of ſpirit to deſerve that name, I would not content myſelf to taſte them only, but fruitleſsly poured on them acid liquors, to try if they contained any volatile ſalt or ſpirit, which (had there been any there) would probably have diſcovered itſelf by making an ebullition with the affuſed liquor. And now I mention corroſive ſpirits, I am minded to inform you, that though they ſeem to be nothing elſe but fluid ſalts, yet they abound in water, as you may obſerve, if either you entangle, and ſo fix their ſaline part, by making them corrode ſome idoneous body, or elſe if you mortify it with a contrary ſalt; as I have very manifeſtly obſerved in the making a medicine ſomewhat like *Helmont*'s Balſamus Samech, with diſtilled vinegar inſtead of ſpirit of wine, wherewith he prepares it. For you would ſcarce believe (what I have lately obſerved) that of that acid ſpirit, the ſalt of tartar, from which it is diſtilled, will, by mortifying and retaining the acid ſalt, turn into worthleſs phlegm near twenty times its weight; before it be ſo fully impregnated as to rob no more diſtilled vinegar of its ſalt. And though ſpirit of wine exquiſitely rectified, ſeem of all liquors to be the moſt free from water, it being ſo igneous, that it will flame all away without leaving the leaſt drop behind it, yet even this fiery liquor is by *Helmont* not improbably affirmed, in caſe what he relates be true, to be materially water, under a ſulphurous diſguiſe: for according to him, in the making that excellent medicine, *Paracelſus* his Balſamus Samech, (which is nothing but ſal tartari dulcified, by diſtilling from it ſpirit of wine till the ſalt be ſufficiently glutted with its ſulphur, and till it ſuffer the liquor to be drawn off, as ſtrong as it was poured on) when the ſalt of tartar from which it is diſtilled hath retained, or deprived it of the ſulphurous parts of the ſpirit of wine, the reſt, which is incomparably the greater part of the liquor, will remigrate into phlegm. I added that clauſe [*In caſe what he relates be true*] becauſe I have not as yet ſufficiently tried it myſelf. But not only ſomething of experiment keeps me from thinking it, as many chymiſts do, abſurd, (though I have, as well as they, in vain tried it with ordinary ſalt of tartar) but beſides that *Helmont* often relates it, and draws conſequences from it; a perſon noted for his ſoberneſs and ſkill in ſpagyrical preparations, having been aſked by me, whether the experiment might not be made to ſucceed, if the ſalt and ſpirit were prepared, according to a way ſuitable to my principles, he affirmed to me, that he had that way, I propoſed, made *Helmont*'s experiment ſucceed very well, without adding any thing to the ſalt and ſpirit. But our way is neither ſhort nor eaſy.

I HAVE indeed (ſays *Carneades*) ſometimes wondered to ſee, how much phlegm may be obtained from bodies by the fire. But concerning that phlegm I may anon have occaſion to note ſomething, which I therefore ſhall not now anticipate. But to return to the opinion of *Thales*, and of *Helmont*, I conſider, that ſuppoſing the alkaheſt could reduce all bodies into water, yet whether that water, becauſe inſipid, muſt be elementary, may not groundleſsly be doubted: for I remember the candid and eloquent *Petrus Lauremberglus*, in his notes upon *Sala*'s aphoriſms, affirms, that he ſaw an inſipid menſtruum, that was a powerful diſſolvent, and (if my memory does not much miſ-inform me) could diſſolve gold. And the water, which may be drawn from quickſilver without addition, though it be almoſt taſteleſs, you will, I believe, think of a differing nature from ſimple water, eſpecially if you digeſt in it appropriated minerals. To which I ſhall add but this, that this conſideration may be further extended. For I ſee no neceſſity to conceive, that the water mentioned in the beginning of *Geneſis*, as the univerſal matter, was ſimple and elementary water; ſince though we ſhould ſuppoſe it to have been an agitated congeries or heap conſiſting of a great variety of ſeminal principles and rudiments, and of other corpuſcles fit to be ſubdued and faſhioned by them, it might yet be a body fluid like water, in caſe the corpuſcles, it was made up of, were by their Creator made ſmall enough, and put into ſuch an actual motion, as might make them glide along one another. And as we now ſay, the ſea conſiſts of water (notwithſtanding the ſaline, terreſtrial, and other bodies mingled with it) ſuch a liquor may well enough be called water, becauſe that was the greateſt of the known bodies, whereunto it was like; though, that a body may be fluid enough to appear a liquor, and yet contain corpuſcles of a very differing nature, you will eaſily believe, if you but expoſe a good quantity of vitriol in a ſtrong veſſel to a competent fire. For although it contains both aqueous, earthy, ſaline, ſulphureous, and metalline corpuſcles, yet the whole maſs will at firſt be fluid like water, and boil like a ſeething-pot.

I MIGHT eaſily (continues *Carneades*) enlarge myſelf on ſuch conſiderations, if I were now obliged to give you my judgment of the Thaleſian and Helmontian hypotheſis. But, whether or no we conclude, that all things were at firſt generated of water, I may deduce from what I have tried concerning the growth of vegetables, nouriſhed with water, all that I now propoſed to myſelf or need at preſent to prove; namely, that ſalt, ſpirit, earth, and even oil (though that be thought of all bodies the moſt oppoſite to water) may be produced out of water; and conſequently, that a Chymical principle, as well as a Peripatetic element, may (in ſome caſes) be generated anew, or obtained from ſuch a parcel of matter, as was not endowed with the form of ſuch a principle or element before.

AND having thus, *Eleutherius*, evinced, that it is poſſible, that ſuch ſubſtances as thoſe, that chymiſts are wont to call their *tria prima*, may be generated anew; I muſt next endeavour to make it probable, that the operation of the fire does actually (ſometimes) not only divide compounded bodies into ſmall parts, but compound thoſe parts after a new manner: whence conſequently, for aught we know, there may emerge as well ſaline and ſulphureous ſubſtances, as bodies of other textures. And, perhaps, it will aſſiſt us in our inquiry after the effects of the operations of the fire upon other bodies, to conſider a little, what it does to thoſe mixtures, which being productions of the art of man, we beſt know the compoſition of. You may then be pleaſed to take notice, that though ſoap is made up by the ſoap-boilers of oil or greaſe, and ſalt, and water diligently incorporated together; yet if you expoſe the maſs they conſtitute to a gradual fire in a retort, you ſhall then indeed make a ſeparation, but not of the ſame ſubſtances, that were united into ſoap, but of others of a diſtant and yet not an elementary nature, and eſpecially of an oil very ſharp and fœtid, and of a very differing quality from that, which was imployed to make the ſoap. So, if you mingle in a due proportion ſal armoniac with quick-lime, and diſtil them by degrees of fire, you ſhall not divide the ſal armoniac from the quick-lime, though the one be a volatile, and the other a fixed ſubſtance, but that, which will aſcend, will be a ſpirit much more fugitive, penetrant, and ſtinking, than ſal armoniac; and there will remain with the quick-lime all, or very near all the ſea-ſalt, that concurred to make up the ſal armoniac; concerning which ſea-ſalt I ſhall, to ſatisfy you how well it was united to the lime, inform you, that I have, by making the fire at length very vehement, cauſed both the ingredients to melt in the retort itſelf into one maſs, and ſuch maſſes are apt to relent in the moiſt air. If it be here objected, that theſe inſtances are taken from factitious concretes, which are more compounded than thoſe, which nature produces; I ſhall reply, that beſides that I have mentioned them as much to illuſtrate what I propoſed, as to prove it; it will be difficult to evince, that nature herſelf does not make decompounded bodies; I mean, mingle together ſuch mixt bodies, as are already compounded of elementary, or rather of more ſimple ones. For vitriol (for inſtance) though I have ſometimes taken it out of mineral earths, where nature had without any aſſiſtance of art prepared it to my hand, is really, though chymiſts are pleaſed to reckon it among ſalts, a decompounded body, conſiſting (as I ſhall have occaſion to declare anon) of a terreſtrial ſubſtance, of a metal, and alſo of at leaſt one ſaline body, of a peculiar, and not elementary nature. And we ſee alſo in animals, that their blood may be compoſed of divers very differing mixt bodies, ſince we find it obſerved, that divers ſea-fowl taſte rank of the fiſh, on which they ordinarily feed; and *Hippocrates* himſelf obſerves, that a child may be purged by the milk of the nurſe, if ſhe have taken elaterium; which argues, that the purging corpuſcles of the medicament concur to make up the milk of the nurſe; and that white liquor is generally by phyſicians ſuppoſed to be but blanched and altered blood. And I remember I have obſerved, not far from the *Alps*, that at a certain time of the year the butter of that country was very offenſive to ſtrangers, by reaſon of the rank taſte of a certain herb, whereon the cows were then wont plentifully to feed. But (proceeds *Carneades*) to give you inſtances of another kind, to ſhew that things may be obtained by the fire from a mixt body, that were not pre-exiſtent in it, let me remind you, that from many vegetables there may without any addition be obtained glaſs; a body, which I preſume you will not ſay was pre-exiſtent in it, but produced by the fire. To which I ſhall add but this one example more, namely, that by a certain artificial way of handling quickſilver, you may without addition ſeparate from it at leaſt a fifth, or fourth part of clear liquor; which, with an ordinary Peripatetic, would paſs for water, and which a vulgar chymiſt would not ſcruple to call phlegm, and which, for aught I have yet ſeen or heard, is not reducible into mercury again, and conſequently is more than a diſguiſe of it. Now, beſides that divers chymiſts will not allow mercury to have any, or at leaſt any conſiderable quantity of either of the ignoble ingredients, earth and water; beſides this, I ſay, the great ponderouſneſs of quickſilver makes it very unlikely, that it can have ſo much water in it, as may be thus obtained from it, ſince mercury weighs twelve or fourteen times as much as water of the ſame bulk. Nay, for a further confirmation of this argument, I will add this ſtrange relation, that two friends of mine, the one a phyſician, and the other a mathematician, and both of them perſons of unſuſpected credit, have ſolemnly aſſured me, that after many trials, they made to reduce mercury into water, in order to a philoſophical work, upon gold (which yet, by the way, I now proved unſucceſsful) they did once, by divers cohobations reduce a pound of quickſilver into almoſt a pound of water, and this, without the addition of any other ſubſtance, but only by preſſing the mercury by a ſkilfully managed fire, in purpoſely contrived veſſels. But of theſe experiments, our friend, (ſays *Carneades*, pointing at the Regiſter of this dialogue) will, perhaps, give you a more particular account, than it is neceſſary for me to do; ſince what I have now ſaid, may ſufficiently evince, that the fire may ſometimes as well alter bodies as divide them, and by it we may obtain from a mixed body, what was not pre-exiſtent in it. And how are we ſure, that in no other body, what we call phlegm is barely ſeparated, not produced by the action of the fire? ſince ſo many other mixed bodies are of a much leſs conſtant, and more alterable nature, than mercury (by many tricks it is wont to put upon chymiſts, and, by the experiments I told you of, about an hour ſince) appears to be. But, becauſe I ſhall ere long have occaſion to reſume into conſideration the power of the fire to produce new concretes, I ſhall no longer inſiſt on this argument at preſent: only I muſt mind you, that if you will not diſbelieve *Helmont*'s relations, you muſt confeſs, that the *tria prima* are neither ingenerable, nor incorruptible ſubſtances; ſince by his alkaheſt ſome of them may be produced of bodies, that were before of another denomination; and by the ſame powerful menſtruum all of them may be reduced into inſipid water.

Here *Carneades* was about to paſs on to his third conſideration, when *Eleutherius* being deſirous to hear what he could ſay, to clear his ſecond general conſideration from being repugnant to what he ſeemed to think the true theory of miſtion, prevented him, by telling him, I ſomewhat wonder, *Carneades*, that you, who are in ſo many points unſatisfied with the Peripatetic opinion touching the elements and mixed bodies, ſhould alſo ſeem averſe to that notion touching the manner of miſtion, wherein the chymiſts (though perhaps without knowing, that they do ſo) agree with moſt of the antient philoſophers, that preceded *Ariſtotle*; and that for reaſons ſo conſiderable, that divers modern naturaliſts and phyſicians, in other things unfavourable enough to the Spagyriſts, do in this caſe ſide with them againſt the common opinion of the ſchools. If you ſhould aſk me (continues *Eleutherius*) what reaſons I mean? I ſhould partly by the writings of *Sennertus* and other learned men, and partly by my own thoughts, be ſupplied with more, than it were at preſent proper for me to inſiſt largely on. And therefore, I ſhall mention only, and that briefly, three or four. Of theſe, I ſhall take the firſt, from the ſtate of the controverſy itſelf, and the genuine notion of miſtion, which, though much intricated by the ſchoolmen, I take in ſhort to be this: *Ariſtotle*, at leaſt, as many of his interpreters expound him, and as indeed he teaches in ſome places, where he profeſſedly diſſents from the antients, declares miſtion to be ſuch a mutual penetration, and perfect union of the mingled elements, that there is no portion of the mixed body, how minute ſoever, which does not contain all, and every of the four elements, or in which, if you pleaſe, all the elements are not. And I remember, that he reprehends the miſtion taught by the antients, as too ſlight or groſs, for this reaſon, that bodies mixed according to their hypotheſis, though they appear to human eyes, would not appear ſuch to the acute eyes of a lynx, whoſe perfecter ſight would diſcern the elements, if they were no otherwiſe mingled, than as his predeceſſors would have it, to be but blended, not united; whereas the antients, though they did not all agree about what kind of bodies were mixed, yet they did almoſt unanimouſly hold, that in a compounded body, though the *Miſcibilia*, whether elements, principles, or whatever they pleaſed to call them, were aſſociated in ſuch ſmall parts, and with ſo much exactneſs, that there was no ſenſible part of the maſs, but ſeemed to be of the ſame nature with the reſt, and with the whole; yet as to the atoms, or other inſenſible parcels of matter, whereof each of the *Miſcibilia* conſiſted, they retained each of them its own nature, being but by appoſition or juxta-poſition united with the reſt into one body. So that although by virtue of this compoſition the mixed body did, perhaps, obtain divers new qualities; yet ſtill the ingredients, that compounded it, retaining their own nature, were by the deſtruction of the compoſitum ſeparable from each other, the minute parts diſengaged from thoſe of a differing nature, and aſſociated with thoſe of their own ſort, returning to be again, fire, earth, or water, as they were before they chanced to be ingredients of that compoſitum. This may be explained (continues *Eleutherius*) by a piece of cloth made of white and black threads interwoven, wherein though the whole piece appear neither white nor black, but of a reſulting colour, that is grey, yet each of the white and black threads, that compoſe it, remains what it was before, as would appear, if the threads were pulled aſunder, and ſorted each colour by itſelf. This (purſues *Eleutherius)* being, as I underſtand it, the ſtate of the controverſy, and the Ariſtotelians, after their maſter, commonly defining, that miſtion is *miſcibilium alteratorum unio*, that ſeems to comport much better with the opinion of the Chymiſts, than with that of their adverſaries; ſince, according to that, as the newly-mentioned example declares, there is but a juxta-poſition of ſeparable corpuſcles, retaining each its own nature; whereas, according to the Ariſtotelians, when what they are pleaſed to call a mixed body, reſults from the concourſe of the elements, the *Miſcibilia* cannot ſo properly be ſaid to be altered, as deſtroyed, ſince there is no part in the mixed body, how ſmall ſoever, that can be called either fire, or air, or water, or earth.

NOR indeed, can I well underſtand, how bodies can be mingled other ways, than as I have declared, or, at leaſt, how they can be mingled, as our Peripatetics would have it. For whereas *Ariſtotle* tells us, that, if a drop of wine be put into ten thouſand meaſures of water, the wine being overpowered by ſo vaſt a quantity of water, will be turned into it, he ſpeaks, to my apprehenſion, very improbably. For, though one ſhould add to that quantity of water as many drops of wine, as would a thouſand times exceed it all, yet, by his rule, the whole liquor ſhould not be a *crama*, a mixture of wine and water, wherein the wine would be predominant, but water only; ſince the wine being added but by a drop at a time, would ſtill fall into nothing but water, and conſequently would be turned into it. And, if this would hold in metals too, it were a rare ſecret for goldſmiths, and refiners; for by melting a maſs of gold, or ſilver, and by but caſting into it lead or antimony, grain after grain, they might at pleaſure, within a reaſonable compaſs of time, turn what quantity they deſire, of the ignoble into the noble metals. And, indeed, ſince a pint of wine, and a pint of water, amount to about a quart of liquor, it ſeems manifeſt to ſenſe, that theſe bodies do not totally penetrate one another, as one would have it; but, that each retains its own dimenſions; and conſequently, that they are, by being mingled, only divided into minute bodies, that do but touch one another with their ſurfaces, as do the grains of wheat, rye, barley, *&c.* in a heap of ſeveral ſorts of corn. And unleſs we ſay, that as when one meaſure of wheat, for inſtance, is blended with a hundred meaſures of barley, there happens only a juxta-poſition and ſuperficial contact betwixt the grains of wheat, and as many or thereabouts of the grains of barley; ſo, when a drop of wine is mingled with a great deal of water, there is but an appoſition of ſo many vinous corpuſcles to a correſpondent number of aqueous ones: unleſs I ſay, this be ſaid, I ſee not how that abſurdity will be avoided, whereunto the Stoical notion of miſtion (namely by γυſχυσις, or confuſion) was liable, according to which the leaſt body may be co-extended with the greateſt: ſince in a mixt body, wherein, before the elements were mingled, there was, for inſtance, but one pound of water to ten thouſand of earth, yet according to them there muſt not be the leaſt part of that compound, that conſiſted not as well of earth, as water. But I inſiſt, perhaps, too long (ſays *Eleutherius)* upon the proofs afforded me by the nature of miſtion: wherefore I will but name two or three other arguments; whereof the firſt ſhall be, that, according to *Ariſtotle* himſelf, the motion of a mixt body follows the nature of the predominant element, as thoſe, wherein the earth prevails, tend towards the centre of heavy bodies. And ſince many things make it evident, that in divers mixt bodies the elementary qualities are as well active, though not altogether ſo much ſo, as in the elements themſelves; it ſeems not reaſonable to deny the actual exiſtence of the elements in thoſe bodies, wherein they operate.

To which I ſhall add this convincing argument, that experience manifeſts, and *Ariſtotle* confeſſes it, that the *Miſcibilia* may be again ſeparated from a mixt body, as is obvious in the chymical reſolutions of plants and animals, which could not be, unleſs they did actually retain their forms in it. For ſince, according to *Ariſtotle*, and I think according to truth, there is but one common maſs of all things, which he has been pleaſed to call *materia prima*; and ſince it is not therefore the matter, but the form, that conſtitutes and diſcriminates things, to ſay, that the elements remain not in a mixt body, according to their forms, but according to their matter, is not to ſay, that they remain there at all; ſince although thoſe portions of matter were earth and water, *&c.* before they concurred, yet the reſulting body being once conſtituted, may as well be ſaid to be ſimple as any of the elements; the matter being confeſſedly of the ſame nature in all bodies, and the elementary forms being, according to this hypotheſis, periſhed and aboliſhed.

AND laſtly, and if we will conſult chymical experiments, we ſhall find the advantages of the chymical doctrine above the Peripatetic little leſs than palpable. For in that operation, that refiners call quartation, which they employ to purify gold, although three parts of ſilver be ſo exquiſitely mingled by fuſion with a fourth part of gold (whence the operation is denominated) that the reſulting maſs acquires ſeveral new qualities, by virtue of the compoſition, and that there is ſcarce any ſenſible part of it, that is not compoſed of both the metals; yet if you caſt this mixture into aqua fortis, the ſilver will be diſſolved in the menſtruum, and the gold like a dark or black powder will fall to the bottom of it, and either body may be again reduced into ſuch a metal as it was before; which ſhews, that it retained its nature, notwithſtanding its being mixt *per minima* with the other. We likewiſe ſee, that though one part of pure ſilver be mingled with eight or ten parts, or more, of lead; yet the fire will upon the cupel eaſily and perfectly ſeparate them again. And that, which I would have you peculiarly conſider on this occaſion, is, that not only in chymical anatomies there is a ſeparation made of the elementary ingredients, but that ſome mixt bodies afford a very much greater quantity of this or that element or principle, than of another; as we ſee, that turpentine and amber yield much more oil and ſulphur, than they do water; whereas wine, which is confeſſed to be a perfectly mixt body, yields but a little inflammable ſpirit, or ſulphur, and not much more earth; but affords a vaſt proportion of phlegm or water. Which could not be, if, as the Peripatetics ſuppoſe, every, even of the minuteſt particles, were of the ſame nature with the whole, and conſequently did contain both earth and water, and air, and fire. Wherefore as to what *Ariſtotle* principally, and almoſt only objects, that unleſs his opinion be admitted, there would be no true and perfect miſtion, but only aggregates or heaps of contiguous corpuſcles, which, though the eye of a man cannot diſcern, yet the eye of a lynx might perceive not to be of the ſame nature with one another and with their totum, as the nature of miſtion requires, if he do not beg the queſtion, and make miſtion to conſiſt in what other naturaliſts deny to be requiſite to it, yet he, at leaſt, objects that as a great inconvenience, which I cannot take for ſuch, till he have brought as conſiderable arguments as I have propoſed to prove the contrary, to evince, that nature makes other miſtions than ſuch as I have allowed, wherein the *Miſcibilia* are reduced into minute parts, and united as far as ſenſe can diſcern: which if you will not grant to be ſufficient for a true miſtion, he muſt have the ſame quarrel with nature herſelf, as with his adverſaries.

WHEREFORE (continues *Eleutherius)* I cannot but ſomewhat marvel, that *Carneades* ſhould oppoſe the doctrine of the chymiſts in a particular, wherein they do as well agree with his old miſtreſs, Nature, as diſſent from his old adverſary, *Ariſtotle.*

I MUST not (replies *Carneades)* engage myſelf at preſent to examine thoroughly the controverſies concerning miſtion: and if there were no third thing, but that I were reduced to embrace abſolutely and unreſervedly either the opinion of *Ariſtotle*, or that of the philoſophers, that went before him, I ſhould look upon the latter, which the chymiſts have adopted, as the more defenſible opinion. But becauſe differing in the opinions about the elements from both parties, I think I can take a middle courſe, and diſcourſe to you of miſtion, after a way, that does neither perfectly agree, nor perfectly diſagree with either: as I will not peremptorily define, whether there be not caſes, wherein ſome phænomena of miſtion ſeem to favour the opinion, that the chymiſts patrons borrowed of the antients, I ſhall only endeavour to ſhew you, that there are ſome caſes, which may keep the doubt, which makes up my ſecond general conſideration, from being unreaſonable.

I SHALL then freely acknowledge to you (ſays *Carneades)* that I am not over-well ſatisfied with the doctrine, that is aſcribed to *Ariſtotle*, concerning miſtion; eſpecially, ſince it teaches, that the four elements may again be ſeparated from the mixt body; whereas, if they continued not in it, it would not be ſo much a ſeparation as a production. And I think the antient philoſophers, that preceded *Ariſtotle*, and chymiſts, who have ſince received the ſame opinion, do ſpeak of this matter more intelligibly, if not more probably, than the Peripatetics: but though they ſpeak congruouſly enough to their believing, that there are a certain number of primogenial bodies, by whoſe concourſe all thoſe we call mixt are generated, and which, in the deſtruction of mixt bodies, do barely part company, and reduce from one another, juſt ſuch as they were when they came together; yet I, who meet with very few opinions, that I can entirely acquieſce in, muſt confeſs to you, that I am inclined to differ, not only from the Ariſtotelians, but from the old philoſophers and the chymiſts, about the nature of miſtion. And if you will give me leave, I ſhall briefly propoſe to you my preſent notion of it, provided you will look upon it, not ſo much as an aſſertion as an hypotheſis; in talking of which, I do not now pretend to propoſe and debate the whole doctrine of miſtion, but to ſhew, that it is not improbable, that ſometimes mingled ſubſtances may be ſo ſtrictly united, that it doth not by the uſual operations of the fire, by which chymiſts are wont to ſuppoſe themſelves to have made the analyſis of mixt bodies, ſufficiently appear, that in ſuch bodies the *Miſcibilia*, that concurred to make them up, do each of them retain its own peculiar nature; and by the Spagyriſts fires may be more eaſily extricated and recovered, than altered; either by a change of texture in the parts of the ſame ingredient, or by an aſſociation with ſome parts of another ingredient, more ſtrict than was that of the parts of this or that *Miſcibile* among themſelves. At theſe words, *Eleutherius* having preſſed him to do what he propoſed, and promiſed to do what he deſired

I CONSIDER then (reſumes *Carneades*) that, not to mention thoſe improper kinds of miſtion, wherein homogeneous bodies are joined, as when water is mingled with water, or two veſſels full of the ſame kind of wine with one another, the miſtion I am now to diſcourſe of ſeems, generally ſpeaking, to be but an union *per minima* of any two or more bodies of differing denominations; as when aſhes and ſand are colliquated into glaſs, or antimony and iron into regulus martis, or wine and water are mingled, and ſugar is diſſolved in the mixture. Now in this general notion of miſtion it does not appear clearly comprehended, that the *Miſcibilia* or ingredients do in their ſmall parts ſo retain their nature, and remain diſtinct in the compound, that they may thence by the fire be again taken aſunder: for though I deny not, that in ſome miſtions of certain permanent bodies this recovery of the ſame ingredients may be made, yet I am not convinced, that it will hold in all, or even in moſt, or that it is neceſſarily deducible from chymical experiments, and the true notion of miſtion. To explain this a little, I aſſume, that bodies may be mingled, and that very durably, that are not elementary, nor have been reſolved into elements or principles, that they may be mingled; as is evident in the regulus of colliquated antimony, and iron newly mentioned; and in gold coin, which laſts ſo many ages; wherein generally the gold is alloyed by the mixture of a quantity, greater or leſſer (in our mints they uſe about a 12th part) of either ſilver, or copper, or both. Next, I conſider, that there being but one univerſal matter of things, as it is known that the Ariſtotelians themſelves acknowledge, who call it *materia prima* (about which nevertheleſs I like not all their opinions) the portions of this matter ſeem to differ from one another but in certain qualities or accidents, fewer or more; upon whoſe account the corporeal ſubſtance they belong to receives its denomination, and is referred to this or that particular ſort of bodies: ſo that if it come to loſe, or be deprived of thoſe qualities, though it ceaſes not to be a body, yet it ceaſes from being that kind of body as a plant, or animal, or red, green, ſweet, ſour, or the like. I conſider, that it very often happens, that the ſmall parts of bodies cohere together but by immediate contact and reſt, and that, however, there are few bodies, whoſe minute parts ſtick ſo cloſe together, to what cauſe ſoever their combination be aſcribed, but that it is poſſible to meet with ſome other body, whoſe ſmall parts may get between them, and ſo disjoin them; or may be fitted to cohere more ſtrongly with ſome of them, than thoſe ſome do with the reſt; or at leaſt may be combined ſo cloſely with them, as that neither the fire, nor the other uſual inſtruments of chymical anatomies will ſeparate them. Theſe things being premiſed, I will not peremptorily deny, but that there may be ſome cluſters of particles, wherein the particles are ſo minute, and the coherence ſo ſtrict, or both, that when bodies of differing denominations, and conſiſting of ſuch durable cluſters, happen to be mingled, though the compound body made up of them may be very differing from either of the ingredients, yet each of the little maſſes or cluſters may ſo retain its own nature, as to be again ſeparable, ſuch as it was before. As when gold and ſilver being melted together in a due proportion (for in every proportion, the refiners will tell you, that the experiment will not ſucceed) aqua fortis will diſſolve the ſilver, and leave the gold untouched; by which means, as you lately noted, both the metals may be recovered from the mixed maſs. But (continues *Carneades*) there are other cluſters, wherein the particles ſtick not ſo cloſe together, but that they may meet with corpuſcles of another denomination, which are diſpoſed to be more cloſely united with ſome of them, than they were among themſelves. And in ſuch caſe, two thus combining corpuſcles loſing that ſhape, or ſize, or motion, or other accident, upon whoſe account they were endowed with ſuch a determinate quality or nature, each of them really ceaſes to be a corpuſcle of the ſame denomination it was before; and from the coalition of theſe there may emerge a new body, as really one, as either of the corpuſcles was before they were mingled, or, if you pleaſe, confounded: ſince this concretion is really endowed with its own diſtinct qualities, and can no more by the fire, or any other known way of analyſis, be divided again into the corpuſcles, that at firſt concurred to make it, than either of them could, by the ſame means, be ſubdivided into other particles. But (ſays *Eleutherius)* to make this more intelligible by particular examples; if you diſſolve copper in aqua fortis, or ſpirit of nitre (for I remember not which I uſed, nor do I think it much material) you may by cryſtallizing the ſolution obtain a goodly vitriol; which, though by virtue of the compoſition it have manifeſtly divers qualities, not to be met with in either of the ingredients, yet it ſeems, that the nitrous ſpirits, or at leaſt many of them, may in this compounded maſs retain their former nature: for having for trial-ſake diſtilled this vitriol ſpirit, there came over ſtore of red fumes, which by that colour, by their peculiar ſtink, and by their ſourneſs, manifeſted themſelves to be nitrous ſpirits, and that the remaining calx continued copper, I ſuppoſe you will eaſily believe. But if you diſſolve minium, which is but lead powdered by the fire, in good ſpirit of vinegar, and cryſtallize the ſolution, you ſhall not only have a ſaccharine ſalt exceedingly differing from both its ingredients; but the union of ſome parts of the menſtruum with ſome of thoſe of the metal is ſo ſtrict, that the ſpirit of vinegar ſeems to be, as ſuch, deſtroyed; ſince the ſaline corpuſcles have quite loſt that acidity, upon whoſe account the liquor was called ſpirit of vinegar; nor can any ſuch acid parts, as were put to the minium, be ſeparated by any known way from the *ſaccharum Saturni* reſulting from them both. For not only there is no ſourneſs at all, but an admirable ſweetneſs to be taſted in the concretion; and not only I found not that ſpirit of wine, which otherwiſe will immediately hiſs, when mingled with ſtrong ſpirit of vinegar, would hiſs being poured upon *ſaccharum Saturni*, wherein yet the acid ſalt of vinegar, did it ſurvive, may ſeem to be concentrated; but upon the diſtillation of *ſaccharum Saturni* by itſelf, I found indeed a liquor very penetrant, but not at all acid, and differing as well in ſmell and other qualities, as in taſte, from the ſpirit of vinegar; which likewiſe ſeemed to have left ſome of its parts very firmly united to the *caput mortuum*, which though of a leaden nature was in ſmell, colour, *&c.* differing from minium. Which brings into my mind, that though two powders, the one blue, and the other yellow, may appear a green mixture, without either of them loſing its own colour, as a good microſcope has ſometimes informed me; yet having mingled minium and ſal armoniac in a requiſite proportion, and expoſed them in a glaſs veſſel to the fire, the whole maſs became white, and the red corpuſcles were deſtroyed: for though the calcined lead was ſeparable from the ſalt, yet you will eaſily believe it did not part from it in the form of a red powder, ſuch as was the minium, when it was put to the ſal armoniac. I leave it alſo to be conſidered, whether in blood, and divers other bodies, it be probable, that each of the corpuſcles, that concur to make a compound body, doth, though ſome of them in ſome caſes may, retain its own nature in it, ſo that chymiſts may extricate each ſort of them from all the others, wherewith it concurred to make a body of one denomination.

I KNOW there may be a diſtinction betwixt matter immanent, when the material parts remain, and retain their own nature in the things materiated, as ſome of the ſchoolmen ſpeak (in which ſenſe, wood, ſtones and lime are the matter of a houſe;) and tranſient, which in the materiated thing is ſo altered, as to receive a new form,

without

without being capable of re-admitting again the old. In which ſenſe the friends of this diſtinction ſay, that chyle is the matter of blood, and blood that of a human body, of all whoſe parts it is preſumed to be the aliment. I know alſo, that it may be ſaid, that of material principles, ſome are common to all mixt bodies, as *Ariſtotle's* four elements, or the chymiſts *tria prima*; others peculiar, which belong to this or that ſort of bodies; as butter and a kind of whey may be ſaid to be the proper principles of cream: and I deny not, but that theſe diſtinctions may in ſome caſes be of uſe; but partly by what I have ſaid already, and partly by what I am to ſay, you may eaſily enough gueſs, in what ſenſe I admit them, and diſcern, that in ſuch a ſenſe they will either illuſtrate ſome of my opinions, or at leaſt will not overthrow any of them.

To proſecute then what I was ſaying before, I will add to this purpoſe, that ſince the major part of chymiſts credit what thoſe they call philoſophers affirm of their ſtone, I may repreſent to them, that though when common gold and lead are mingled together, the lead may be ſevered almoſt unaltered from the gold; yet if inſtead of gold a tantillum of the red elixir be mingled with a ſaturn, their union will be ſo indiſſoluble in the perfect gold, that will be produced by it, that there is no known, nor perhaps no poſſible way of ſeparating the diffuſed elixir from the fixed lead, but they both conſtitute a moſt permanent body, wherein the ſaturn ſeems to have quite loſt its properties, that made it be called lead, and to have been rather tranſmuted by the elixir, than barely aſſociated to it. So that it ſeems not always neceſſary, that the bodies, that are put together *per minima*, ſhould each retain its own nature; ſo as when the maſs itſelf is diſſipated by the fire, to be more diſpoſed to re-appear in its priſtine form, than in any new one, which by a ſtricter aſſociation of its parts with thoſe of ſome of the other ingredients of the compoſitum, than with one another, it may have acquired.

AND if it be objected, that unleſs the hypotheſis I oppoſe be admitted, in ſuch caſes as I have propoſed, there would not be an union, but a deſtruction of mingled bodies, which ſeems all one as to ſay, that of ſuch bodies there is no miſtion at all; I anſwer, that though the ſubſtances, that are mingled, remain, only their accidents are deſtroyed, and though we may with tolerable congruity call them *Miſcibilia*, becauſe they are diſtinct bodies before they are put together, however afterwards they are ſo confounded, that I ſhould rather call them concretions, or reſulting bodies, than mixed ones; and though, perhaps, ſome other and better account may be propoſed, upon which the name of miſtion may remain; yet, if what I have ſaid be thought reaſon, I ſhall not wrangle about words, though I think it fitter to alter a term of art, than reject a new truth, becauſe it ſuits not with it. If it be alſo objected, that this notion of mine, concerning miſtion, though it may be allowed, when bodies already compounded are put to be mingled, yet it is not applicable to thoſe miſtions, that are immediately made of the elements, or principles themſelves; I anſwer in the firſt place, that I here conſider the nature of miſtion ſomewhat more generally, than the chymiſts; who yet cannot deny, that there are oftentimes mixtures, and thoſe very durable ones, made of bodies, that are not elementary. And in the next place, that though it may be probably pretended, that in thoſe mixtures that are made immediately of the bodies, that are called principles or elements, the mingled ingredients may better retain their own nature in the compounded maſs, and be more eaſily ſeparated from thence; yet, beſides that it may be doubted, whether there be any ſuch primary bodies, I ſee not why the reaſon I alleged, of the deſtructibility of the ingredients of bodies in general, may not ſometimes be applicable to ſalt, ſulphur, or mercury; till it be ſhewn upon what account we are to believe them privileged. And however (if you pleaſe but to recall to mind, to what purpoſe I told you at firſt, I meant to ſpeak of miſtion at this time) you will perhaps allow, that what I have hitherto diſcourſed about it, may not only give ſome light to the nature of it in general (eſpecially when I ſhall have an opportunity to declare to you my thoughts on that ſubject more fully) but may on ſome occaſions alſo be ſerviceable to me in the enſuing part of this diſcourſe.

BUT to look back now to that part of our diſcourſe, whence this excurſion concerning miſtion has ſo long diverted us, though we there deduced, from the differing ſubſtances obtained from a plant nouriſhed only with water, and from ſome other things, that it was not neceſſary, that nature ſhould always compound a body at firſt of all ſuch differing bodies, as the fire could afterwards make it afford; yet this is not all, that may be collected from thoſe experiments. For from them there ſeems alſo deducible ſomething, that ſubverts another foundation of the chymical doctrine. For ſince that (as we have ſeen) out of fair water alone, not only ſpirit, but oil, and ſalt, and earth may be produced; it will follow, that ſalt and ſulphur are not primogenial bodies, and principles, ſince they are every day made out of plain water by the texture, which the ſeed or ſeminal principle of plants put it into. And this would not perhaps ſeem ſo ſtrange, if, though pride or negligence, we were not wont to overlook the obvious and familiar workings of nature; for if we conſider what ſlight qualities they are, that ſerve to denominate one of the *tria prima*, we ſhall find, that nature does frequently enough work as great alterations in divers parcels of matter; for to be readily diſſoluble in water, is enough to make the body, that is ſo, paſs for a ſalt. And yet I ſee not, why from a new ſhuffling and diſpoſition of the component particles of a body, it ſhould be much harder for nature to compoſe a body diſſoluble in water, of a portion of water, that was not ſo before, than of the liquid ſubſtance of an egg, which will eaſily mix with water, to produce by the bare warmth of a hatching hen, membranes, feathers, tendons, and other parts, that are not diſſoluble in water as that liquid ſubſtance was: nor is the hardneſs and brittleneſs of ſalt more difficult for nature to introduce into ſuch a yielding body as water, than it is for her to make the bones of a chick out of the tender ſubſtance of the liquors of an egg. But inſtead of proſecuting this conſideration, as I eaſily might, I will proceed as ſoon as I have taken notice of an objection, that lies in my way. For I eaſily foreſee it will be alleged, that the above mentioned examples are all taken from plants, and animals, in whom the matter is faſhioned by the plaſtic power of the ſeed, or ſomething analogous thereunto. Whereas the fire does not act like any of the ſeminal principles, but deſtroys them all, when they come within its reach. But to this I ſhall need at preſent to make but this eaſy anſwer, that whether it be a ſeminal principle, or any other, which faſhions that matter after thoſe various manners I have mentioned to you, yet it is evident, that either by the plaſtic principle alone, or that and heat together, or by ſome other cauſe capable to contex the matter, it is yet poſſible, that the matter may be anew contrived into ſuch bodies. And it is only for the poſſibility of this, that I am now contending.

THE

SCEPTICAL CHYMIST:

PART III.

WHAT I have hitherto diſcourſed, *Eleutherius*, (ſays his friend to him) has, I preſume, ſhewn you, that a conſidering man may very well queſtion the truth of thoſe very ſuppoſitions, which Chymiſts as well as Peripateticks, without proving, take for granted; and upon which depends the validity of the inferences they draw from their experiments. Wherefore having diſpatched that, which though a chymiſt perhaps will not, yet I do, look upon as the moſt important, as well as difficult, part of my taſk, it will now be ſeaſonable for me to proceed to the conſideration of the experiments themſelves, wherein they are wont ſo much to triumph and glory. And theſe will the rather deſerve a ſerious examination, becauſe thoſe, that alledge them, are wont to do it with ſo much confidence and oſtentation, that they have hitherto impoſed upon almoſt all perſons, without excepting philoſophers and phyſicians themſelves, who have read their books, or heard them talk. For ſome learned men have been content rather to believe what they ſo boldly affirm, than be at the trouble and charge, to try whether or no it be true. Others again, who have curioſity enough to examine the truth of what is averred, want ſkill and opportunity to do what they deſire. And the generality even of learned men, ſeeing the chymiſts (not contenting themſelves with the ſchools to amuſe the world with empty words) actually perform divers ſtrange things, and, among thoſe, reſolve compound bodies into ſeveral ſubſtances, not known by former philoſophers to be contained in them; men I ſay, ſeeing theſe things, and hearing with what confidence chymiſts aver the ſubſtances obtained from compound bodies by the fire to be the true elements, or, (as they ſpeak) hypoſtatical principles of them, are forward to think it but juſt, as well as modeſt, that according to the logicians rule, the ſkilful artiſts ſhould be credited in their own art; eſpecially when thoſe things, whoſe nature they ſo confidently take upon them to teach others, are not only productions of their own ſkill, but ſuch as others know not elſe what to make of.

BUT though (continues *Carneades*) the chymiſts have been able, upon ſome or other of the mentioned accounts, not only to delight but amaze, and almoſt to bewitch even learned men; yet ſuch as you and I, who are not unpractiſed in the trade, muſt not ſuffer ourſelves to be impoſed upon by hard names, or bold aſſertions; nor to be dazled by that light which ſhould but aſſiſt us to diſcern things the more clearly. It is one thing to be able to help nature to produce things, and another thing to underſtand well the nature of the things produced. As we ſee, that many perſons that can beget children, are for all that as ignorant of the number and nature of the parts, eſpecially the internal ones, that conſtitute a child's body, as they that never were parents. Nor do I doubt, but you will excuſe me, if, as I thank the chymiſts for the things their analyſis ſhews me, ſo I take the liberty to conſider, how many, and what they are, without being aſtoniſhed at them; as if, whoſoever hath ſkill enough to ſhew men ſome new thing of his own making, had the right to make them believe whatſoever he pleaſes to tell them concerning it.

WHEREFORE, I will now proceed to my third general conſideration; which is, that it does not appear, that three is preciſely and univerſally the number of the diſtinct ſubſtances or elements, whereunto mixt bodies are reſoluble by the fire: I mean, that it is not proved by chymiſts, that all the compound bodies, which are granted to be perfectly mixt, are upon their chymical analyſis diviſible each of them into juſt three diſtinct ſubſtances, neither more nor leſs, which are wont to be looked upon as elementary, or may as well be reputed ſo as thoſe, that are ſo reputed. Which laſt clauſe I ſubjoin, to prevent your objecting, that ſome of the ſubſtances I may have occaſion to mention by and by, are not perfectly homogeneous, nor conſequently worthy of the name of principles. For that which I am now to conſider is, into how many differing ſubſtances, that may plauſibly paſs for the elementary ingredients of a mixt body, it may be analyzed by the fire; but whether each of theſe be uncompounded I reſerve to examine, when I ſhall come to the next general conſideration; where I hope to evince, that the ſubſtances, which the chymiſts not only allow, but aſſert to be the component principles of the body reſolved into them, are not wont to be uncompounded.

Now there are two kind of arguments (purſues *Carneades*) which may be brought to make my third propoſition ſeem probable; one ſort of them being of a more ſpeculative nature, and the other drawn from experience. To begin then with the firſt of theſe.

BUT as *Carneades* was going to do as he had ſaid, *Eleutherius* interrupted him, by ſaying with a ſomewhat ſmiling countenance;

IF you have no mind I ſhould think, that the proverb, *that good wits have bad memories*, is rational and applicable to you, you muſt not forget now you are upon the ſpeculative conſiderations, that may relate to the number of the elements, that yourſelf did not long ſince deliver and concede ſome propoſitions in favour of the chymical doctrine, which I may without diſparagement to you think it uneaſy, even for *Carneades* to anſwer.

I HAVE not, replies he, forgot the conceſſions you mean; but I hope too, that you have not forgot neither with what cautions they were made, when I had not yet aſſumed the perſon I am now ſuſtaining. But however, I ſhall, to content you, ſo diſcourſe of my third general conſideration, as to let you ſee, that I am not unmindful of the things you would have me remember.

To talk then again according to ſuch principles as I then made uſe of, I ſhall repreſent, that if it be granted rational to ſuppoſe, as I then did, that the elements conſiſted at firſt of certain ſmall and primary coalitions of the minute particles of matter into corpuſcles very numerous, and very like each other, it will not be abſurd to conceive, that ſuch primary cluſters may be of far more ſorts than three or five; and conſequently, that we need not ſuppoſe, that in each of the compound bodies we are treating of, there ſhould be found juſt three ſorts of ſuch primitive coalitions, as we are ſpeaking of.

AND if, according to this notion, we allow a conſiderable number of differing elements, I may add, that it ſeems very poſſible, that to the conſtitution of one ſort of mixt bodies two kinds of elementary ones may ſuffice (as I lately exemplified to you, in that moſt durable concrete, glaſs,) another ſort of mixts may be compoſed of three elements, another of four, another of five, and another perhaps of many more. So that, according to this notion, there can be no determinate number aſſigned, as that of the elements of all ſorts of compound bodies whatſoever; it being very probable, that ſome concretes conſiſt of fewer, ſome of more elements. Nay, it does not ſeem impoſſible, according to theſe principles, but that there may be two ſorts of mixts, whereof the one may not have any of all the ſame elements as the other conſiſts of; as we oftentimes ſee two words, whereof one has not any one of the letters to be met with in the other; or as we often meet with divers electuaries, in which no ingredient (except ſugar) is common to any two of them. I will not here debate, whether there may not be a multitude of theſe corpuſcles, which by reaſon of their being primary and ſimple, might be called elementary, if ſeveral ſorts of them ſhould convene to compoſe any body, which are as yet free, and neither as yet contexed and entangled with primary corpuſcles of other kinds, but remain liable to be ſubdued and faſhioned by ſeminal principles, or the like powerful and tranſmuting agent; by whom they may be ſo connected among themſelves, or with the parts of the bodies, as to make the compound bodies, whoſe ingredients they are, reſoluble into more, or other elements than thoſe that chymiſts have hitherto taken notice of.

To all which I may add, that ſince it appears, by what I obſerved to you of the permanency of gold and ſilver, that even corpuſcles, that are not of an elementary, but compounded nature, may be of ſo durable a texture, as to remain indiſſoluble in the ordinary analyſis that Chymiſts make of bodies by the fire; it is not impoſſible, but that, though there were but three elements, yet there may be a greater number of bodies, which the wonted ways of anatomy will not diſcover to be no elementary bodies.

BUT, (ſays *Carneades)* having thus far, in compliance to you, talked conjecturally of the number of the elements, it is now time to conſider, not of how many elements it is poſſible that nature may compound mixed bodies, but (at leaſt, as far as the ordinary experiments of chymiſts will inform us) of how many ſhe doth make them up.

I SAY then, that it does not by theſe ſufficiently appear to me, that there is any one determinate number of elements to be uniformly met with in all the ſeveral ſorts of bodies allowed to be perfectly mixed.

AND for the more diſtinct proof of this propoſition, I ſhall in the firſt place repreſent, that there are divers bodies, which I could never ſee by fire divided into ſo many as three elementary ſubſtances. I would fain (as I ſaid lately to *Philoponus)* ſee that fixed and noble metal we call gold, ſeparated into ſalt, ſulphur and mercury: and if any man will ſubmit to a competent forfeiture in caſe of failing, I ſhall willingly, in caſe of proſperous ſucceſs, pay for both the materials and the charges of ſuch an experiment. It is not, that after what I have tried myſelf I dare peremptorily deny, that there may out of gold be extracted a certain ſubſtance, which I cannot hinder chymiſts from calling its tincture or ſulphur; and which leaves the remaining body deprived of its wonted colour. Nor am I ſure, that there cannot be drawn out of the ſame metal a real quick and running mercury. But for the ſalt of gold, I never could either ſee it, or be ſatisfied that there was ever ſuch a thing ſeparated, *in rerum naturâ*, by the relation of any credible eye-witneſs. And for the ſeveral proceſſes, that promiſe that effect, the materials, that muſt be wrought upon, are ſomewhat too precious and coſtly to be waſted upon ſo groundleſs adventures, of which not only the ſucceſs is doubtful, but the very poſſibility is not yet demonſtrated. Yet that, which moſt deters me from ſuch trials, is not their chargeableneſs, but their unſatisfactorineſs, though they ſhould ſucceed. For the extraction of this golden ſalt being in chymiſts proceſſes preſcribed to be effected by corroſive menſtruums, or the intervention of other ſaline bodies, it will remain doubtful to a wary perſon, whether the emergent ſalt be that of the gold itſelf, or of the ſaline bodies or ſpirits employed to prepare it; for that ſuch diſguiſes of metals do often impoſe upon artiſts, I am ſure *Eleutherius* is not ſo much a ſtranger to chymiſtry as to ignore. I would likewiſe willingly ſee the three principles ſeparated from the pure ſort of virgin-ſand, from oſteocolla, from refined ſilver, from quickſilver freed from its adventitious ſulphur, from Venetian talc, which by long detention in an extreme reverberium, I could but divide into ſmaller particles, not the conſtituent principles; nay, which, when I cauſed it to be kept I know not how long, in a glaſs-houſe fire, came out in the figure its lumps had, when put in, though altered to an almoſt amethyſtine colour; and from divers other bodies, which it were now unneceſſary to enumerate. For though I dare not abſolutely affirm it to be impoſſible to analyze theſe bodies into their *tria prima*; yet becauſe neither of my own experiments, nor any competent teſtimony hath hitherto either taught me, how ſuch an analyſis may be made, or ſatisfied me, that it hath been ſo; I muſt take the liberty to refrain from believing it, till the chymiſts prove it, or give us intelligible and practicable proceſſes to perform what they pretend. For whilſt they affect that enigmatical obſcurity, with which they are wont to puzzle the readers of their divulged proceſſes concerning the analytical preparation of gold or mercury, they leave wary perſons much unſatisfied, whether or no the differing ſubſtances, they promiſe to produce, be truly the hypoſtatical principles, or only ſome intermixtures of the divided bodies with thoſe employed to work upon them, as is evident in the ſeeming cryſtals of ſilver, and thoſe of mercury; which, though by ſome inconſiderately ſuppoſed to be the ſalts of thoſe metals, are plainly but mixtures of the metalline bodies with the ſaline parts of aqua fortis, or other corroſive liquors; as is evident by their being reducible into ſilver or quickſilver, as they were before.

I CANNOT but confeſs (ſaith *Eleutherius*) that though chymiſts may, upon probable grounds, affirm themſelves able to obtain their *tria prima* from animals and vegetables, yet I have often wondered, that they ſhould ſo confidently pretend alſo, to reſolve all metalline and other mineral bodies into ſalt, ſulphur, and mercury. For it is a ſaying almoſt proverbial among thoſe chymiſts themſelves, that are accounted philoſophers, and our famous countryman *Roger Bacon* has particularly adopted it; that *facilius eſt aurum facere, quam deſtruere*. And I fear, with you, that gold is not the only mineral, from which chymiſts are wont fruitleſly to attempt the ſeparating of their three principles. I know indeed (continues *Eleutherius*) that the learned *Sennertus*, even in that book, where he takes not upon him to play the advocate for the chymiſts, but the umpire betwixt them and the Peripateticks, expreſſes himſelf roundly, thus; *Salem omnibus ineſſe (mixtis ſcilicet) & ex iis fieri poſſe omnibus in reſolutionibus chymicis verſatis notiſſimum eſt*. And in the next page, *quod de ſale dixi*, ſays he, *idem de ſulphure dici poteſt*. Sennert. lib. de conſ. & deſſenſ. *pag.* 147. But, by his favour, I muſt ſee very good proofs, before I believe ſuch general aſſertions, how boldly ſoever made; and he, that would convince me of their truth, muſt firſt teach me ſome true and practicable way of ſeparating ſalt and ſulphur from gold, ſilver, and thoſe many different ſorts of ſtones, that a violent fire does not bring to lime, but to fuſion: and not only I, for my own part, never ſaw any of thoſe newly named bodies ſo reſolved; but *Helmont*, who was much better verſed in the chymical anatomizing of bodies then either *Sennertus* or I, has ſomewhere this reſolute paſſage; *Scio* (ſays he) *ex arena, ſilicibus & ſaxis, non* Helmon. *pag.* 409.

Quercet. apud Billich. in Theſſalo. redivivo, *pag.* 99.

calcariis, nunquam ſulphur aut mercurium trahi poſſe: nay, *Quercetanus* himſelf, though the grand ſtickler for the *tria prima*, has this confeſſion of the irreſolubleneſs of diamonds; *Adamas* (ſaith he) *omnium factus lapidum ſolidiſſimus ac duriſſimus ex arctiſſima videlicet trium principiorum unione ac cohærentia, qua nulla arte ſeparationis in ſolutionem principiorum ſuorum ſpiritualium disjungi poteſt.* And indeed, purſues *Eleutherius*, I was not only glad, but ſomewhat ſurprized to find you inclined to admit, that there may be a ſulphur and a running mercury drawn from gold; for unleſs you do (as your expreſſion ſeemed to intimate) take the word ſulphur in a very looſe ſenſe, I muſt doubt, whether our chymiſts can ſeparate a ſulphur from gold: for when I ſaw you make the experiment, that I ſuppoſe invited you to ſpeak as you did, I did not judge the golden tincture to be the true principle of ſulphur extracted from the body, but an aggregate of ſome ſuch highly coloured parts of the gold, as a chymiſt would have called a ſulphur incombuſtible, which in plain Engliſh ſeems to be little better than to call it a ſulphur and no ſulphur. And as for metalline mercuries, I had not wondered at it, though you had expreſſed much more ſeverity in ſpeaking of them; for I remember, that having once met an old and famous artiſt, who had long been (and ſtill is) chymiſt to a great monarch, the repute he had of a very honeſt man invited me to deſire him to tell me ingeniouſly, whether or no, among his many labours, he had ever really extracted a true and running mercury out of metals; to which queſtion he freely replied, that he had never ſeparated a true mercury from any metal; nor had ever ſeen it really done by any man elſe. And though gold is, of all metals, that, whoſe mercury chymiſts have moſt endeavoured to extract, and which they do the moſt brag they have extracted; yet the experienced *Angelus Sala*, in his ſpagyrical account of the ſeven terreſtrial planets (that is, the ſeven metals) affords us this memorable teſtimony, to our preſent purpoſe: *Quanquam* (ſays he) *&c. experientia tamen (quam ſtultorum magiſtram vocamus) certe comprobavit, mercurium auri adeo fixum, maturum, & arctè cum reliquis ejuſdem corporis ſubſtantiis conjungi, ut nullo modo retrogredi poſſit.* To which he ſubjoins, that he himſelf had ſeen much labour ſpent upon that deſign, but could never ſee any ſuch mercury produced thereby. And I eaſily believe what he annexes; *that he had often ſeen detected many tricks and impoſtures of cheating alchymiſts.* For, the moſt part of thoſe, that are fond of ſuch charletans, being unſkilful or credulous, or both, it is very eaſy for ſuch as have ſome ſkill, much craft, more boldneſs, and no conſcience, to impoſe upon them: and therefore, though many profeſſed alchymiſts, and divers perſons of quality have told me, that they have made or ſeen the mercury of gold, or of this or that other metal; yet I have been ſtill apt to fear, that either theſe perſons have had a deſign to deceive others, or have had not ſkill and circumſpection enough to keep themſelves from being deceived.

You recall to my mind (ſays *Carneades*) a certain experiment I once deviſed, innocently to deceive ſome perſons, and let them and others ſee, how little is to be built upon the affirmation of thoſe, that are either unſkilful or unwary, when they tell us they have ſeen alchymiſts make the mercury of this or that metal: and to make this the more evident, I made my experiment much more ſlight, ſhort and ſimple, than the chymiſts uſual proceſſes to extract metalline mercuries; which operations being commonly more elaborate and intricate, and requiring a much more longer time, give the alchymiſts a greater opportunity to cozen, and conſequently are more obnoxious to the ſpectators ſuſpicion. And that, wherein I endeavoured to make my experiment look the more like a true analyſis, was, that I not only pretended, as well as others, to extract a mercury from the metal I wrought upon, but likewiſe to ſeparate a large proportion of manifeſt and inflammable ſulphur. I take then of the filings of copper about a dram or two; of common ſublimate, powdered, the like weight; and ſal armoniac near about as much as of ſublimate: theſe three being well mingled together, I put into a ſmall phial with a long neck, or, which I find better, into a glaſs-urinal, which (having firſt ſtopped it with cotton) to avoid the noxious fumes, I approach by degrees to a competent fire of well kindled coals, or (which looks better, but more endangers the glaſs) to the flame of a candle; and after a while the bottom of the glaſs being held juſt upon the kindled coals, or in the flame, you may in about a quarter of an hour, or, perchance, in half that time, perceive in the bottom of the glaſs ſome running mercury; and if then you take away the glaſs, and break it, you ſhall find a parcel of quickſilver, perhaps altogether, and perhaps part of it in the pores of the ſolid maſs. You ſhall find too, that the remaining lump being held to the flame of the candle, will readily burn with a greeniſh flame, and after a little while (perchance preſently) will in the air acquire a greeniſh blue, which being the colour that is aſcribed to copper, when its body is unlocked, it is eaſy to perſuade men, that this is the true ſulphur of *Venus*, eſpecially ſince not only the ſalts may be ſuppoſed partly to be flown away, and partly to be ſublimed to the upper part of the glaſs, (whoſe inſide will commonly appear whitened by them) but the metal ſeems to be quite deſtroyed, the copper no longer appearing in a metalline form, but almoſt in that of a reſinous lump: whereas, indeed, the caſe is only this, that the ſaline parts of the ſublimate, together with the ſal armoniac, being excited and actuated by the vehement heat, fall upon the copper, (which is a metal they can more eaſily corrode than ſilver) whereby the ſmall parts of the mercury being freed from the ſalts, that kept them aſunder, and being by the heat tumbled up and down after many occurſions, they convene into a conſpicuous maſs of liquor; and as for the ſalts, ſome of the more volatile of them ſubliming to the upper part of the glaſs, the others corrode the copper, and uniting themſelves with it, do ſtrangely alter and diſguiſe its metallick form, and compoſe with it a new kind of concrete, inflammable like ſulphur; concerning which, I ſhall not now ſay any thing, ſince I can refer you to the diligent obſervations, which I remember Mr. *Boyle* has made concerning this odd kind of verdigreaſe. But, continues *Carneades* ſmiling, you know I was not cut out for a mountebank, and therefore I will haſten to reſume the perſon of a ſceptick, and take up my diſcourſe, where you diverted me from proſecuting it.

In the next place then I conſider, that, as there are ſome bodies, which yield not ſo many as the three principles; ſo there are many others, that in their reſolution exhibit more principles than three; and that therefore the ternary number is not that of the univerſal and adequate principles of bodies. If you allow of the diſcourſe I lately made you, touching the primary aſſociations of the ſmall particles of matter, you will ſcarce think it improbable, that of ſuch elementary corpuſcles there may be more ſorts than either three, or four, or five. And if you will grant, what will ſcarce be denied, that corpuſcles of a compounded nature may, in all the wonted examples of chymiſts, paſs for elementary, I ſee not why you ſhould think it impoſſible, that aqua fortis, or aqua regis, will make a ſeparation of colliquated ſilver and gold, though the fire cannot: ſo there may be ſome agent found out ſo ſubtile and ſo powerful, at leaſt in reſpect of thoſe particular compounded corpuſcles, as to be able to reſolve them into thoſe more ſimple ones, whereof they conſiſt, and conſequently increaſe the number of the diſtinct ſubſtances, whereunto the mixed body has been hitherto reſoluble. And if that be true, which I recited to you a while ago out of *Helmont*, concerning the operations of the alkaheſt, which divides bodies into other diſtinct ſubſtances, both as to number and nature, than the fire does; it will not a little countenance my conjecture. But confining ourſelves to ſuch ways of analyzing mixed bodies, as are already not unknown to chymiſts, it may without abſurdity be queſtioned, whether beſides thoſe groſſer elements of bodies, which they call ſalt, ſulphur and mercury, there may not be ingredients of a more ſubtile nature, which being extremely little, and not being in themſelves viſible, may eſcape unheeded at the junctures of the diſtillatory veſſels, though never ſo carefully luted. For let me obſerve to you one thing, which, though not taken notice of by chymiſts, may be a notion of good uſe in divers caſes to a naturaliſt, that we may well ſuſpect, that there may be ſeveral ſorts of bodies, which are not immediate objects of any one of our ſenſes; ſince we ſee, that not only thoſe little corpuſcles, that iſſue out of the loadſtone, and perform the wonders, for which it is juſtly admired; but the effluviums of amber, jet, and other electrical concretes, though by their effects upon the particular bodies diſpoſed to receive their action, they ſeem to fall under the cognizance of our ſight, yet do they not as electrical immediately affect any of our ſenſes, as do the bodies, whether minute or greater, that we ſee, feel, taſte, *&c.* But (continues *Carneades)* becauſe you may expect I ſhould, as the chymiſts do, conſider only the ſenſible ingredients of mixt bodies, let us now ſee what experience will, even as to theſe, ſuggeſt to us.

It ſeems then queſtionable enough, whether from grapes variouſly ordered there may not be drawn more diſtinct ſubſtances by the help of the fire, than from moſt other mixt bodies. For the grapes themſelves being dried into raiſins and diſtilled, will (beſides alkali, phlegm, and earth) yield a conſiderable quantity of an empyreumatical oil, and a ſpirit of a very different nature from that of wine. Alſo the unfermented juice of grapes affords other diſtilled liquors than wine doth. The juice of grapes, after fermentation, will yield a *ſpiritus ardens*; which, if competently rectified, will all burn away without leaving any thing remaining. The ſame fermented juice degenerating into vinegar yields an acid and corroding ſpirit. The ſame juice tunned up arms itſelf with tartar; out of which may be ſeparated, as out of other bodies, phlegm, ſpirit, oil, ſalt and earth: not to mention what ſubſtances may be drawn from the vine itſelf, probably differing from thoſe, which are ſeparated from tartar, which is a body by itſelf, that has few reſemblers in the world. And I will further conſider, that what force ſoever you will allow this inſtance, to evince, that there are ſome bodies, that yield more elements than others, it can ſcarce be denied, but that the major part of bodies, that are diviſible into elements, yield more than three. For, beſides thoſe, which the chymiſts are pleaſed to name hypoſtatical, moſt bodies contain two others, phlegm and earth; which concurring as well as the reſt to the conſtitution of mixts, and being as generally, if not more, found in their analyſis, I ſee no ſufficient cauſe, why they ſhould be excluded from the number of elements. Nor will it ſuffice to object, as the Paracelſians are wont to do, that the *tria prima* are the moſt uſeful elements, and the earth and water but worthleſs and unactive; for elements being called ſo, in relation to the conſtituting of mixt bodies, it ſhould be upon the account of its ingrediency, not of its uſe, that any thing ſhould be affirmed or denied to be an element: and as for the pretended uſefulneſs of earth and water, it would be conſidered, that uſefulneſs, or the want of it, denotes only a reſpect or relation to us; and therefore the preſence, or abſence of it, alters not the intrinſick nature of the thing. The hurtful teeth of vipers are, for aught I know, uſeleſs to us, and yet are not to be denied to be parts of their bodies; and it were hard to ſhew of what greater uſe to us, than phlegm and earth, are thoſe undiſcerned ſtars, which our new teleſcopes diſcover to us, in many blanched places of the ſky: and yet we cannot but acknowledge them conſtituent, and conſiderably great parts of the univerſe. Beſides that, whether or no the phlegm and earth be immediately uſeful, but neceſſary to conſtitute the body whence, they are ſeparated; and conſequently, if the mixt body be not uſeleſs to us, thoſe conſtituent parts, without which it could not have been that mixed body, may be ſaid not to be unuſeful to us: and though the earth and water be not ſo conſpicuouſly operative (after ſeparation) as the other three more active principles, yet in this caſe it will not be amiſs to remember the lucky fable of *Menenius Agrippa*, of the dangerous ſedition of the hands and legs, and other more buſy parts of the body, againſt the ſeemingly unactive ſtomach. And to this caſe alſo we may not unfitly apply that reaſoning of an Apoſtle, to another purpoſe; *If the ear ſhall ſay, becauſe I am not the eye, I am not of the body; is it therefore not of the body? If the whole body were eye, where were the hearing? If the whole were for hearing, where the ſmelling?* In a word, ſince earth and water appear, as clearly and as generally as the other principles, upon the reſolution of bodies, to be the ingredients, whereof they are made up; and ſince they are uſeful (if not immediately to us or rather to phyſicians) to the bodies they conſtitute, and ſo, though in ſomewhat a remoter way, are ſerviceable to us; to exclude them out of the number of elements, is not to imitate nature.

AND on this occaſion I cannot but take notice, that whereas the great argument, which the chymiſts are wont to employ to vilify earth and water, and make them be looked upon as uſeleſs and unworthy to be reckoned among the principles of mixed bodies, is, that they are not endowed with ſpecifick properties, but only with elementary qualities; of which they uſe to ſpeak very ſlightingly, as of qualities contemptible and unactive; I ſee no ſufficient reaſon for this practice of the chymiſts: for it is confeſſed, that heat is an elementary quality, and yet that an almoſt innumerable company of conſiderable things are performed by heat, is manifeſt to them, that duly conſider the various phænomena, wherein it intervenes as a principal actor; and none ought leſs to ignore or diſtruſt this truth than a chymiſt; ſince almoſt all the operations and productions of his art are performed chiefly by the means of heat. And as for cold itſelf, upon whoſe account they ſo deſpiſe the earth and water, if they pleaſe to read in the voyages of our Engliſh and Dutch navigators in *Nova Zembla* and other northern regions, what ſtupendous things may be effected by cold, they would not perhaps think it ſo deſpicable. And not to repeat what I lately recited to you out of *Paracelſus* himſelf, who by the help of an intenſe cold teaches to ſeparate the quinteſſence of wine; I will only now obſerve to you, that the conſervation of the texture of many bodies, both animate and inanimate, does ſo much depend upon the convenient motion both of their own fluid and looſer parts, and of the ambient bodies, whether air, water, *&c.* that not only in human bodies we ſee, that the immoderate or unſeaſonable coldneſs of the air (eſpecially when it finds ſuch bodies over-heated) does very frequently diſcompoſe the œconomy of them, and occaſion variety of diſeaſes; but in the ſolid and durable body of iron itſelf, in which one would not expect, that ſudden cold ſhould produce any notable change, it may have ſo great an operation, that if you take a wire, or other ſlender piece of ſteel, and having brought it in the fire to a white heat, you ſuffer it afterwards to cool leiſurely in the air, it will, when it is cold, be much of the ſame hardneſs it was of before. Whereas, if as ſoon as you remove it from the fire, you plunge it into cold water, it will upon the ſudden refrigeration acquire a very much greater hardneſs than it had before; nay, and will become manifeſtly brittle. And that you may not impute this to any peculiar quality in the water, or other liquor, or unctuous matter, wherein ſuch heated ſteel is wont to be quenched, that it may be tempered; I know a very ſkilful tradeſman, that divers times hardens ſteel by ſuddenly cooling it in a body, that is neither a liquor, nor ſo much as moiſt. A trial of that nature I remember I have ſeen made. And, however, by the operation, that water has upon ſteel quenched in it, whether upon the account of its coldneſs and moiſture, or upon that of any other of its qualities, it appears, that water is not always ſo inefficacious and contemptible a body, as our chymiſts would have it paſs for. And, what I have ſaid of the efficacy of cold and heat, might perhaps be eaſily enough carried further by other conſiderations and experiments; were it not, that having been mentioned only upon the by, I muſt not inſiſt on it, but proceed to another ſubject.

BUT (purſues *Carneades*) though I think it evident, that earth and phlegm are to be reckoned among the elements of moſt animal and vegetable bodies, yet it is not upon that account alone, that I think divers bodies reſoluble into more ſubſtances than three. For there are two experiments, that I have ſometimes made, to ſhew, that at leaſt ſome mixts are diviſible into more diſtinct ſubſtances than five. The one of theſe experiments, though it will be more ſeaſonable for me to mention it fully anon, yet in the mean time, I ſhall tell you thus much of it; that out of two diſtilled liquors, which paſs for elements of the bodies, whence they are drawn, I can, without addition, make a true yellow and inflammable ſulphur, notwithſtanding that the two liquors remain afterwards diſtinct. Of the other experiment, which, perhaps, will not be altogether unworthy your notice, I muſt now give you this particular account: I had long obſerved, that by the diſtillation of divers woods, both in ordinary, and ſome unuſual ſorts of veſſels, the copious ſpirit, that came over, had, beſides a ſtrong taſte to be met with in the empyreumatical ſpirits of many other bodies, an acidity almoſt like that of vinegar: wherefore I ſuſpected, that though the ſouriſh liquor diſtilled, for inſtance, from box-wood, be looked upon by chymiſts as barely the ſpirit of it, and therefore as one ſingle element or principle; yet it does really conſiſt of two differing ſubſtances, and may be diviſible into them; and conſequently, that ſuch woods and other mixts, as abound with ſuch a vinegar, may be ſaid to conſiſt of one element or principle, more than the chymiſts as yet are aware of. Wherefore bethinking myſelf, how the ſeparation of theſe two ſpirits might be made, I quickly found, that there were ſeveral ways of compaſſing it. But that of them, which I ſhall at preſent mention, was this: having diſtilled a quantity of box-wood *per ſe*, and ſlowly rectified the ſouriſh ſpirit, the better to free it both from oil and phlegm, I caſt into this rectified liquor a convenient quantity of powdered coral, expecting that the acid part of the liquor would corrode the coral, and being aſſociated with it, would be ſo retained by it, that the other part of the liquor, which was not of an acid nature, nor fit to faſten upon the corals, would be permitted to aſcend alone. Nor was I deceived in my expectation; for having gently abſtracted the liquor from the corals, there came over a ſpirit of a ſtrong ſmell, and of a taſte very piercing, but without any ſourneſs; and which was in divers qualities manifeſtly different, not only from a ſpirit of vinegar, but from ſome ſpirit of the ſame wood, that I purpoſely kept by me without depriving it of its acid ingredient. And to ſatisfy you, that theſe two ſubſtances were of a very differing nature, I might inform you of ſeveral trials, that I made, but muſt not name ſome of them, becauſe I cannot do ſo without making ſome unſeaſonable diſcoveries. Yet this I ſhall tell you at preſent, that the ſour ſpirit of box not only would, as I juſt now related, diſſolve corals, which the other would not faſten on, but being poured upon ſalt of tartar, would immediately boil and hiſs, whereas the other would lie quietly upon it. The acid ſpirit poured upon minium made a ſugar of lead, which I did not find the other to do: ſome drops of this penetrant ſpirit being mingled with ſome drops of the blue ſyrup of violets, ſeemed rather to dilute, than otherwiſe alter the colour; whereas the acid ſpirit turned the ſyrup of a reddiſh colour, and would probably have made it of as pure a red, as acid ſalts are wont to do, had not its operation been hindered by the mixture of the other ſpirit. A few drops of the compound ſpirit being ſhaken into a pretty quantity of the infuſion of lignum nephriticum, preſently deſtroyed all the bluiſh colour, whereas the other ſpirit would not take it away. To all which it might be added, that having for trial's ſake poured fair water upon the corals, that remained in the bottom of the glaſs, wherein I had rectified the double ſpirit (if I may ſo call it) that was firſt drawn from the box, I found, according to my expectation, that the acid ſpirit had really diſſolved the corals, and had coagulated with them. For by the affuſion of fair water, I obtained a ſolution, which (to note that ſingularity upon the by) was red, whence the water being evaporated, there remained a ſoluble ſubſtance much like the ordinary ſalt of coral, as chymiſts are pleaſed to call that magiſtery of corals, which they make by diſſolving them in common ſpirit of vinegar, and abſtracting the menſtruum *ad ſiccitatem*. I know not whether I ſhould ſubjoin on this occaſion, that the ſimple ſpirit of box, if chymiſts will have it therefore ſaline becauſe it has a ſtrong taſte, will furniſh us with a new kind of ſaline bodies, differing from thoſe hitherto taken notice of. For whereas of the three chief ſorts of ſalts, the acid, the alcalizate, and the ſulphureous, there is none, that ſeems to be friends with both the other two, as I may, ere it be long, have occaſion to ſhew; I did not find, but that the ſimple ſpirit of box did agree very well (at leaſt as far as I had occaſion to try it) both with the acid and the other ſalts. For though it would lie very quiet with ſalt of tartar, ſpirit of urine, or other bodies, whoſe ſalts were either of an alcalizate or fugitive nature; yet did not the mingling of oil of vitriol itſelf produce any hiſſing or efferveſcence, which you know is wont to enſue upon the affuſion of that highly acid liquor upon either of the bodies newly mentioned.

I THINK myſelf, (ſays *Eleutherius*) beholden to you for this experiment; not only becauſe I foreſee you will make it helpful to you in the enquiry you are now upon, but becauſe it teaches us a method, whereby we may prepare a numerous ſort of new ſpirits, which though more ſimple than any that are thought elementary, are manifeſtly endowed with peculiar and powerful qualities, ſome of which may probably be of conſiderable uſe in phyſick, as well alone as aſſociated with other things; as one may hopefully gueſs by the redneſs of that ſolution your ſour ſpirit made of corals, and by ſome other circumſtances of your narrative. And ſuppoſe (purſues *Eleutherius*) that you are not ſo confined, for the ſeparation of the acid parts of theſe compound ſpirits from the other, to employ corals; but that you may as well make uſe of any alcalizate ſalt, or of pearls, or crabs eyes, or any other body, upon which common ſpirit of vinegar will eaſily work, and, to ſpeak in an *Helmontial* phraſe, exantlate itſelf.

I HAVE not yet tried (ſays *Carneades*) of what uſe the mentioned liquors may be in phyſic, either as medicines or as menſtruums: but I could mention now (and may another time) divers of the trials, that I made to ſatisfy myſelf of the difference of theſe two liquors. But that, as I allow your thinking what you newly told me about corals, I preſume you will allow me, from what I have ſaid already, to deduce this corollary; that there are divers compound bodies, which may be reſolved into four ſuch differing ſubſtances, as may as well merit the name of principles, as thoſe to which the chymiſts freely give it. For ſince they ſcruple not to reckon that, which I call the compound ſpirit of box, for the ſpirit, or, as others would have it, the mercury of that wood; I ſee not, why the acid liquor, and the other, ſhould not each of them, eſpecially that laſt named, be looked upon as more worthy to be called an elementary principle; ſince it muſt needs be of a more ſimple nature than the liquor, which was found to be diviſible into that, and the acid ſpirit. And this further uſe (continues *Carneades*) may be made of our experiment to my preſent purpoſe, that it may give us a riſe to ſuſpect, that ſince a liquor reputed, by the chymiſts to be, without diſpute, homogeneous, is, by ſo ſlight a way, diviſible into two diſtinct and more ſimple ingredients, ſome more ſkilful or happier experimenter than I, may find a way either further to divide one of theſe ſpirits, or to ſolve ſome or other, if not all, of thoſe other ingredients of mixt bodies, that have hitherto paſſed among chymiſts for their elements or principles.

THE

SCEPTICAL CHYMIST:

PART IV.

AND thus much (ſays *Carneades*) may ſuffice to be ſaid of the number of the diſtinct ſubſtances ſeparable from mixt bodies by the fire: wherefore I now proceed to conſider the nature of them, and ſhew you, that though they ſeem homogeneous bodies, yet have they not the purity and ſimplicity that is requiſite to elements; and I ſhould immediately proceed to the proof of my aſſertion, but that the confidence, wherewith chymiſts are wont to call each of the ſubſtances we ſpeak of by the name of ſulphur or mercury, or the other of the hypoſtatical principles, and the intolerable ambiguity they allow themſelves in their writings and expreſſions, makes it neceſſary for me, in order to the keeping you either from miſtaking me, or thinking I miſtake the controverſy, to take notice to you, and complain of the unreaſonable liberty they give themſelves of playing with names at pleaſure. And indeed if I were obliged in this diſpute, to have ſuch regard to the phraſeology of each particular chymiſt, as not to write any thing, which this or that author may not pretend, not to contradict this or that ſenſe, which he may give us as occaſion ſerves to his ambiguous expreſſions, I ſhould ſcarce know how to diſpute, nor which way to turn myſelf. For I find that even eminent writers (ſuch as *Raymund Lully*, *Paracelſus*, and others) do ſo abuſe the terms they employ, that as they will now and then give divers things one name; ſo they will oftentimes give one thing many names; and ſome of them (perhaps) ſuch, as do much more properly ſignify ſome diſtinct body of another kind; nay, even in technical words or terms of art, they refrain not from this confounding liberty; but will, as I have obſerved, call the ſame ſubſtance, ſometimes the ſulphur, and ſometimes the mercury of a body. And now I ſpeak of mercury, I cannot but take notice, that the deſcriptions they give us of that principle or ingredient of mixt bodies, are ſo intricate, that even thoſe, that have endeavoured to poliſh and illuſtrate the notions of the chymiſts, are fain to confeſs, that they know not what to make of it either by ingenuous acknowledgments, or deſcriptions, that are not intelligible.

I MUST confeſs (ſays *Eleutherius*) I have in the reading of *Paracelſus*, and other chymical authors, been troubled to find, that ſuch hard words and equivocal expreſſions, as you juſtly complain of, do, even when they treat of principles, ſeem to be ſtudiouſly affected by thoſe writers; whether to make themſelves to be admired by their readers, and their art appear more venerable and myſterious, or (as they would have us think) to conceal from them a knowledge themſelves judge ineſtimable.

BUT whatever (ſays *Carneades*) theſe men may promiſe themſelves from a canting way of delivering the principles of nature, they will find the major part of knowing men ſo vain, as when they underſtand not what they read, to conclude, that it is rather the writer's fault than their own. And thoſe, that are ſo ambitious to be admired by the vulgar, that rather than go without the admiration of the ignorant they will expoſe themſelves to the contempt of the learned, thoſe ſhall, by my conſent, freely enjoy their option. As for the myſtical writers ſcrupling to communicate their knowledge, they might leſs to their own diſparagement, and to the trouble of their readers, have concealed it by writing no books, than by writing bad ones. If *Themiſtius* were here, he would not ſtick to ſay, that chymiſts write thus darkly, not becauſe they think their notions too precious to be explained, but becauſe they fear, that if they were explained, men would diſcern, that they are far from being precious. And indeed I fear, that the chief reaſon, why chymiſts have written ſo obſcurely of their three principles, may be, that not having clear and diſtinct notions of them themſelves, they cannot write otherwiſe than confuſedly of what they but confuſedly apprehend: not to ſay, that divers of them, being conſcious to the invalidity of their doctrine, might well enough diſcern, that they could ſcarce keep themſelves from being confuted, but by keeping themſelves from being clearly underſtood. But though much may be ſaid to excuſe the chymiſts when they write darkly, and ænigmatically, about the preparation of their elixir, and ſome few other grand arcana, the divulging of which they may, upon grounds plauſible enough, eſteem unfit; yet when they pretend to teach the general principles of natural philoſophers, this equivocal way of writing is not to be endured. For in ſuch ſpeculative enquiries, where the naked knowledge of the truth is the thing principally aimed at, what does he teach me worth thanks, that does not, if he can, make his notion intelligible to me, but by myſtical terms, and ambiguous phraſes darkens what he ſhould clear up; and makes me add the trouble of gueſſing at the ſenſe of what he equivocally expreſſes, to that of examining the truth of what he ſeems to deliver? And if the matter of the philoſopher's ſtone, and the manner of preparing it, be ſuch myſteries, as they would have the world believe them, they may write intelligibly and clearly of the principles of mixt bodies in general, without diſcovering what they call the great work. But for my part (continues *Carneades*) what my indignation at this un-philoſophical way of teaching principles has now extorted from me, is meant chiefly to excuſe myſelf, if I ſhall hereafter oppoſe any particular opinion or aſſertion, that ſome follower of *Paracelſus* or any eminent artiſt may pretend not to be his maſter's. For, as I told you long ſince, I am not obliged to examine private men's writings (which were a labour as endleſs as unprofitable) being only engaged to examine thoſe opinions about the *tria prima*, which I find thoſe chymiſts I have met with to agree in moſt: and I doubt not, but my arguments againſt their doctrine will be in great part eaſily enough applicable even to thoſe private opinions, which they do not ſo directly and expreſsly oppoſe. And indeed, that, which I am now entering upon, being the conſideration of the things themſelves, whereinto Spagyriſts reſolve mixt bodies by the fire, if I can ſhew, that theſe are not of an elementary nature, it will be no great matter what names theſe or thoſe chymiſts have been pleaſed to give them. And I queſtion not, that to a wiſe man, and conſequently to *Eleutherius*, it will be leſs conſiderable to know, what men have thought of things, than what they ſhould have thought.

In the fourth and laſt place, then, I conſider, that as generally as chymiſts are wont to appeal to experience, and as confidently as they uſe to inſtance the ſeveral ſubſtances ſeparated by the fire from a mixt body, as a ſufficient proof of their being its component elements; yet thoſe differing ſubſtances are many of them far enough from elementary ſimplicity, and may be yet looked upon as mixt bodies, moſt of them alſo retaining, ſomewhat at leaſt, if not very much, of the nature of thoſe concretes whence they were forced. I am glad (ſays *Eleutherius*) to ſee the vanity or envy of the canting chymiſts thus diſcovered and chaſtiſed; and I could wiſh, that learned men would conſpire together to make theſe deluding writers ſenſible, that they muſt no longer hope with impunity to abuſe the world. For whilſt ſuch men are quietly permitted to publiſh books with promiſing titles, and therein to aſſert what they pleaſe, and contradict others, and even themſelves as they pleaſe, with as little danger of being confuted as of being underſtood, they are encouraged to get themſelves a name, at the coſt of the readers, by finding, that intelligent men are wont, for the reaſon newly mentioned, to let their books and them alone: and the ignorant and credulous (of which the number is ſtill much greater than that of the other) are forward to admire moſt what they leaſt underſtand. But if judicious men, ſkilled in chymical affairs, ſhall once agree to write clearly and plainly of them, and thereby keep men from being ſtunned, as it were, or impoſed upon by dark or empty words; it is to be hoped, that theſe men finding, that they can no longer write impertinently and abſurdly, without being laughed at for doing ſo, will be reduced either to write nothing, or books, that may teach us ſomething, and not rob men, as formerly, of invaluable time; and ſo ceaſing to trouble the world with riddles or impertinencies, we ſhall either by their books receive an advantage, or by their ſilence eſcape an inconvenience.

But after all this is ſaid (continues *Eleutherius*) it may be repreſented in favour of the chymiſts, that, in one regard the liberty they take in uſing names, if it be excuſable at any time, may be more ſo when they ſpeak of the ſubſtances, whereinto their analyſis reſolves mixt bodies: ſince as parents have the right to name their own children, it has ever been allowed to the authors of new inventions, to impoſe names upon them. And therefore the ſubjects we ſpeak of being ſo the productions of the chymiſts art, as not to be otherwiſe, but by it, obtainable; it ſeems but equitable to give the artiſts leave to name them as they pleaſe: conſidering alſo, that none are ſo fit and likely to teach us what thoſe bodies are, as they to whom we owed them.

I told you already (ſays *Carneades*) that there is great difference betwixt the being able to make experiments, and the being able to give a philoſophical account of them. And I will not now add, that many a mine-digger may meet, whilſt he follows his work, with a gem or a mineral which he knows not what to make of, till he ſhews it a jeweller or a mineraliſt to be informed what it is. But that which I would rather have here obſerved, is, that the chymiſts I am now in debate with have given up the liberty you challenged for them, of uſing names at pleaſure, and confined themſelves by their deſcriptions, though but ſuch as they are, of their principles; ſo that although they might freely have called any thing their analyſis preſents them with, either ſulphur, or mercury, or gas, or blas, or what they pleaſed; yet when they have told me, that ſulphur (for inſtance) is a primogeneal and ſimple body, inflammable, odorous, *&c.* they muſt give me leave to diſbelieve them, if they tell me, that a body, that is either compounded or uninflammable, is ſuch a ſulphur; and to think they play with words, when they teach, that gold and ſome other minerals abound with an incombuſtible ſulphur, which is as proper an expreſſion, as a ſunſhine night, or a fluid ice.

BUT before I deſcend to the mention of particulars belonging to my fourth conſideration, I think it convenient to premiſe a few generals; ſome of which I ſhall the leſs need to inſiſt on at preſent, becauſe I have touched on them already.

AND firſt I muſt invite you to take notice of a certain paſſage in *Helmont*[*]; which though I have not found much heeded by his readers, he himſelf mentions as a notable thing, and I take to be a very conſiderable one: for whereas the diſtilled oil-olive, though drawn *per ſe*, is (as I have tried) of a very ſharp and fretting quality, and of an odious taſte, he tells us, that ſimple oil being only digeſted with *Paracelſus*'s *ſal circulatum*, is reduced into diſſimilar parts, and yields a ſweet oil, very differing from the oil diſtilled from ſallet oil; as alſo, that by the ſame way there may be ſeparated from wine a very ſweet and gentle ſpirit, partaking of a far other and nobler quality than that which is immediately drawn by diſtillation, and called dephlegmed aqua vitæ, from whoſe acrimony this other ſpirit is exceedingly remote; although the *ſal circulatum* that makes theſe anatomies be ſeparated from the analyzed bodies, in the ſame weight and with the ſame qualities it had before. Which affirmation of *Helmont*, if we admit to be true, we muſt acknowledge, that there may be a very great diſparity betwixt bodies of the ſame denomination (as ſeveral oils, or ſeveral ſpirits) ſeparable from compound bodies: for beſides the differences I ſhall anon take notice of, betwixt thoſe diſtilled oils that are commonly known to chymiſts, it appears by this, that by means of the *ſal circulatum*, there may be quite another ſort of oils obtained from the ſame body: and who knows, but that there may be yet other agents bound in nature, by whoſe help there may, whether by tranſmutation or otherwiſe, be obtained from the bodies vulgarly called mixt, oils or other ſubſtances, differing from thoſe of the ſame denomination, known either to vulgar chymiſts, or even to *Helmont* himſelf? But for fear you ſhould tell me, that this is but a conjecture grounded upon another man's relation, whoſe truth we have not the means to experiment, I will not inſiſt upon it; but leaving you to conſider of it at leiſure, I ſhall proceed to what is next.

SECONDLY, then, if that be true, which was the opinion of *Leucippus*, *Democritus*, and other prime anatomiſts of old, and is in our days revived by no mean philoſophers; namely, that our culinary fire, ſuch as chymiſts uſe, conſiſts of ſwarms of little bodies ſwiftly moving, which by their ſmallneſs and motion are able to permeate the ſolideſt and compacteſt bodies, and even glaſs itſelf; if this (I ſay) be true, ſince we ſee, that in flints and other concretes, the fiery part is incorporated with the groſſer, it will not be irrational to conjecture, that multitudes of theſe fiery corpuſcles, getting in at the pores of the glaſs, may aſſociate themſelves with the parts of the mixt body whereon they work, and with them conſtitute new kinds of compound bodies, according as the ſhape, ſize, and other affections of the parts of the diſſipated body happen to diſpoſe them, in reference to ſuch combinations; of which alſo there may be the greater number, if it be likewiſe granted, that the corpuſcles of the fire, though all exceeding minute, and very ſwiftly moved, are not all of the ſame bigneſs, nor figure: and if I had not weightier conſiderations to diſcourſe to you of, I could name to you, to countenance what I have newly ſaid, ſome particular experiments, by which I have been induced to think, that the particles of an open fire working upon ſome bodies may really aſſociate themſelves therewith, and add to the quantity. But becauſe I am not ſure, that when the fire works upon bodies included in glaſſes, it does it by a real trajection of the fiery corpuſcles themſelves, through the ſubſtance of the glaſs, I will proceed to what is next to be mentioned.

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[*] *Illud notabile, in vino eſſe ſpiritum quendam mitiorem ulterioris & nobilioris qualitatis participem, quam qui immediatè per diſtillationem elicitur, diciturque aqua vitæ dephlegmata, quod facilius in ſimplici olivarum oleo ad oculum ſpectatur. Quippe diſtillatum oleum abſque latorum aut tegularum additamento, quodque oleum philoſophorum dicitur, multum differt ab ejus oleitate, quæ elicitur prius reducto oleo ſimplici in partes diſſimilares ſola digeſtione & ſalis circulati Paracelſici appoſitione; ſiquidem ſal circulatum idem in pondere & quantitatibus priſtinis ab oleo ſegregatur, poſtquam oleum olivarum in ſui heterogeneitates eſt diſpoſitum. Dulce enim tunc oleum olivarum ex oleo, prout & ſuaviſſimus vini ſpiritus à vino hoc pacto ſeparantur, longeque ab aquæ vitæ acrimonia diſtinctus.* Helmont. Aura vitalis, pag. 725.

I COULD (ſays *Eleutherius)* help you to ſome proofs, whereby I think it may be made very probable, that when the fire acts immediately upon a body, ſome of its corpuſcles may ſtick to thoſe of the burnt body, as they ſeem to do in quick-lime, but in greater numbers and more permanently. But for fear of retarding your progreſs, I ſhall deſire you to defer this inquiry till another time, and proceed as you intended.

YOU may then, in the next place (ſays *Carneades)* obſerve with me, that not only there are ſome bodies, as gold and ſilver, which do not by the uſual examens, made by fire, diſcover themſelves to be mixt; but if (as you may remember I formerly told you) it be a de-compound body, that is diſſipable into ſeveral ſubſtances, by being expoſed to the fire it may be reſolved into ſuch, as are neither elementary, nor ſuch as it was upon its laſt mixture compounded of; but into new kinds of mixts. Of this I have already given you ſome examples in ſoap, ſugar of lead, and vitriol. Now if we ſhall conſider, that there are ſome bodies, as well natural (as that I laſt named) as factitious, manifeſtly de-compounded; that in the bowels of the earth nature may, as we ſee ſhe ſometimes does, make ſtrange mixtures; that animals are nouriſhed with other animals and plants; and that theſe themſelves have almoſt all of them their nutriment and growth, either from a certain nitrous juice harboured in the pores of the earth, or from the excrements of animals, or from the putrified bodies, either of living creatures or vegetables, or from other ſubſtances of a compounded nature; if, I ſay, we conſider this, it may ſeem probable, that there may be among the works of nature (not to mention thoſe of art) a greater number of de-compound bodies, than men take notice of; and indeed, as I have formerly alſo obſerved, it does not at all appear, that all mixtures muſt be of elementary bodies; but it ſeems far more probable, that there are divers ſorts of compound bodies, even in regard of all or ſome of their ingredients, conſidered antecedently to their mixture. For though ſome ſeem to be made up by the immediate coalitions of the elements, or principles themſelves, and therefore may be called *prima miſta*, or *miſta primaria*; yet it ſeems, that many other bodies are mingled (if I may ſo ſpeak) at the ſecond hand, their immediate ingredients being not elementary, but theſe primary mixt newly ſpoken of; and from divers of thoſe ſecondary ſorts of mixt may reſult, by a further compoſition, a third ſort, and ſo onwards. Nor is it improbable, that ſome bodies are made up of mixt bodies, not all of the ſame order, but of ſeveral; as (for inſtance) a concrete may conſiſt of ingredients, whereof the one may have been a primary, the other a ſecondary mixt body; (as I have in native cinnaber, by my way of reſolving it, found both that coarſer part, that ſeems more properly to be ore, and a combuſtible ſulphur, and a running mercury:) or perhaps without any ingredient of this latter ſort, it may be compoſed of mixt bodies, ſome of them of the firſt, and ſome of the third kind. And this may perhaps be ſomewhat illuſtrated by reflecting upon what happens in ſome chymical preparations of thoſe medicines which they call their bezoardicums. For firſt, they take antimony and iron, which may be looked upon as *prima miſta*; of theſe they compound a ſtarry regulus, and to this they add, according to their intention, either gold or ſilver, which makes with it a new and further compoſition. To this they add ſublimate, which is itſelf a de-compound body (conſiſting of common quickſilver, and divers ſalts united by ſublimation into a cryſtalline ſubſtance) and from this ſublimate, and the other metalline mixtures, they draw a liquor, which may be allowed to be of a yet more compounded nature. If it be true, as chymiſts affirm it, that by this art ſome of the gold or ſilver mingled with the regulus may be carried over the helm with it by the ſublimate; as indeed a ſkilful and candid perſon complained to me a while ſince, that an experienced friend of his and mine, having by ſuch a way brought over a great deal of gold, in hope to do ſomething further with it, which might be gainful to him, has not only miſſed of his aim, but is unable to recover his volatilized gold out of the antimonial butter, wherewith it is ſtrictly united.

Now (continues *Carneades*) if a compound body conſiſt of ingredients, that are not merely elementary; it is not hard to conceive, that the ſubſtances, into which the fire diſſolves it, though ſeemingly homogeneous enough, may be of a compounded nature, thoſe parts of each body, that are moſt of kin, aſſociating themſelves into a compound of a new kind. As when (for example ſake) I have cauſed vitriol and ſal armoniac, and ſalt-petre to be mingled and diſtilled together, the liquor, that came over, manifeſted itſelf not to be either ſpirit of nitre, or of ſal armoniac, or of vitriol. For none of theſe would diſſolve crude gold, which yet my liquor was able readily to do; and thereby manifeſted itſelf to be a new compound, conſiſting at leaſt of ſpirit of nitre, and ſal armoniac (for the latter diſſolved in the former, will work on gold) which nevertheleſs are not by any known way ſeparable, and conſequently would not paſs for a mixt body, if we ourſelves did not, to obtain it, put and diſtil together divers concretes, whoſe diſtinct operations were known beforehand. And to add on this occaſion the experiment I lately promiſed you, becauſe it is applicable to our preſent purpoſe, I ſhall acquaint you, that ſuſpecting the common oil of vitriol not to be altogether ſuch a ſimple liquor, as chymiſts preſume it, I mingled it with an equal or a double quantity (for I tried the experiment more than once) of common oil of turpentine, ſuch as together with the other liquor I bought at the drugſters. And having carefully (for the experiment is nice, and ſomewhat dangerous) diſtilled the mixture in a ſmall glaſs retort, I obtained, according to my deſire, (beſides the two liquors I had put in) a pretty quantity of a certain ſubſtance, which ſticking all about the neck of the retort, diſcovered itſelf to be ſulphur, not only by a very ſtrong ſulphureous ſmell, and by the colour of brimſtone; but alſo by this, that being put upon a coal, it was immediately kindled, and burned like common ſulphur. And of this ſubſtance I have yet by me ſome little parcels, which you may command and examine when you pleaſe. So that from this experiment I may deduce either one, or both of theſe propoſitions, that a real ſulphur may be made by the conjunction of two ſuch ſubſtances as chymiſts take for elementary, and which did not either of them apart appear to have any ſuch body in it; or, that oil of vitriol, though a diſtilled liquor, and taken for part of the ſaline principle of the concrete, that yields it, may yet be ſo compounded a body, as to contain, beſide its ſaline part, a ſulphur like common brimſtone, which would hardly be itſelf a ſimple or un-compounded body.

I might (purſues *Carneades)* remind you, that I formerly repreſented it as poſſible, that as there may be more elements than five, or ſix; ſo the elements of one body may be different from thoſe of another: whence it would follow, that from the reſolution of de-compound bodies there may reſult mixts of an altogether new kind, by the coalition of elements, that never perhaps convened before. I might, I ſay, mind you of this, and add divers things to this ſecond conſideration; but for fear of wanting time, I willingly pretermit them, to paſs on to the third, which is this, that the fire does not always barely reſolve or take aſunder, but may alſo after a new manner mingle and compound together the parts (whether elementary or not) of the body diſſipated by it.

This is ſo evident (ſays *Carneades)* in ſome obvious examples, that I cannot but wonder at their ſupineneſs, that have not taken notice of it. For when wood being burnt in a chimney is diſſipated by the fire into ſmoke and aſhes, that ſmoke compoſes ſoot, which is ſo far from being any one of the principles of the wood, that (as I noted above) you may by a further analyſis ſeparate five or ſix diſtinct ſubſtances from it. And as for the remaining aſhes, the chymiſts themſelves teach us, that by a further degree of fire they may be indiſſolubly united into glaſs. It is true, that the analyſis, which the chymiſts principally build upon, is made, not in the open air, but in cloſe veſſels; but however, the examples lately produced may invite you ſhrewdly to ſuſpect, that heat may as well compound as diſſipate the parts of mixt bodies: and not to tell you, that I have known a vitrification made even in cloſe veſſels, I muſt remind you, that the flowers of antimony, and thoſe of ſulphur, are very mixt bodies, though they aſcend in cloſe veſſels; and that it was in ſtopt glaſſes, that I brought up the whole body of camphire. And whereas it may be objected, that all theſe examples are of bodies forced up in a dry, not a fluid form, as are the liquors wont to be obtained by diſtillation; I anſwer, that beſides it is poſſible, that a body may be changed from conſiſtent to fluid, or from fluid to conſiſtent, without being otherwiſe much altered, as may appear by the eaſineſs, wherewith in winter, without any addition or ſeparation of viſible ingredients, the ſame ſubſtance may be quickly hardened into brittle ice, and thawed again into fluid water; beſides this, I ſay, it ſhould be conſidered, that common quickſilver itſelf, which the eminenteſt chymiſts confeſs to be a mixt body, may be driven over the helm in its priſtine form of quickſilver, and conſequently, in that of a liquor. And certainly it is poſſible, that very compounded bodies may concur to conſtitute liquors; ſince, not to mention, that I have found it poſſible, by the help of a certain menſtruum, to diſtil gold itſelf through a retort, even with a moderate fire; let us but conſider what happens in butter of antimony. For if that be carefully rectified, it may be reduced into a very clear liquor; and yet if you caſt a quantity of fair water upon it, there will quickly precipitate a ponderous and vomitive calx, which made before a conſiderable part of the liquor, and yet is indeed (though ſome eminent chymiſts would have it mercurial) an antimonial body, carried over and kept diſſolved by the ſalts of the ſublimate, and conſequently a compounded one; as you may find, if you will have the curioſity to examine this white powder by a ſkilful reduction. And that you may not think, that bodies, as compounded as flowers of brimſtone, cannot be brought to concur to conſtitute diſtilled liquors; and alſo, that you may not imagine, with divers learned men, that pretend no ſmall ſkill in chymiſtry, that at leaſt no mixt body can be brought over the helm, but by corroſive ſalts; I am ready to ſhew you, when you pleaſe, among other ways of bringing over flowers of brimſtone (perhaps I might add, even mineral ſulphurs) ſome, wherein I employ none but oleaginous bodies to make volatile liquors, in which not only the colour, but (which is a much ſurer mark) the ſmell and ſome operations manifeſt, that there is brought over a ſulphur, that makes part of the liquor.

ONE thing more there is, *Eleutherius* (ſays *Carneades*) which is ſo pertinent to my preſent purpoſe, that, though I have touched upon it before, I cannot but on this occaſion take notice of it. And it is this; that the qualities or accidents, upon whoſe account chymiſts are wont to call a portion of matter by the name of mercury, or ſome other of their principies, are not ſuch, but that it is poſſible as great (and therefore why not the like?) may be produced by ſuch changes of texture, and other alterations, as the fire may make in the ſmall parts of a body. I have already proved, when I diſcourſed of the ſecond general conſideration, by what happens to plants nouriſhed only with fair water, and eggs hatched into chickens, that by changing the diſpoſition of the component parts of a body, nature is able to effect as great changes in a parcel of matter reputed ſimilar, as thoſe requiſite to denominate one of the *tria prima*. And, though *Helmont* do ſomewhere wittily call the fire the deſtructor and the artificial death of things; and, although another eminent chymiſt and phyſician be pleaſed to build upon this, that fire can never generate any thing but fire; yet you will, I doubt not, be of another mind, if you conſider, how many new ſorts of mixed bodies chymiſts themſelves have produced by means of the fire; and particularly, if you conſider, how that noble and permanent body, glaſs, is not only manifeſtly produced by the violent action of the fire, but has never, for aught we know, been produced any other way. And, indeed, it ſeems but an inconſiderate aſſertion of ſome Helmontians, that every ſort of body of a peculiar denomination muſt be produced by ſome ſeminal power; as I think I could evince, if I thought it ſo neceſſary, as it is for me to haſten to what I have further to diſcourſe. Nor need it much move us, that there are ſome, who look upon whatſoever the fire is employed to produce, not as upon natural, but artificial bodies. For there is not always ſuch a difference, as many imagine, betwixt the one and the other; nor is it ſo eaſy as they think, clearly to aſſign that, which properly, conſtantly, and ſufficiently, diſcriminates them. But not to engage myſelf in ſo nice a diſquiſition, it may now ſuffice to obſerve, that a thing is commonly termed artificial, when a parcel of matter is by the artificer's hand, or tools, or both, brought to ſuch a ſhape or form, as he deſigned before-hand in his mind: whereas in many of the chymical productions the effect would be produced, whether the artificer intended it or no; and is oftentimes very much other than he intended or looked for: and the inſtruments employed are not tools artificially faſhioned and ſhaped, like thoſe of tradeſmen, for this or that particular work; but, for the moſt part, agents of nature's own providing, and whoſe chief powers of operation they receive from their own nature or texture, not the artificer. And, indeed, the fire is as well a natural agent as ſeed: and the chymiſt that employs it, does but apply natural agents and patients, who being thus brought together, and acting according to their reſpective natures, perform the work themſelves; as apples, plums, or other fruit, are natural productions, though the gardener bring and faſten together the ſcions and the ſtock, and both water, and do perhaps divers other ways contribute to its bearing fruit. But, to proceed to what

I was going to ſay; you may obſerve with me, *Eleutherius*, that, as I told you once before, qualities ſlight enough may ſerve to denominate a chymical principle. For, when they anatomize a compound body by the fire, if they get a ſubſtance inflammable, and that will not mingle with water, that they preſently call ſulphur; what is ſapid and diſſoluble in water, that muſt paſs for ſalt; whatſoever is fixed and indiſſoluble in water, that they name earth. And I was going to add, that whatſoever volatile ſubſtance they know not what to make of, not to ſay, whatſoever they pleaſe, that they call mercury. But that theſe qualities may either be produced, otherwiſe than by ſuch as they call ſeminal agents, or may belong to bodies of a compounded nature, may be ſhewn, among other inſtances, in glaſs made of aſhes, where the exceeding ſtrong-taſted alcalizate ſalt joining with the earth becomes inſipid, and with it conſtitutes a body; which, though alſo dry, fixed and indiſſoluble in water, is yet manifeſtly a mixed body; and made ſo by the fire itſelf.

AND I remember to our preſent purpoſe, that *Helmont*, amongſt other medicines that he commends, has a ſhort proceſs, wherein, though the directions for practice are but obſcurely intimated, yet I have ſome reaſon not to diſbelieve the proceſs, without affirming or denying any thing about the virtues of the remedy to be made Helmont, p. 412. by it. *Quando* (ſays he) *oleum cinnamomi, &c. ſuo ſali alkali miſcetur abſque omni aqua, trium menſium artificioſa occultaque circulatione, totum in ſalem volatilem commutatum eſt, vere eſſentiam ſui ſimplicis in nobis exprimit, & uſque in prima noſtri conſtitutiva ſeſe ingerit.* A not unlike proceſs he delivers in another place; from whence, if we ſuppoſe him to ſay true, I may argue, that ſince by the fire there may be produced a ſubſtance, that is as well ſaline and volatile, as the ſalt of hartſhorn, blood, *&c.* which paſs for elementary; and ſince that this volatile ſalt is really compounded of a chymical oil and a fixed ſalt, the one made volatile by the other, and both aſſociated by the fire, it may well be ſuſpected, that other ſubſtances, emerging upon the diſſipation of bodies by the fire, may be new ſorts of mixts, and conſiſt of ſubſtances of differing natures: and particularly, I have ſometimes ſuſpected, that ſince the volatile ſalts of blood, hartſhorn, *&c.* are fugitive, and endowed with an exceeding ſtrong ſmell, either that chymiſts do erroneouſly aſcribe all odours to ſulphurs, or that ſuch ſalts conſiſt of ſome oily parts well incorporated with the ſaline ones. And the like conjecture I have alſo made concerning ſpirit of vinegar, which though the chymiſts think one of the principles of that body, and though being an acid ſpirit it ſeems to be much leſs of kin, than volatile ſalts, to ſulphurs; yet, not to mention its piercing ſmell, which I know not with what congruity the chymiſts will deduce from ſalt, I wonder they Tyroc. Chym. *L.* 1. *C.* 4. have not taken notice of what their own *Tyrocinium Chymicum* teaches us concerning the diſtillation of *ſaccharum Saturni*; out of which, *Beguinus* aſſures us, that he diſtilled, beſides a very fine ſpirit, no leſs than two oils, the one blood-red and ponderous, but the other ſwimming upon the top of the ſpirit, and of a yellow colour; of which, he ſays, that he kept then ſome by him, to verify what he delivers. And though I remember not, that I have had two diſtinct oils from ſugar of lead, yet that it will, though diſtilled without addition, yield ſome oil, diſagrees not with my experience. I know the chymiſts will be apt to pretend, that theſe oils are but the volatilized ſulphur of lead; and will perhaps argue it from what *Beguinus* relates, that when the diſtillation is ended, you'll find a *caput mortuum* extremely black, and (as he ſpeaks) *nullius momenti*, as if the body, or at leaſt the chief part of the metal itſelf, were by the diſtillation carried over the helm. But ſince you know, as well as I, that *ſaccharum Saturni* is a kind of magiſtery, made only by calcining of lead *per ſe*, diſſolving it in diſtilled vinegar, and cryſtallizing the ſolution; if I had leiſure to tell you, how differing a thing I did, upon examination, find the *caput mortuum*, ſo ſlighted by *Beguinus*, to be, from what he repreſents it, I believe you would think the conjecture propoſed leſs probable than one or other of theſe three; either that this oil did formerly concur to conſtitute the ſpirit of vinegar, and ſo that what paſſes for a chymical principle, may yet be further reſoluble into diſtinct ſubſtances; or that ſome parts of the ſpirit, together with ſome parts of the lead, may conſtitute a chymical oil, which therefore, though it paſs for homogeneous, may be a very compounded body; or at leaſt, that by the action of the diſtilled vinegar and the ſaturnine calx one upon another, part of the liquor may be ſo altered, as to be tranſmuted from an acid ſpirit into an oil. And though the truth of either of the two former conjectures would make the example I have reflected on, more pertinent to my preſent argument; yet you'll eaſily diſcern, the third and laſt conjecture cannot be unſerviceable to confirm ſome other paſſages of my diſcourſe.

To return then to what I was ſaying juſt before I mentioned *Helmont*'s experiment; I ſhall ſubjoin, that chymiſts muſt confeſs alſo, that in the perfectly dephlegmed ſpirit of wine, or other fermented liquors, that, which they call the ſulphur of the concrete, loſes, by the fermentation, the property of oil, (which the chymiſts likewiſe take to be the true ſulphur of the mixt) of being unmingleable with the water. And if you will credit * *Helmont*, a pound of the pureſt ſpirit of wine may barely, by the help of pure ſalt of tartar, (which is but the fixed ſalt of wine) be reſolved or tranſmuted into ſcarce half an ounce of ſalt, and as much elementary water, as amounts to the remaining part of the mentioned weight. And it may (as I think I formerly alſo noted) be doubted, whether that fixed and alcalizate ſalt, which is ſo unanimouſly agreed on, to be the ſaline principle of incinerated bodies, be not, as it is alcalizate, a production of the fire? For though the taſte of tartar, for example, ſeem to argue, that it contains a ſalt before it be burned; yet that ſalt being very acid, is of a quite differing taſte from the lixiviate ſalt of calcined tartar. And though it be not truly objected againſt the chymiſts, that they obtain all ſalts they make, by reducing the body they work on into aſhes with violent fires, (ſince hartſhorn, amber, blood, and divers other mixts, yield a copious ſalt; before they be burned to aſhes) yet this volatile ſalt differs much, as we ſhall ſee anon, from the fixed alcalizate ſalt I ſpeak of; which, for aught I remember, is not producible by any known way, without incineration. It is not unknown to chymiſts, that quickſilver may be precipitated, without addition, into a dry powder, that remains ſo in water. And ſome eminent Spagyriſts, and even *Raimund Lully* himſelf, teach, that meerly by the fire, quickſilver may in convenient veſſels be reduced (at leaſt in great part) into a thin liquor like water, and mingleable with it. So that by the bare action of the fire, it is poſſible, that the parts of a mixed body ſhould be ſo diſpoſed after new and differing manners, that it may be ſometimes of one conſiſtence, ſometimes of another; and may in one ſtate be diſpoſed to be mingled with water, and in another not. I could alſo ſhew you, that bodies, from which apart chymiſts cannot obtain any thing, that is combuſtible, may, by being aſſociated together, and by the help of the fire, afford an inflammable ſubſtance. And, that on the other ſide, it is poſſible for a body to be inflammable, from which it would very much puzzle any ordinary chymiſt, and perhaps any other, to ſeparate an inflammable principle or ingredient, Wherefore, ſince the principles of chymiſts may receive their denominations from qualities, which it often exceeds not the power of art, nor always that of the fire to produce; and ſince ſuch qualities may be found in bodies, that differ ſo much in other qualities from one another, that they need not be allowed to agree in that pure and ſimple nature, which principles, to be ſo indeed, muſt have; it may juſtly be ſuſpected, that many productions of the fire, that are ſhewed us by chymiſts, as the principles of the concrete, that afforded them, may be but a new kind of mixts. And to annex, on this occaſion, to theſe arguments taken from the nature of the thing, one of thoſe, which logicians call *ad hominem*, I ſhall deſire you to take notice, that though *Paracelſus* himſelf, and ſome, that are ſo miſtaken, as to think he could not be ſo, have ventured to teach, that not only the bodies here below, but the elements themſelves, and all the other parts of the univerſe, are compoſed of ſalt, ſulphur and mercury; yet the learned *Sennertus*, and all the more wary chymiſts, have rejected that conceit, and do many of them confeſs, that the *tria prima* are each of them made up of the four elements; and others of them make earth and water concur with ſalt, ſulphur and mercury, to the conſtitution of mixt bodies. So that one ſort of theſe Spagyriſts, notwithſtanding the ſpecious titles they give to the productions of the fire, do in effect grant what I contend for. And, of the other ſort I may well demand, to what kind of bodies the phlegm and dead earth, to be met with in chymical reſolutions, are to be referred? For either they muſt ſay, with *Paracelſus*, but againſt their own conceſſions, as well as againſt experience, that theſe are alſo compoſed of the *tria prima*, whereof they cannot ſeparate any one from either of them; or elſe they muſt confeſs, that two of the vaſteſt bodies here below, earth and water, are neither of them compoſed of the *tria prima*; and that conſequently thoſe three are not the univerſal, and adequate ingredients, neither of all ſublunary bodies, nor even of all mixed bodies.

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* *Oſtendi aliàs, quomodo lib una aquæ vitæ combibita in ſale tartari ſiccato, vix fiat ſemuncia ſalis, cæterum totum corpus fiat aqua elementalis.* Helmont. in Aura vitali,

I KNOW, that the chief of theſe chymiſts repreſent, that though the diſtinct ſubſtances into which they divide mixt bodies by the fire, are not pure and homogeneous; yet ſince the four elements, into which the Ariſtotelians pretend to reſolve the like bodies by the ſame agent, are not ſimple neither, as themſelves acknowledge, it is as allowable for the chymiſts to call the one *principles*, as for the Peripateticks to call the other *elements*; ſince in both caſes the impoſition of the name is grounded only upon the predominancy of that element, whoſe name is aſcribed to it. Nor ſhall I deny, that this argument of the chymiſts is no ill one againſt the Ariſtotelians. But what anſwer can it prove to me, who, you know, am diſputing as well againſt the Ariſtotelian elements, as the chymical principles, and muſt not look upon any body as a true principle or element, but as yet compounded, which is not perfectly homogeneous, but is further reſoluble into any number of diſtinct ſubſtances, how ſmall ſoever? And as for the chymiſts calling a body ſalt, or ſulphur, or mercury, upon pretence, that the principle of the ſame name is predominant in it, that itſelf is an acknowledgment of what I contend for; namely, that theſe productions of the fire are yet compounded bodies. And yet whilſt this is granted, it is affirmed, but not proved, that the reputed ſalt, or ſulphur, or mercury, conſiſts mainly of one body, that deſerves the name of a principle of the ſame denomination. For how do chymiſts make it appear, that there are any ſuch primitive and ſimple bodies in thoſe we are ſpeaking of; ſince it is upon the matter confeſſed by the anſwer lately made, that theſe are not ſuch? And if they pretend by reaſon to evince what they affirm, what becomes of their confident boaſts, that the chymiſt (whom they therefore, after *Beguinus*, call a *philoſophus* or *opifex ſenſatus*) can convince our eyes, by manifeſtly ſhewing in any mixt body thoſe ſimple ſubſtances he teaches them to be compoſed of?

And indeed, for the chymiſts to have recourſe in this caſe to other proofs than experiments, as it is to wave the grand argument, that has all this while been given out for a demonſtrative one; ſo it releaſes me from the obligation to proſecute a diſpute, wherein I am not engaged to examine any but experimental proofs. I know it may plauſibly enough be repreſented, in favour of the chymiſts, that it being evident, that much the greater part of any thing they call ſalt, or ſulphur, or mercury, is really ſuch; it would be very rigid to deny thoſe ſubſtances the names aſcribed them, only becauſe of ſome ſlight mixture of another body, ſince not only the Peripateticks call particular parcels of matter elementary, though they acknowledge, that elements are not to be any where found pure, at leaſt here below, and ſince eſpecially there is a manifeſt analogy and reſemblance betwixt the bodies obtainable by chymical anatomies, and the principles whoſe names are given them; I have, I ſay, conſidered that theſe things may be repreſented: but as for what is drawn from the cuſtom of the Peripateticks, I have already told you, that though it may be employed againſt them, yet it is not available againſt me, who allow nothing to be an element, that is not perfectly homogeneous. And whereas it is alledged, that the predominant principle ought to give a name to the ſubſtance wherein it abounds; I anſwer, that that might much more reaſonably be ſaid, if either we or the chymiſts had ſeen nature take pure ſalt, pure ſulphur, and pure mercury, and compound of them every ſort of mixt bodies. But, ſince it is to experience that they appeal, we muſt not take it for granted, that the diſtilled oil (for inſtance) of a plant is mainly compoſed of the pure principle called ſulphur, till they have given us an ocular proof, that there is in that ſort of plants ſuch an homogeneous ſulphur. For as for the ſpecious argument, which is drawn from the reſemblance betwixt the productions of the fire, and the reſpective, either Ariſtotelian elements, or chymical prinicples, by whoſe names they are called; it will appear more plauſible than cogent, if you will but recall to mind the ſtate of the controverſy; which is not, whether or no there be obtained from mixt bodies certain ſubſtances, that agree in outward appearance, or in ſome qualities, with quickſilver or brimſtone, or ſome ſuch obvious or copious body; but whether or no all bodies, confeſſed to be perfectly mixt, were compoſed of, and are reſoluble into a determinate number of primary unmixt bodies. For, if you keep the ſtate of the queſtion in your eye, you will eaſily diſcern, that there is much of what ſhould be demonſtrated, left unproved by thoſe chymical experiments we are examining. But (not to repeat what I have already diſcovered more at large) I ſhall now take notice, that it will not preſently follow, that becauſe a production of the fire has ſome affinity with ſome of the greater maſſes of matter here below, that therefore they are both of the ſame nature, and deſerve the ſame name; for the chymiſts are not content, that flame be looked upon as a parcel of the element of fire, though it be hot, dry, and active, becauſe it wants ſome other qualities belonging to the nature of elementary fire. Nor will they let the Peripateticks call aſhes, or quicklime, earth, notwithſtanding the many likeneſſes between them; becauſe they are not taſteleſs, as elementary earth ought to be: but if you ſhould aſk me, what then it is, that all the chymical anatomies of bodies do prove, if they prove not, that they conſiſt of the three principles into which the fire reſolves them; I anſwer, that their diſſections may be granted to prove, that ſome mixt bodies (for in many it will not hold) are by the fire, when they are included in cloſe veſſels, (for that condition alſo is often requiſite) diſſoluble into ſeveral ſubſtances differing in ſome qualities, but principally in conſiſtence. So that out of moſt of them may be obtained a fixt ſubſtance, partly ſaline, and partly inſipid, an unctuous liquor, and another liquor, or more, that without being unctuous have a manifeſt taſte. Now, if chymiſts will agree to call the dry and ſapid ſubſtance, ſalt; the unctuous liquor ſulphur; and the other, mercury; I ſhall not much quarrel with them for ſo doing: but if they will tell me, that ſalt, ſulphur, and mercury are ſimple and primary bodies, whereof each mixt body was actually compounded, and which was really in it antecedently to the operation of the fire, they muſt give me leave to doubt, whether (whatever their other arguments may do) their experiments prove all this. And if they will alſo tell me, that the ſubſtances their anatomies are wont to afford them, are pure and ſimilar, as principles ought to be, they muſt give me leave to believe my own ſenſes, and their own confeſſions, before their bare aſſertions. And that you may not (*Eleutherius*) think I deal ſo rigidly with them, becauſe I ſcruple to take theſe productions of the fire for ſuch, as the chymiſts would have them paſs for, upon the account of their having ſome affinity with them; conſider a little with me, that in regard an element or principle ought to be perfectly ſimilar and homogeneous, there is no juſt cauſe, why I ſhould rather give the body propoſed the name of this or that element or principle, becauſe it has a reſemblance to it in ſome obvious quality, rather than deny it that name upon the account of divers other qualities, wherein the propoſed bodies are unlike; and if you do but conſider, what ſlight and eaſily producible qualities they are, that ſuffice, as I have already more than once obſerved, to denominate a chymical principle or an element, you will not, I hope, think my warineſs to be deſtitute either of example, or elſe of reaſon. For we ſee, that the chymiſts will not allow the Ariſtotelians, that the ſalt in aſhes ought to be called earth, though the ſaline and terreſtrial part ſymbolize in weight, in dryneſs, in fixedneſs and fuſibility, only becauſe the one is ſapid and diſſoluble in water, and the other not: beſides, we ſee that ſapidneſs and volatility are wont to denominate the chymiſts mercury or ſpirit; and yet how many bodies, think you, may agree in thoſe qualities, which may yet be of very differing natures, and diſagree in qualities either more numerous, or more conſiderable, or both? For not only ſpirit of nitre, aqua fortis, ſpirit of ſalt, ſpirit of oil of vitriol, ſpirit of alum, ſpirit of vinegar, and all ſaline liquors diſtilled from animal bodies, but all the acetous ſpirits of woods freed from their vinegar; all theſe, I ſay, and many others muſt belong to the chymiſts mercury, though it appear not, why ſome of them ſhould more be comprehended under one denomination than the chymiſts ſulphur, or oil, ſhould likewiſe be; for their diſtilled oils are alſo fluid, volatile, and taſteable, as well as their mercury: nor is it neceſſary, that their ſulphur ſhould be unctuous or diſſoluble in water, ſince they generally refer ſpirit of wine to ſulphurs, although that ſpirit be not unctuous, and will freely mingle with water. So that bare inflammability muſt conſtitute the eſſence of the chymiſts ſulphur; as uninflammableneſs, joined with any taſte, is enough to intitle a diſtilled liquor to be their mercury. Now ſince I can further obſerve to you, that ſpirit of nitre and ſpirit of hartſhorn being poured together will boil and hiſs, and toſs up one another into the air, which the chymiſts make ſigns of great antipathy in the natures of bodies, (as indeed theſe ſpirits differ much both in taſte, ſmell, and operations) ſince I elſewhere tell you of my having made two ſorts of oil out of the ſame man's blood, that would not mingle with one another; and ſince I might tell you divers examples I have met with, of the contrariety of bodies, which according to the chymiſts muſt be huddled up together under one denomination; I leave you to judge, whether ſuch a multitude of ſubſtances, as may agree in theſe ſlight qualities, and yet diſagree in others more conſiderable, are more worthy to be called by the name of a principle (which ought to be pure and homogeneous) than to have appellations given them, that may make them differ, in name too, from the bodies from which they ſo widely differ in nature. And hence alſo, by the by, you may perceive, that it is not unreaſonable to diſtruſt the chymiſts way of argumentation, when being unable to ſhew us, that ſuch a liquor is (for example) purely ſaline, they prove, that at leaſt ſalt is much the predominant principle, becauſe that the propoſed ſubſtance is ſtrongly taſted, and all taſte proceeds from ſalt; whereas thoſe ſpirits, ſuch as ſpirit of tartar, ſpirit of hartſhorn, and the like, which are reckoned to be the mercuries of the bodies, that afford them, have manifeſtly a ſtrong and piercing taſte, and ſo has (according to what I formerly noted) the ſpirit of box *&c.* even after the acid liquor that concurred to compoſe it has been ſeparated from it. And indeed, if ſapidneſs belong not to the ſpirit or mercurial principle of vegetables and animals, I ſcarce know how it will be diſcriminated from their phlegm; ſince by the abſence of inflammability it muſt be diſtinguiſhed from their ſulphur: which affords me another example, to prove how unaccurate the chymical doctrine is in our preſent caſe; ſince not only the ſpirit of vegetables and animals, but their oils are very ſtrongly taſted, as he, that ſhall but wet his tongue with chymical oil of cinnamon or of cloves, or even turpentine, may quickly find, to his ſmart. And not only I never tried any chymical oils, whoſe taſte was not very manifeſt and ſtrong; but a ſkilful and inquiſitive perſon, who made it his buſineſs by elaborate operations to depurate chymical oils, and reduce them to an elementary ſimplicity, informs us, that he never was able to make them at all taſteleſs: whence I might infer, that the proof chymiſts confidently give us of a body's being ſaline, is ſo far from demonſtrating the predominancy, that it does not clearly evince ſo much as the preſence of the ſaline principle in it. But I will not (purſues *Carneades*) remind you, that the volatile ſalt of hartſhorn, amber, blood *&c.* are exceeding ſtrongly ſcented, notwithſtanding that moſt chymiſts deduce odours from ſulphur, and from them argue the predominancy of that principle in the odorous body; becauſe I muſt not ſo much as add any new examples of the incompetency of this ſort of chymical arguments; ſince having already detained you but too long in thoſe generals, that appertain to my fourth conſideration, it is time, that I proceed to the particulars themſelves, to which I thought fit they ſhould be previous.

THESE generals (continues *Carneades*) being thus premiſed, we might the better ſurvey the unlikeneſs, that an attentive and unprepoſſeſſed obſerver may take notice of in each ſort of bodies, which the chymiſts are wont to call the ſalts of ſulphurs or mercuries of the concretes that yield them, as if they had all a ſimplicity, and identity of nature: whereas ſalts, if they were all elementary, would as little differ as do the drops of pure and ſimple water. It is known, that both chymiſts and phyſicians aſcribe to the fixt ſalts of calcined bodies the virtues of their concretes; and conſequently very differing operations. So we find the alkali of wormwood much commended in diſtempers of the ſtomach; that of eye-bright for thoſe that have a weak ſight; and that of guaiacum (of which a great quantity yields but a very little ſalt) is not only much commended in venereal diſeaſes, but is believed to have a peculiar purgative virtue, which yet I have not had occaſion to try. And though, I confeſs, I have long thought, that theſe alkalizate ſalts are, for the moſt part, very near of kin, and retain very little of the properties of the concretes whence they were ſeparated; yet being minded to obſerve watchfully, whether I could meet with any exceptions to this general obervation, I obſerved at the glaſs-houſe, that ſometimes the metal (as the workmen call it) or maſs of colliquated ingredients, which by blowing they faſhion into veſſels of divers ſhapes, did ſometimes prove of a very differing colour, and a ſomewhat differing texture, from what was uſual. And having inquired, whether the cauſe of ſuch accidents might not be derived from the peculiar nature of the fixt ſalt employed to bring the ſand to fuſion, I found, that the knowingeſt workmen imputed theſe miſ-adventures to the aſhes of ſome certain kind of wood, as having obſerved the ignobler kind of glaſs I lately mentioned to be frequently produced, when they had employed ſuch ſorts of aſhes, which therefore they ſcruple to make uſe of, if they took notice of them beforehand. I remember alſo, that an induſtrious man of my acquaintance having bought a vaſt quantity of tobacco-ſtalks to make a fixt ſalt with, I had the curioſity to go ſee, whether that exotick plant, which ſo much abounds in volatile ſalt, would afford a peculiar kind of alkali; and I was pleaſed to find, that in the lixivium of it it was not neceſſary, as is uſual, to evaporate all the liquor, that there might be obtained a ſaline calx, conſiſting like lime quenched in the air of a heap of little corpuſcles of unregarded ſhapes; but the fixt ſalt ſhot into figured cryſtal, almoſt as nitre or ſal armoniac and other uncalcined ſalts are wont to do. And I further remember, that I have obſerved, in the fixt ſalt of urine, brought by depuration to be very white, a taſte not ſo unlike to that of common ſalt, and very differing from the wonted cauſtick lixiviate taſte of the other ſalts made by incineration. But becauſe the inſtances I have alledged of the difference of alkalizate ſalt are but few, and therefore I am ſtill inclined to think, that moſt chymiſts and many phyſicians do, inconſiderately enough, and without warrant from experience, aſcribe the virtues of the concretes, expoſed to calcination, to the ſalts obtained by it; I ſhall rather, to ſhew the diſparity of ſalts, mention in the firſt place the apparent difference betwixt the vegetable fixt ſalts and the animal volatile ones: as (for example) betwixt ſalt of tartar, and ſalt of hartſhorn; whereof the former is ſo fixt, that it will indure the brunt of a violent fire, and ſtand in fuſion like a metal; whereas the other (beſides, that it has a differing taſte and a very differing ſmell) is ſo far far from being fixt, that it will fly away in a gentle heat as eaſily as ſpirit of wine itſelf. And to this I ſhall add, in the next place, that even among the volatile ſalts themſelves, there is a conſiderable difference, as appears by the diſtinct properties of (for inſtance) ſalt of amber, ſalt of urine, ſalt of man's ſkull, (ſo much extolled againſt the falling ſickneſs) and divers others, which cannot eſcape an ordinary obſerver. And this diverſity of volatile ſalts I have obſerved to be ſometimes diſcernable even to the eye, in their figures. For the ſalt of hartſhorn I have obſerved to adhere to the receiver, in the form almoſt of a parallelopipedon; and of the volatile ſalt of human blood (long digeſted before diſtillation, with ſpirit of wine) I can ſhew you ſtore of grains of that figure, which geometricians call a rhombus; though I dare not undertake, that the figures of theſe or other ſaline cryſtals (if I may ſo call them) will be always the ſame, whatever degree of fire have been employed to force them up, or how haſtily ſoever they been made to convene in the ſpirits or liquors, in the lower part of which, I have uſually obſerved them, after a while to ſhoot. And although, as I lately told you, I ſeldom ſound any difference, as to medical virtues, in the fixt ſalts of divers vegetables; and accordingly I have ſuſpected that moſt of theſe volatile ſalts, having ſo great a reſemblance in ſmell, in taſte, and fugitiveneſs, differ but little, if at all, in their medicinal properties: as indeed I have found them generally to agree in divers of them (as in their being ſomewhat diaphoretick and very deopilative) yet I remember [*] *Helmont* ſomewhere informs us, that there is this difference betwixt the ſaline ſpirit of urine and that of man's blood, that the former will not cure the epilepſy, but the latter will. Of the efficacy alſo of the ſalt of common amber againſt the ſame diſeaſe in children, (for in grown perſons it is not a ſpecifick) I may elſewhere have an occaſion to entertain you. And when I conſider, that to the obtaining of theſe volatile ſalts (eſpecially that of urine)there is not requiſite ſuch a deſtructive violence of the fire, as there is to get thoſe ſalts that muſt be made by incineration, I am the more invited to conclude, that they may differ from one another, and conſequently recede from an elementary ſimplicity. And if I could here ſhew you, what Mr. *Boyle* has obſerved, touching the various chymical diſtinctions of ſalts; you would quickly diſcern, not only that chymiſts do give themſelves a ſtrange liberty to call concretes ſalts, that are, according to their own rules, to be looked upon as very compounded bodies; but that among thoſe very ſalts, that ſeem elementary, becauſe produced upon the anatomy of the bodies, that yield them, there is not only a viſible diſparity, but, to ſpeak in the common language, a manifeſt antipathy or contrariety: as is evident in the ebullition and hiſſing, that is wont to enſue, when the acid ſpirit of vitriol, for inſtance, is poured upon pot-aſhes, or ſalt of tartar. And I ſhall beg leave of this gentleman, (ſays *Carneades*) caſting his eyes on me, to let me obſerve to you, out of ſome of his papers, particularly thoſe, wherein he treats of ſome preparations of urine, that not only one and the ſame body may have two ſalts of a contrary nature, as he exemplifies in the ſpirit and alcali of nitre; but that from the ſame body there may, without addition, be obtained three differing and viſible ſalts. For he relates, that he obſerved in urine, not only a volatile and cryſtalline ſalt, and a fixt ſalt, but likewiſe a kind of ſal armoniac, or ſuch a ſalt as would ſublime in the form of a ſalt, and therefore was not fixt, and yet was far from being ſo fugitive as the volatile ſalt; from which it ſeemed alſo otherwiſe to differ. I have indeed ſuſpected, that this may be a ſal armoniac properly enough ſo called, as compounded of the volatile ſalt of urine, and the fixt of the ſame liquor, which, as I noted, is not unlike ſea-ſalt; but that itſelf argues a manifeſt difference betwixt the ſalts, ſince ſuch a volatile ſalt is not wont to unite thus with an ordinary alkali, but to fly away from it in the heat. And on this occaſion I remember, that to give ſome of my friends an ocular proof of the difference betwixt the fixt and volatile ſalt of (the ſame concrete) wood, I deviſed the following experiment: I took common Venetian ſublimate, and diſſolved as much of it as I well could in fair water; then I took wood-aſhes, and pouring on them warm water, diſſolved their ſalt; and filtrating the water, as ſoon as I found the lixivium ſufficiently ſharp upon the tongue, I reſerved it for uſe. Then on one part of the former ſolution of ſublimate dropping a little of this diſſolved fixt ſalt of wood, the liquors preſently turned of an orange colour; but upon the other part of the clear ſolution of ſublimate putting ſome of the volatile ſalt of wood (which abounds in the ſpirit of ſoot) the liquor immediately turned white, almoſt like milk, and after a while let fall a white ſediment, as the other liquor did a yellow one. To all this that I have ſaid concerning the difference of ſalts, I might add what I formerly told you, concerning the ſimple ſpirit of box, and ſuch like woods, which differ much from the other ſalts hitherto mentioned, and yet would belong to the ſaline principle, if chymiſts did truly teach, that all taſtes proceed from it. And I might alſo annex, what I noted to you out of * *Helmont* concerning bodies, which, though they conſiſt in great part of chymical oils, do yet appear but volatile ſalts: but to inſiſt on theſe things, were to repeat; and therefore I ſhall proceed.

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[*] *Error vero per diſtillationem nobis monſtrat etiam ſpiritum ſalinum plane volatilem odore nequicquam ut nec guſtu deſtinguibilem a ſpiritu urinæ; in eo tamen eſſentialiter diverſum, quod ſpiritus talis cruoris curat epilepſiam, non autem ſpiritus ſalis lotii.* Helmont. Aura Vitalis

THIS diſparity is alſo highly eminent in the ſeparated ſulphurs or chymical oils of things. For they contain ſo much of the ſcent, and taſte, and vertues, of the bodies, whence they were drawn, that they ſeem to be but the material craſis (if I may ſo ſpeak) of their concretes. Thus the oils of cinnamon, cloves, nutmegs and other ſpices, ſeem to be but the united aromatic parts, that did enoble thoſe bodies. And it is a known thing, that oil of cinnamon, and oil of cloves, (which I have likewiſe obſerved in the oils of ſeveral woods) will ſink to the bottom of water; whereas thoſe of nutmegs and divers other vegetables will ſwim upon it. The oil (abuſively called ſpirit) of roſes ſwims at the top of the water in the form of a white butter, which I remember not to have obſerved in any other oil drawn in any limbeck; yet there is a way (not here to be declared) by which I have ſeen it come over in the form of other aromatic oils, to the delight and wonder of thoſe that beheld it. In oil of aniſeeds, which I drew both with, and without fermentation, I obſerved the whole body of the oil in a cool place to thicken into the conſiſtence and appearance of white butter, which, with the leaſt heat, reſumed its former liquidneſs. In the oil of olive drawn over in a retort, I have likewiſe more than once ſeen a ſpontaneous coagulation in the receiver: and I have of it by me thus congealed; which is of ſuch a ſtrangely penetrating ſcent, as if it would perforate the noſes that approach it. The like pungent odour I alſo obſerved in the diſtilled liquor of common ſoap, which forced over from minium, lately afforded an oil of a moſt admirable penetrancy: and he muſt be a great ſtranger, both to the writings and preparations of chymiſts, that ſees not in the oils they diſtill from vegetables and animals, a conſiderable and obvious difference. Nay, I ſhall venture to add, *Eleutherius*, (what perhaps you will think of kin to a paradox, that divers times out of the ſame animal or vegetable, there may be extracted oils of natures obviouſly differing. To which purpoſe I ſhall not inſiſt on the ſwimming and ſinking oils, which I have ſometimes obſerved to float on, and ſubſide under the ſpirit of guaiacum, and that of divers other vegetables diſtilled with a ſtrong and laſting fire; nor ſhall I inſiſt on the obſervation elſewhere mentioned, of the divers and unmingleable oils afforded us by human blood, long fermented and digeſted with ſpirit of wine; becauſe theſe kinds of oils may ſeem chiefly to differ in conſiſtence and weight, being all of them high coloured and aduſt. But the experiment, which I deviſed to make out this difference of the oils of the ſame vegetable, *ad oculum*, (as they ſpeak) was this that follows: I took a pound of aniſeeds, and having groſly beaten them, cauſed them to be put into a very large glaſs retort almoſt filled with fair water; and placing this retort in a ſand-furnace, I cauſed a very gentle heat to be adminiſtered during the firſt day, and a great part of the ſecond, till the water was for the moſt part drawn off, and had brought over with it at leaſt moſt of the volatile and aromatic oil of the ſeeds. And then increaſing the fire, and changing the receiver, I obtained beſides an empyreumatical ſpirit, and a quantity of aduſt oil; whereof a little floated upon the ſpirit, and the reſt was more heavy, and not eaſily ſeparable from it. And whereas theſe oils were very dark, and ſmelled (as the chymiſts ſpeak) ſo ſtrongly of the fire, that their odour did not betray from what vegetables they had been forced; the other aromatic oil was enriched with the genuine ſmell and taſte of the concrete; and ſpontaneouſly coagulating itſelf into white butter, did manifeſt itſelf to be the true oil of aniſeeds: which concrete I therefore choſe to employ about this experiment, that the difference of theſe oils might be more conſpicuous than it would have been, had I, inſtead of it, diſtilled another vegetable.

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* *Aliquando oleum cinnamomi, &c. ſuo ſali alkali miſcetur abſque omni aqua, trium menſium artificioſa occultaque circulatione, totum in ſalem volatilem commutatum eſt.* Helmont. Tria Prima Chymicorum. &c. pag. 412.

I HAD almoſt forgot to take notice, that there is another ſort of bodies, which though not obtained from concretes by diſtillation, many chymiſts are wont to call their ſulphur; not only becauſe ſuch ſubſtances are, for the moſt part, high coloured (whence they are alſo, and that more properly, called tinctures) as diſſolved ſulphurs are wont to be; but eſpecially becauſe they are, for the moſt part, abſtracted and ſeparated from the reſt of the maſs by ſpirit of wine: which liquor thoſe men ſuppoſing to be ſulphureous, they conclude, that what it works upon, and abſtracts, muſt be a ſulphur alſo. And upon this account they preſume, that they can ſequeſter the ſulphur even of minerals and metals; for which it is known, that they cannot by fire alone ſeparate it. To all this I ſhall anſwer, that if theſe ſequeſtered ſubſtances were indeed the ſulphurs of the bodies, whence they are drawn, there would as well be a great diſparity betwixt chymical ſulphurs obtained by ſpirit of wine, as I have already ſhewn there is betwixt thoſe obtained by diſtillation in the form of oils: which will be evident from hence, that not to urge, that themſelves aſcribe diſtinct virtues to mineral tincture of gold againſt ſuch and ſuch diſeaſes; the tincture of antimony, or of its glaſs, againſt others; and the tincture of emerald againſt others; it is plain, that in tinctures drawn from vegetables, if the ſuperfluous ſpirit of wine be diſtilled off, it leaves at the bottom that thicker ſubſtance, which chymiſts uſe to call the extract of the vegetable. And that theſe extracts are endowed with very different qualities, according to the nature of the particular bodies, that afforded them (though I fear ſeldom with ſo much of the ſpecific virtues, as is wont to be imagined) is freely confeſſed both by phyſicians and chymiſts. But, *Eleutherius* (ſays *Carneades*) we may here take notice, that the chymiſts do as well in this caſe, as in many others, allow themſelves a licence to abuſe words: for not again to argue from the differing properties of tinctures, that they are not exactly pure and elementary ſulphurs; they would eaſily appear not to be ſo much as ſulphurs, although we ſhould allow chymical oils to deſerve that name. For however in ſome mineral tinctures, the natural fixedneſs of the extracted body does not always ſuffer it to be eaſily further reſoluble into differing ſubſtances; yet in very many extracts drawn from vegetables, it may very eaſily be manifeſted, that the ſpirit of wine has not ſequeſtered the ſulphureous ingredient from the ſaline and mercureal ones, but has diſſolved (for I take it to be a ſolution) the finer parts of the concrete (without making any nice diſtinction of their being perfectly ſulphureous or not) and united itſelf with them into a kind of magiſtery, which conſequently muſt contain ingredients or parts of ſeveral ſorts. For we ſee, that the ſtones, that are rich in vitriol, being often drenched with rain-water, the liquor will then extract a fine and tranſparent ſubſtance coagulable into vitriol; and yet though this vitriol be readily diſſoluble in water, it is not a true elementary ſalt, but, as you know, a body reſoluble into very differing parts, whereof one (as I ſhall have occaſion to tell you anon) is yet of a metalline, and conſequently not of an elementary nature. You may conſider alſo, that common ſulphur is readily diſſoluble

ſoluble in oil of turpentine, though notwithſtanding its name it abounds as well, if not as much, in ſalt as in true ſulphur; witneſs the great quantity of ſaline liquor it affords, being ſet to flame away under a glaſs bell. Nay, I have, which perhaps you will think ſtrange, with the ſame oil of turpentine alone eaſily enough diſſolved crude antimony finely powdered into a blood red balſam, wherewith perhaps conſiderable things may be performed in ſurgery. And if it were now requiſite, I could tell you of ſome other bodies (ſuch as perhaps you would not ſuſpect) that I have been able to work upon with certain chymical oils. But inſtead of digreſſing further, I ſhall make this uſe of the example I have named. That it is not unlikely, but that ſpirit of wine, which by its pungent taſte, and by ſome other qualities, that argue it better (eſpecially its reducibleneſs, according to *Helmont*, into alcali, and water) ſeems to be as well of a ſaline as of a ſulphureous nature, may well be ſuppoſed capable of diſſolving ſubſtances, that are not merely elementary ſulphurs, though perhaps they may abound with parts, that are of kin thereunto. For I find, that ſpirit of wine will diſſolve gum lacca, benzoin, and the reſinous parts of jallap, and even of guaiacum; whence we may well ſuſpect, that it may from ſpices, herbs, and other leſs compacted vegetables, extract ſubſtances, that are not perfect ſulphurs, but mixt bodies. And to put it paſt diſpute, there is many a vulgar extract drawn with ſpirit of wine, which committed to diſtillation will afford ſuch differing ſubſtances, as will loudly proclaim it to have been a very compounded body. So that we may juſtly ſuſpect, that even in mineral tinctures it will not always follow, that becauſe a red ſubſtance is drawn from the concrete by ſpirit of wine, that ſubſtance is its true and elementary ſulphur. And though ſome of theſe extracts may perhaps be inflammable; yet, beſides that others are not, and beſides that their being reduced to ſuch minuteneſs of parts may much facilitate their taking fire; beſides this, I ſay, we ſee, that common ſulphur, common oil, gum lac, and many unctuous and reſinous bodies, will flame well enough, though they be of very compounded natures: nay, travellers of unſuſpected credit aſſure us, as a known thing, that in ſome northern countries, where fir-trees and pines abound, the poorer ſort of inhabitants uſe long ſplinters of thoſe reſinous woods to burn inſtead of candles. And as for the redneſs wont to be met with in ſuch ſolutions, I could eaſily ſhew, that it is not neceſſary it ſhould proceed from the ſulphur of the concrete, diſſolved by the ſpirit of wine; if I had leiſure to manifeſt, how much chymiſts are wont to delude themſelves and others, by the ignorance of thoſe other cauſes, upon whoſe account ſpirit of wine and other menſtruums may acquire a red or ſome other high colour. But to return to our chymical oils, ſuppoſing that they were exactly pure; yet I hope they would be, as the beſt ſpirit of wine is, but the more inflammable and deflagrable. And therefore, ſince an oil can be by the fire alone immediately turned into flame, which is ſomething of a very differing nature from it; I ſhall demand, how this oil can be a primogeneal and incorruptible body, as moſt chymiſts would have their principles; ſince it is further reſoluble into flame, which, whether or no it be a portion of the element of fire, as an Ariſtotelian would conclude, is certainly ſomething of a very differing nature from a chymical oil, ſince it burns, and ſhines, and mounts ſwiftly upwards; none of which a chymical oil does, whilſt it continues ſuch. And if it ſhould be objected, that the diſſipated parts of this flaming oil may be caught and collected again into oil or ſulphur; I ſhall demand, what chymiſt appears to have ever done it: and without examining, whether it may not hence be as well ſaid, that ſulphur is but compacted fire, as that fire is but diffuſed ſulphur, I ſhall leave you to conſider, whether it may not hence be argued, that neither fire nor ſulphur are primitive and indeſtructible bodies. And I ſhall further obſerve, that at leaſt it will hence appear, that a portion of matter may, without being compounded with new ingredients, by having the texture and motion of its ſmall parts changed, be eaſily, by the means of the fire, endowed with new qualities, more differing from them it had before, than are thoſe, which ſuffice to diſcriminate the chymiſts principles from one another.

We are next to conſider, whether in the anatomy of mixt bodies, that, which chymiſts call the mercurial part of them, be uncompounded, or no. But to tell you true, though chymiſts do unanimouſly affirm, that their reſolutions diſcover a principle, which they call mercury, yet I find them to give of it deſcriptions ſo differing, and ſo ænigmatical, that I, who am not aſhamed to confeſs, that I cannot underſtand what is not ſenſe, muſt acknowledge to you, that I know not what to make of them. *Paracelſus* himſelf, and therefore, as you will eaſily believe, many of his followers, does ſomewhere call that mercury, which aſcends upon the burning of wood, as the Peripatetics are wont to take the ſame ſmoke for air; and ſo ſeems to define mercury by volatility, or (if I may coin ſuch a word) effumability. But ſince, in this example, both volatile ſalt and ſulphur make part of the ſmoke, which does indeed conſiſt alſo both of phlegmatic and terrene corpuſcles, this notion is not to be admitted; and I find, that the more ſober chymiſts themſelves diſavow it. Yet to ſhew you, how little of clearneſs we are to expect in the accounts even of later Spagyriſts, be pleaſed to take notice, that *Beguinus*, even in his *Tyrocinium Chymicum*, written for the inſtruction of novices, when he comes to tell us what are meant by the *tria prima*, which for their being principles ought to be defined the more accurately and plainly, gives us this deſcription of mercury; *Mercurius* (ſays he) *eſt liquor ille acidus, permeabilis, penetrabilis, æthereus, ac puriſſimus, à quo omnis nutricatio, ſenſus, motus, vires, colores, ſenectutiſque præproperæ retardatio.* Which words are not ſo much a definition of it, as an encomium: and yet *Quercetanus*, in his deſcription of the ſame principle, adds to theſe divers other epithets. But both of them, to ſkip very many other faults, that may be found with their metaphorical deſcriptions, ſpeak incongruouſly to the chymiſts own principles. For if mercury be an acid liquor, either hermetical philoſophy muſt err in aſcribing all taſtes to ſalt, or elſe mercury muſt not be a principle, but compounded of a ſaline ingredient and ſomewhat elſe. *Libavius*, though he finds great fault with the obſcurity of what the chymiſts write concerning their mercurial principle, does yet but give us ſuch a negative deſcription of it, as *Sennertus*, how favourable ſoever to the *tria prima*, is not ſatisfied with. And this *Sennertus* himſelf, though the learnedeſt champion for the hypoſtatical principles, does almoſt as frequently, as juſtly, complain of the unſatisfactorineſs of what the chymiſts teach concerning their mercury; and yet he himſelf (but with his wonted modeſty) ſubſtitutes inſtead of the deſcription of *Libavius*, another, which many readers, eſpecially if they be not Peripatetics, will not know what to make of. For ſcarce telling us any more, than that in all bodies, that, which is found beſides ſalt and ſulphur, and the elements, or, as they call them, phlegm and dead earth, is that ſpirit, which, in *Ariſtotle*'s language, may be called ȣ̓σία ἀνάλογος των ἄϛρων ϛοιχείῳ· he ſays that, which I confeſs is not at all ſatisfactory to me, who do not love to ſeem to acquieſce in any man's myſtical doctrines, that I may be thought to underſtand them.

Chym. Tyrocin. lib. 1. cap. 2.

If (ſays *Eleutherius*) I durſt preſume, that the ſame thing would be thought clear by me, and thoſe, that are fond of ſuch cloudy expreſſions, as you juſtly tax the chymiſts for, I ſhould venture to offer to conſideration, whether or no, ſince the mercurial principle, that ariſes from diſtillation, is unanimouſly aſſerted to be diſtinct from the ſalt and ſulphur of the ſame concrete, that may not be called the mercury of a body, which though it aſcend in diſtillation, as do the phlegm and ſulphur, is neither inſipid like the former, nor inflammable like the latter. And therefore I would ſubſtitute to the too much abuſed name of mercury, the more clear and familiar appellation of ſpirit, which is alſo now very much made uſe of, even by the chymiſts themſelves of our times, though they have not given us ſo diſtinct an explication, as were fit, of what may be called the ſpirit of a mixt body.

I SHOULD not perhaps (ſays *Carneades*) much quarrel with your notion of mercury. But as for the chymiſts, what they can mean, with congruity to their own principles, by the mercury of animals and vegetables, it will not be ſo eaſy to find out; for they aſcribe taſtes only to the ſaline principle, and conſequently would be much put to it to ſhew what liquor it is, in the reſolution of bodies, that not being inſipid, for that they call phlegm, neither is inflammable as oil or ſulphur, nor has any taſte; which, according to them, muſt proceed from a mixture, at leaſt, of ſalt. And if we ſhould take ſpirit in the ſenſe of the word received among modern chymiſts and phyſicians, for any diſtilled liquor, that is neither phlegm nor oil, the appellation would yet appear ambiguous enough. For plainly, that which firſt aſcends in the diſtillation of wine and fermented liquors, is generally as well by chymiſts as others reputed a ſpirit. And yet pure ſpirit of wine being wholly inflammable, ought, according to them, to be reckoned to the ſulphureous, not the mercureal principle. And among the other liquors, that go under the name of ſpirits, there are divers, which ſeem to belong to the family of ſalts; ſuch as are the ſpirit of nitre, vitriol, ſea-ſalt and others, and even the ſpirit of hartſhorn, being as I have tried, in great part, if not totally, reducible into ſalt and phlegm, may be ſuſpected to be but a volatile ſalt, diſguiſed by the phlegm mingled with it into the form of a liquor; however, if this be a ſpirit, it manifeſtly differs very much from that of vinegar, the taſte of the one being acid, and the other ſalt; and their mixture, in caſe they be very pure, ſometimes occaſioning an effervescence like that of thoſe liquors the chymiſts account moſt contrary to one another. And even among thoſe liquors, that ſeem to have a better title, than thoſe hitherto mentioned, to the name of ſpirits, there appears a ſenſible diverſity; for ſpirit of oak, for inſtance, differs from that of tartar, and this from that of box, or of guaiacum. And in ſhort, even theſe ſpirits, as well as other diſtilled liquors, manifeſt a great diſparity betwixt themſelves, either in their actions on our ſenſes, or in their other operations.

AND (continues *Carneades*) beſides this diſparity, that is to be met with among thoſe liquors, that the moderns call ſpirits, and take for ſimilar bodies, what I have formerly told you concerning the ſpirit of box-wood, may let you ſee, that ſome of thoſe liquors not only have qualities very differing from others, but may be further reſolved into ſubſtances differing from one another.

AND ſince many modern chymiſts and other naturaliſts are pleaſed to take the mercurial ſpirit of bodies for the ſame principle, under differing names, I muſt invite you to obſerve, with me, the great difference, that is conſpicuous betwixt all the vegetable and animal ſpirits I have mentioned, and running mercury. I ſpeak not of that, which is commonly ſold in ſhops, that many of themſelves will confeſs to be a mixt body; but of that, which is ſeparated from metals, which by ſome chymiſts, that ſeem more philoſophers than the reſt, and eſpecially by the above mentioned *Claveus*, is (for diſtinction ſake) called *mercurius corporum*. Now this metalline liquor being one of thoſe three principles, of which mineral bodies are, by Spagyriſts, affirmed to be compoſed, and to be reſoluble into them, the many notorious differences betwixt them and the mercuries, as they call them, of vegetables and animals, will allow me to infer, either that minerals and the other two ſorts of mixt bodies conſiſt not of the ſame elements, or that thoſe principles, whereunto minerals are immediately reſolved, which chymiſts with great oſtentation ſhew us as the true principles of them, are but ſecondary principles, or mixts of a peculiar ſort, which muſt be themſelves reduced to a very differing form, to be of the ſame kind with vegetable and animal liquors.

BUT this is not all; for though I formerly told you, how little credit there is to be given to the chymical proceſſes, commonly to be met with, of extracting the mercuries of metals, yet I will now add, that ſuppoſing, that the more judicious of them do not untruly affirm, that they have really drawn true and running mercury from ſeveral metals (which I wiſh they had clearly taught us how to do alſo) yet it may be ſtill doubted, whether ſuch extracted mercuries do not as well differ from common quickſilver, and from one another, as from the mercuries of vegetables and animals. *Claveus* *, in his apology, ſpeaking of ſome experiments, whereby metalline mercuries may be fixt into the nobler metals, adds, that he ſpake of the mercuries drawn from metals; becauſe common quickſilver, by reaſon of its exceſſive coldneſs and moiſture, is unfit for that particular kind of operation; for which, though a few lines before he preſcribes in general the mercuries of metalline bodies, yet he chiefly commends that drawn by art from ſilver. And elſewhere, in the ſame book, he tells us, that he himſelf tried, that by bare coction the quickſilver of tin or pewter *(argentum vivum ex ſtanno prolicitum)* may, by an efficient cauſe (as he ſpeaks) be turned into pure gold. And the experienced *Alexander van Suchten* ſomewhere tells us, that by a way he intimates may be made a mercury of copper, not of the ſilver colour of other mercuries, but green; to which I ſhall add, that an eminent perſon, whoſe name his travels and learned writings have made famous, lately aſſured me, that he had more than once ſeen the mercury of lead (which whatever authors promiſe, you will find it very difficult to make, at leaſt in any conſiderable quantity) fixt into perfect gold. And being by me demanded, whether or no any other mercury would not as well have been changed by the ſame operations, he aſſured me of the negative.

AND ſince I am fallen upon the mention of the mercuries of metals, you will, perhaps, expect *(Eleutherius)* that I ſhould ſay ſomething of their two other principles; but I muſt freely confeſs to you, that what diſparity there may be between the ſalts and ſulphurs of metals or other minerals, I am not myſelf experienced enough in the ſeparations and examens of them, to venture to determine: (for, as for the ſalts of metals, I formerly repreſented it as a thing much to be queſtioned, whether they have any at all.) And for the proceſſes of ſeparation I find in authors, if they were (what many of them are not) ſucceſsfully practicable, as I noted above, yet they are to be performed by the aſſiſtance of other bodies, ſo hardly, if upon any terms at all, ſeparable from them, that it is very difficult to give the ſeparated principles all their due, and no more. But the ſulphur of antimony, which is vehemently vomitive, and the ſtrongly ſcented anodyne ſulphur of vitriol, inclines me to think, that not only mineral ſulphurs differ from vegetable ones, but alſo from one another, retaining much of the nature of their concretes. The ſalts of metals, and of ſome ſort of minerals, you will eaſily gueſs (by the doubts I formerly expreſſed, whether metals have any ſalt at all) that I have not been ſo happy as yet to ſee, perhaps not for want of curioſity. But if *Paracelſus* did always write ſo conſentaneouſly to himſelf, that his opinion were confidently to be collected from every place of his writings, where he ſeems to expreſs it, I might ſafely take upon me to tell you, that he both countenances in general, what I have delivered in my fourth main conſideration, and in particular warrants me to ſuſpect, that there may be a difference in metalline and mineral ſalts, as well as we find it in thoſe of other bodies. For, *Sulphur* (ſays he) *aliud in auro, aliud in argento, aliud in ferro, aliud in plumbo, ſtanno, &c. ſic aliud in ſaphro, aliud in ſmaragdo, aliud in rubino, chryſolitho, amethyſto, magnete, &c. item aliud in lapidibus, ſilice, ſalibus, fontibus, &c. Nec vero tot ſulphura tantum, ſed & totidem ſalia; ſal aliud in metallis, aliud in gemmis, aliud in lapidibus, aliud in ſalibus, aliud in vitriolo, aliud in alumine: ſimilis etiam mercurii eſt ratio. Alius in metallis, alius in gemmis, &c. Ita ut unicuique ſpeciei ſuus peculiaris mercurius ſit. Et tamen res ſaltem tres ſunt; una eſſentia eſt ſulphur; una eſt ſal; una eſt mercurius. Addo quod & ſpecialius adhuc ſingula dividantur; aurum enim non unum, ſed multiplex, ut & non unum pyrum, pomum, ſed idem multiplex, totidem etiam ſulphura auri, ſalia auri, mercurii auri; idem competit etiam metallis & gemmis; ut quod ſaphyri præſtantiores, læviores, &c. tot etiam ſaphyrica ſulphura, ſaphyrica ſalia, ſaphyrici mercurii, &c. Idem verum etiam eſt de turconibus & gemmis aliis univerſis.* From which paſſage, *Eleutherius*, I ſuppoſe you will think I might without raſhneſs conclude, either that my opinion is favoured by that of *Paracelſus*, or that *Paracelſus* his opinion was not always the ſame. But becauſe in divers other places of his writings he ſeems to talk at a differing rate of the three principles and the four elements, I ſhall content myſelf to infer from the alleged paſſage, that if his doctrine be not conſiſtent with that part of mine, which it is brought to countenance, it is very difficult to know what his opinion concerning ſalt, ſulphur, and mercury, was; and that conſequently we had reaſon, about the beginning of our conferences, to decline taking upon us, either to examine or oppoſe it.

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* *Dixi autem de argento vivo à metallis prolicito, quod vulgare ob nimiam frigiditatem et humiditatem nimium concoctioni eſt contumax, nec ab auro ſolum alteratio coerceri poteſt.* Gaſt. Clave. in Apol.

Paracelſ. de Mineral. Tract. 1. Pag. 141.

I KNOW not whether I ſhould on this occaſion add, that thoſe very bodies, the chymiſts call phlegm and earth, do yet recede from an elementary ſimplicity. That common earth and water frequently do ſo, notwithſtanding the received contrary opinion, is not denied by the more wary of the modern Peripatetics themſelves: and certainly moſt earths are much leſs ſimple bodies than is commonly imagined even by chymiſts, who do not ſo conſiderately, to preſcribe and employ earths promiſcuouſly in thoſe diſtillations, that require the mixture of ſome *caput mortuum*, to hinder the flowing together of the matter, and to retain its groſſer parts. For I have found ſome earths to yield, by diſtillation, a liquor very far from being inodorous or inſipid; and it is a known obſervation, that moſt kinds of fat earth kept covered from the rain, and hindered from ſpending themſelves in the production of vegetables, will in time become impregnated with ſalt-petre.

BUT I muſt remember, that the water and earths I ought here to ſpeak of, are ſuch as are ſeparated from mixt bodies by the fire; and therefore to reſtrain my diſcourſe to ſuch, I ſhall tell you, that we ſee the phlegm of vitriol (for inſtance) is a very effectual remedy againſt burns; and I know a very famous and experienced phyſician, whoſe unſuſpected ſecret (himſelf confeſſed to me) it is, for the diſcuſſing of hard and obſtinate tumours. The phlegm of vinegar, though drawn exceeding leiſurely in a digeſting furnace, I have purpoſely made trial of; and ſometimes found it able to draw, though ſlowly, a ſaccarine ſweetneſs out of lead; and as I remember, by long digeſtion, I diſſolved corals in it. The phlegm of the ſugar of Saturn is ſaid to have very peculiar properties. Divers eminent chymiſts teach, that it will diſſolve pearls, which being precipitated by the ſpirit of the ſame concrete, are thereby (as they ſay) rendered volatile; which has been confirmed to me, upon his own obſervation, by a perſon of great veracity. The phlegm of wine, and indeed divers other liquors, that are indiſcriminately condemned to be caſt away as phlegm, are endowed with qualities, that make them differ both from meer water, and from each other. And whereas the chymiſts are pleaſed to call the *caput mortuum* of what they have diſtilled (after they have, by affuſion of water, drawn away its ſalt) *terra damnata*, or earth, it may be doubted, whether or no thoſe earths are all of them perfectly alike; and it is ſcarce to be doubted, but that there are ſome of them, which remain yet unreduced to an elementary nature. The aſhes of wood, deprived of all the ſalt, and bone-aſhes, or calcined hartſhorn, which refiners chooſe to make teſts of, as freeſt from ſalt, ſeem unlike: and he that ſhall compare either of theſe inſipid aſhes to lime, and much more to the calx of talc (though by the affuſion of water they be exquiſitely dulcified) will perhaps ſee cauſe to think them things of a ſomewhat different nature. And it is evident in colcothar, that the exacteſt calcination, followed by an exquiſite dulcification, does not always reduce the remaining body into elementary earth; for after the ſalt or vitriol (if the calcination have been too faint) is drawn out of the colcothar, the reſidue is not earth, but a mixt body, rich in medical virtues (as experience has informed me) and which *Angelus Sala* affirms to be partly reducible into malleable copper: which I judge very probable; for though, when I was making experiments upon colcothar, I was deſtitute of a furnace capable of giving a heat intenſe enough to bring ſuch a calx to fuſion; yet having conjectured, that if colcothar abounded with that metal, aqua fortis would find it out there, I put ſome dulcified colcothar into that menſtruum, and found the liquor, according to my expectation, preſently coloured as highly, as if it had been an ordinary ſolution of copper.

THE

SCEPTICAL CHYMIST:

PART V.

HERE *Carneades* making a pauſe, I muſt not deny (ſays his friend to him) that I think you have ſufficiently proved, that theſe diſtinct ſubſtances, which chymiſts are wont to obtain from mixt bodies, by their vulgar diſtillation, are not pure and ſimple enough to deſerve, in rigour of ſpeaking, the name of elements, or principles. But I ſuppoſe you have heard, that there are ſome modern Spagyriſts, who give out, that they can, by further and more ſkilful purifications, ſo reduce the ſeparated ingredients of mixt bodies to an elementary ſimplicity, that the oils (for inſtance) extracted from all mixts ſhall as perfectly reſemble one another, as the drops of water do.

IF you remember (replies *Carneades*) that at the beginning of our conference with *Philoponus*, I declared to him before the reſt of the company, that I would not engage myſelf at preſent to do any more, than examine the uſual proofs alledged by chymiſts, for the vulgar doctrine of their three hypoſtatical principles; you will eaſily perceive, that I am not obliged to make anſwer to what you newly propoſed; and that it rather grants, than diſproves what I have been contending for: ſince by pretending to make ſo great a change in the reputed principles, that diſtillation affords the common Spagyriſts, it is plainly enough pre-ſuppoſed, that before ſuch artificial depurations be made, the ſubſtances to be made more ſimple were not yet ſimple enough to be looked upon as elementary: wherefore in caſe the artiſts you ſpeak of could perform what they give out they can, yet I ſhould not need to be aſhamed of having queſtioned the vulgar opinion touching the *tria prima*. And as to the thing itſelf, I ſhall freely acknowledge to you, that I love not to be forward in determining things to be impoſſible, till I know and have conſidered the means, by which they are propoſed to be effected. And therefore I ſhall not peremptorily deny, either the poſſibility of what theſe artiſts promiſe, or my aſſent to any juſt inference, however deſtructive to my conjectures, that may be drawn from their performances. But give me leave to tell you withal, that becauſe ſuch promiſes are wont (as experience has more than once informed me) to be much more eaſily made, than made good by chymiſts, I muſt with-hold my belief from their aſſertions, till their experiments exact it; and muſt not be ſo eaſy as to expect before-hand an unlikely thing, upon ſtronger inducements than are yet given me: beſides that I have not yet found by what I have heard of theſe artiſts, that though they pretend to bring the ſeveral ſubſtances, into which the fire has divided the concrete, to an exquiſite ſimplicity, they pretend alſo to be able by the fire to divide all concretes, minerals, and others, into the ſame number of diſtinct ſubſtances. And in the mean time I muſt think it improbable, that they can either truly ſeparate as many differing bodies from gold (for inſtance) or oſtiocolla, as we can do from wine, or vitriol; or that the mercury (for example) of gold or Saturn would be perfectly of the ſame nature with that of hartſhorn; and that the ſulphur of antimony would be but numerically different from the diſtilled butter or oil of roſes.

BUT ſuppoſe (ſays *Eleutherius*) that you ſhould meet with chymiſts, who would allow you to take in earth and water into the number of the principles of mixt bodies; and being alſo content to change the ambiguous name of mercury for that more intelligible one of ſpirit, ſhould conſequently make the principles or compound bodies to be five; would you not think it ſomething hard to reject ſo plauſible an opinion, only becauſe the five ſubſtances, into which the fire divides mixt bodies, are not exactly pure, and homogeneous? For my part (continues *Eleutherius*) I cannot but think it ſomewhat ſtrange, in caſe this opinion be not true, that it ſhould fall out ſo luckily, that ſo great a variety of bodies ſhould be analyzed by the fire into juſt five diſtinct ſubſtances; which ſo little differing from the bodies, that bear thoſe names, may ſo plauſibly be called oil, ſpirit, ſalt, water, and earth.

THE opinion you now propoſe (anſwers *Carneades*) being another than that I was engaged to examine, it is not requiſite for me to debate at this preſent; nor ſhould I have leiſure to do it throughly. Wherefore, I ſhall only tell you in general, that though I think this opinion in ſome reſpects more defenſible than that of the vulgar chymiſts; yet you may eaſily enough learn from the paſt diſcourſe, what may be thought of it: ſince many of the objections made againſt the vulgar doctrine of the chymiſts ſeem, without much alteration, employable againſt this hypotheſis alſo. For, beſides that this doctrine does, as well as the other, take it for granted (what is not eaſy to be proved) that the fire is the true and adequate analyzer of bodies, and that all the diſtinct ſubſtances obtainable from a mixt body by the fire, were ſo pre-exiſtent in it, that they were but extricated from each other by the analyſis; beſides that this opinion too aſcribes to the productions of the fire an elementary ſimplicity, which I have ſhewn not to belong to them; and beſides that this doctrine is liable to ſome of the other difficulties, wherewith that of the *tria prima* is incumbered: beſides all this, I ſay, this quinary number of elements (if you pardon the expreſſion) ought at leaſt to have been reſtrained to the generality of animal and vegetable bodies, ſince not only among theſe there are ſome bodies (as I formerly argued) which, for aught yet has been made to appear, do conſiſt, either of fewer or more ſimilar ſubſtances, than preciſely five. But in the mineral kingdom, there is ſcarce one concrete, that has been evinced to be adequately diviſible into ſuch five principles or elements, and neither more nor leſs, as this opinion would have every mixt body to conſiſt of.

AND this very thing (continues *Carneades*) may ſerve to take away or leſſen your wonder, that juſt ſo many bodies as five ſhould be found upon the reſolution of concretes. For ſince we find not, that the fire can make any ſuch analyſis (into five elements) of metals and other mineral bodies, whoſe texture is more ſtrong and permanent, it remains, that the five ſubſtances under conſideration be obtained from vegetable and animal bodies, which (probably by reaſon of their looſer contexture) are capable of being diſtilled. And as to ſuch bodies, it is natural enough, that, whether we ſuppoſe, that there are, or are not, preciſely five elements, there ſhould ordinarily occur in the diſſipated parts, a five-fold diverſity of ſcheme (if I may ſo ſpeak.) For, if the parts do not remain all fixed, as in gold, calcined talc, *&c.* nor all aſcend, as in the ſublimation of brimſtone, camphire, *&c.* but after their diſſipation do aſſociate themſelves into new ſchemes of matter; it is very likely, that they will, by the fire, be divided into fixed and volatile; (I mean, in reference to that degree of heat, by which they are diſtilled) and thoſe volatile parts will, for the moſt part, aſcend either in a dry form, which chymiſts are pleaſed to call, if they be taſteleſs, flowers; if ſapid, volatile ſalt; or in a liquid form. And this liquor muſt be either inflammable, and ſo paſs for oil; or not inflammable, and yet ſubtile and pungent, which may be called ſpirit; or elſe ſtrengthleſs or inſipid, which may be named phlegm, or water. And as for the ſixth part, or *caput mortuum*, it will moſt commonly conſiſt of corpuſcles, partly ſoluble in water, or ſapid (eſpecially if the ſaline parts were not ſo volatile, as to fly away before) which make up its fixt ſalt; and partly inſoluble and inſipid, which therefore ſeems to challenge the name of earth. But although upon this ground one might eaſily enough have foretold, that the differing ſubſtances obtained from a perfectly mixt body by the fire would for the moſt part be reducible to the five newly-mentioned ſtates of matter; yet it will not preſently follow, that theſe five diſtinct ſubſtances were ſimple and primogeneal bodies, ſo pre-exiſtent in the concrete, that the fire does but take them aſunder. Beſides that it does not appear, that all mixt bodies (witneſs, gold, ſilver, mercury, *&c.*) nay, nor perhaps all vegetables, which may appear by what we ſaid above of camphire, benzoin, *&c.* are reſoluble by fire into juſt ſuch differing ſchemes of matter. Nor will the experiments, formerly alleged, permit us to look upon theſe ſeparated ſubſtances as elementary or uncompounded. Neither will it be a ſufficient argument of their being bodies, that deſerve the names, which chymiſts are pleaſed to give them, that they have an analogy in point of conſiſtence, or either volatility or fixedneſs,

neſs, or elſe ſome other obvious quality, with the ſuppoſed principles, whoſe names are aſcribed to them. For, as I told you above, notwithſtanding this reſemblance in ſome one quality, there may be ſuch a diſparity in others, as may be more fit to give them differing appellations, than the reſemblance is to give them one and the ſame. And indeed it ſeems but ſomewhat a groſs way of judging of the nature of bodies, to conclude without ſcruple, that thoſe muſt be of the ſame nature, that agree in ſome ſuch general quality, as fluidity, dryneſs, volatility, and the like: ſince each of thoſe qualities, or ſtates of matter, may comprehend a great variety of bodies, otherwiſe of a very differing nature; as we may ſee in the calxes of gold, of vitriol, and of Venetian talc, compared with common aſhes; which yet are very dry, and fixed by the vehemence of the fire, as well as they. And as we may likewiſe gather from what I have formerly obſerved, touching the ſpirit of box-wood, which though a volatile, ſapid, and not inflammable liquor, as well as the ſpirits of hartſhorn, of blood and others, (and therefore has been hitherto called the ſpirit, and eſteemed for one of the principles of the wood that affords it) may yet, as I told you, be ſubdivided into two liquors, differing from one another, and one of them at leaſt, from the generality of other chymical ſpirits.

BUT you may yourſelf, if you pleaſe, (purſues *Carneades)* accommodate to the hypotheſis you propoſed, what other particulars you ſhall think applicable to it in the foregoing diſcourſe. For I think it unſeaſonable for me to meddle now any further with a controverſy, which, ſince it does not now belong to me, leaves me at liberty to take my own time to declare myſelf about it.

ELEUTHERIUS perceiving, that *Carneades* was ſomewhat unwilling to ſpend any more time upon the debate of this opinion, and having perhaps ſome thoughts of taking hence a riſe to make him diſcourſe it more fully another time, thought not fit as then to make any further mention to him of the propoſed opinion; but told him,

I PRESUME I need not mind you, *Carneades*, that both the patrons of the ternary number of principles, and thoſe that would have five elements, endeavour to back their experiments with a ſpecious reaſon or two; and eſpecially ſome of thoſe embracers of the opinion laſt named (whom I have converſed with, and found them learned men) aſſign this reaſon of the neceſſity of five diſtinct elements; that otherwiſe mixt bodies could not be ſo compounded and tempered, as to obtain a due conſiſtence and competent duration. For ſalt (ſay they) is the baſis of ſolidity, and permanency in compound bodies, without which the other four elements might indeed be variouſly and looſely blended together, but would remain incompacted; but that ſalt might be diſſolved into minute parts, and conveyed to the other ſubſtances to be compacted by it, and with it, there is a neceſſity of water. And that the mixture may not be too hard and brittle, a ſulphureous or oily principle muſt intervene, to make the maſs more tenacious: to this a mercurial ſpirit muſt be ſurperadded; which by its activity may for a while permeate, and, as it were, leaven the whole maſs, and thereby promote the more exquiſite mixture and incorporation of the ingredients. To all which, laſtly, a portion of earth muſt be added, which by its dryneſs and poroſity may ſoak up part of that water, wherein the ſalt was diſſolved, and eminently concur with the other ingredients to give the whole body the requiſite conſiſtence.

I PERCEIVE (ſays *Carneades* ſmiling) that if it be true, as it was lately noted from the proverb, *That good wits have bad memories*, you have that title, as well as a better, to a place among the good wits. For you have already more than once forgot, that I declared to you, that I would at this conference examine only the experiments of my adverſaries, not their ſpeculative reaſons. Yet it is not (ſubjoins *Carneades)* for fear of meddling with the argument you have propoſed, that I decline the examining it at preſent. For if when we are more at leiſure, you ſhall have a mind, that we may ſolemnly conſider of it together; I am confident we ſhall ſcarce find it inſoluble. And in the mean time we may obſerve, that ſuch a way of arguing may, it ſeems, be ſpeciouſly accommodated to differing hypotheſes. For I find, that *Beguinus*, and other aſſertors of the *tria prima*, pretend to make out by ſuch a way the requiſiteneſs of their ſalt, ſulphur and mercury, to conſtitute mixt bodies, without taking notice of any neceſſity of an addition of water and earth.

AND indeed neither ſort of chymiſts ſeem to have duly conſidered, how great variety there is in the textures and conſiſtences of compound bodies; and how little the conſiſtence and duration of many of them ſeem to accommodate and be explicable by the propoſed notion. And not to mention thoſe almoſt incorruptible ſubſtances obtainable by the fire, which I have proved to be ſomewhat compounded, and which the chymiſts will readily grant not to be perfectly mixt bodies; (not to mention theſe, I ſay) if you will but recall to mind ſome of thoſe experiments, whereby I ſhewed you, that out of common water not only mixt bodies (but even living ones) of very differing conſiſtences, and reſoluble by fire into as many principles as other bodies acknowledged to be perfectly mixt, may be produced; if you do this, I ſay, you will not, I ſuppoſe, be averſe from believing, that nature, by a convenient diſpoſition of the minute parts of a portion of matter, may contrive bodies durable enough, and of this, or that, or the other conſiſtence, without being obliged to make uſe of all, much leſs of any determinate quantity of each of the five elements, or of the three principles to compound ſuch bodies of. And I have (purſues *Carneades)* ſomething wondered, chymiſts ſhould not conſider, that there is ſcarce any body in nature ſo permanent and indiſſoluble as glaſs; which yet themſelves teach us may be made of bare aſhes, brought to fuſion by the meer violence of the fire; ſo that, ſince aſhes are granted to conſiſt but of pure ſalt and ſimple earth, ſequeſtered from all the other principles or elements, they muſt acknowledge, that even art itſelf can of two elements only, or if you pleaſe, one principle and one element, compound a body more durable than almoſt any in the world. Which being undeniable, how will they prove, that nature cannot compound mixt bodies, and even durable ones, under all the five elements or material principles?

BUT to inſiſt any longer on this occaſional diſquiſition, touching their opinion, that would eſtabliſh five elements, were to remember as little as you did before, that the debate of this matter is no part of my firſt undertaking; and conſequently, that I have already ſpent time enough in what I look back upon but as a digreſſion, or at beſt an excurſion.

AND thus, *Eleutherius*, (ſays *Carneades)* having at length gone through the four conſiderations I propoſed to diſcourſe unto you, I hold it not unfit, for fear my having inſiſted ſo long on each of them may have made you forget their ſeries, briefly to repeat them by telling you, that

SINCE, in the firſt place, it may juſtly be doubted, whether or no the fire be, as chymiſts ſuppoſe it, the genuine and univerſal reſolver of mixt bodies;

SINCE we may doubt, in the next place, whether or no all the diſtinct ſubſtances, that may be obtained from a mixt body by the fire, were pre-exiſtent there, in the forms, in which they were ſeparated from it;

neſs, or elſe ſome other obvious quality, with the ſuppoſed principles, whoſe names are aſcribed to them. For, as I told you above, notwithſtanding this reſemblance in ſome one quality, there may be ſuch a diſparity in others, as may be more fit to give them differing appellations, than the reſemblance is to give them one and the ſame. And indeed it ſeems but ſomewhat a groſs way of judging of the nature of bodies, to conclude without ſcruple, that thoſe muſt be of the ſame nature, that agree in ſome ſuch general quality, as fluidity, dryneſs, volatility, and the like: ſince each of thoſe qualities, or ſtates of matter, may comprehend a great variety of bodies, otherwiſe of a very differing nature; as we may ſee in the calxes of gold, of vitriol, and of Venetian talc, compared with common aſhes; which yet are very dry, and fixed by the vehemence of the fire, as well as they. And as we may likewiſe gather from what I have formerly obſerved, touching the ſpirit of box-wood, which though a volatile, ſapid, and not inflammable liquor, as well as the ſpirits of hartſhorn, of blood and others, (and therefore has been hitherto called the ſpirit, and eſteemed for one of the principles of the wood that affords it) may yet, as I told you, be ſubdivided into two liquors, differing from one another, and one of them at leaſt, from the generality of other chymical ſpirits.

But you may yourſelf, if you pleaſe, (purſues *Carneades*) accommodate to the hypotheſis you propoſed, what other particulars you ſhall think applicable to it in the foregoing diſcourſe. For I think it unſeaſonable for me to meddle now any further with a controverſy, which, ſince it does not now belong to me, leaves me at liberty to take my own time to declare myſelf about it.

Eleutherius perceiving, that *Carneades* was ſomewhat unwilling to ſpend any more time upon the debate of this opinion, and having perhaps ſome thoughts of taking hence a riſe to make him diſcourſe it more fully another time, thought not fit as then to make any further mention to him of the propoſed opinion; but told him,

I presume I need not mind you, *Carneades*, that both the patrons of the ternary number of principles, and thoſe that would have five elements, endeavour to back their experiments with a ſpecious reaſon or two; and eſpecially ſome of thoſe embracers of the opinion laſt named (whom I have converſed with, and found them learned men) aſſign this reaſon of the neceſſity of five diſtinct elements; that otherwiſe mixt bodies could not be ſo compounded and tempered, as to obtain a due conſiſtence and competent duration. For ſalt (ſay they) is the baſis of ſolidity, and permanency in compound bodies, without which the other four elements might indeed be variouſly and looſely blended together, but would remain incompacted; but that ſalt might be diſſolved into minute parts, and conveyed to the other ſubſtances to be compacted by it, and with it, there is a neceſſity of water. And that the mixture may not be too hard and brittle, a ſulphureous or oily principle muſt intervene, to make the maſs more tenacious: to this a mercurial ſpirit muſt be ſurperadded; which by its activity may for a while permeate, and, as it were, leaven the whole maſs, and thereby promote the more exquiſite mixture and incorporation of the ingredients. To all which, laſtly, a portion of earth muſt be added, which by its dryneſs and poroſity may ſoak up part of that water, wherein the ſalt was diſſolved, and eminently concur with the other ingredients to give the whole body the requiſite conſiſtence.

I perceive (ſays *Carneades* ſmiling) that if it be true, as it was lately noted from the proverb, *That good wits have bad memories*, you have that title, as well as a better, to a place among the good wits. For you have already more than once forgot, that I declared to you, that I would at this conference examine only the experiments of my adverſaries, not their ſpeculative reaſons. Yet it is not (ſubjoins *Carneades*) for fear of meddling with the argument you have propoſed, that I decline the examining it at preſent. For if when we are more at leiſure, you ſhall have a mind, that we may ſolemnly conſider of it together; I am confident we ſhall ſcarce find it inſoluble. And in the mean time we may obſerve, that ſuch a way of arguing may, it ſeems, be ſpeciouſly accommodated to differing hypotheſes. For I find, that *Beguinus*, and other aſſertors of the *tria prima*, pretend to make out by ſuch a way the requiſiteneſs of their ſalt, ſulphur and mercury, to conſtitute mixt bodies, without taking notice of any neceſſity of an addition of water and earth.

And indeed neither ſort of chymiſts ſeem to have duly conſidered, how great variety there is in the textures and conſiſtences of compound bodies; and how little the conſiſtence and duration of many of them ſeem to accommodate and be explicable by the propoſed notion. And not to mention thoſe almoſt incorruptible ſubſtances obtainable by the fire, which I have proved to be ſomewhat compounded, and which the chymiſts will readily grant not to be perfectly mixt bodies; (not to mention theſe, I ſay) if you will but recall to mind ſome of thoſe experiments, whereby I ſhewed you, that out of common water not only mixt bodies (but even living ones) of very differing conſiſtences, and reſoluble by fire into as many principles as other bodies acknowledged to be perfectly mixt, may be produced; if you do this, I ſay, you will not, I ſuppoſe, be averſe from believing, that nature, by a convenient diſpoſition of the minute parts of a portion of matter, may contrive bodies durable enough, and of this, or that, or the other conſiſtence, without being obliged to make uſe of all, much leſs of any determinate quantity of each of the five elements, or of the three principles to compound ſuch bodies of. And I have (purſues *Carneades*) ſomething wondered, chymiſts ſhould not conſider, that there is ſcarce any body in nature ſo permanent and indiſſoluble as glaſs; which yet themſelves teach us may be made of bare aſhes, brought to fuſion by the meer violence of the fire; ſo that, ſince aſhes are granted to conſiſt but of pure ſalt and ſimple earth, ſequeſtered from all the other principles or elements, they muſt acknowledge, that even art itſelf can of two elements only, or if you pleaſe, one principle and one element, compound a body more durable than almoſt any in the world. Which being undeniable, how will they prove, that nature cannot compound mixt bodies, and even durable ones, under all the five elements or material principles?

But to inſiſt any longer on this occaſional diſquiſition, touching their opinion, that would eſtabliſh five elements, were to remember as little as you did before, that the debate of this matter is no part of my firſt undertaking; and conſequently, that I have already ſpent time enough in what I look back upon but as a digreſſion, or at beſt an excurſion.

And thus, *Eleutherius*, (ſays *Carneades*) having at length gone through the four conſiderations I propoſed to diſcourſe unto you, I hold it not unfit, for fear my having inſiſted ſo long on each of them may have made you forget their ſeries, briefly to repeat them by telling you, that

Since, in the firſt place, it may juſtly be doubted, whether or no the fire be, as chymiſts ſuppoſe it, the genuine and univerſal reſolver of mixt bodies;

Since we may doubt, in the next place, whether or no all the diſtinct ſubſtances, that may be obtained from a mixt body by the fire, were pre-exiſtent there, in the forms, in which they were ſeparated from it;

SINCE alſo, though you ſhould grant the ſubſtances ſeparable from mixt bodies by the fire, to have been their component ingredients, yet the number of ſuch ſubſtances does not appear the ſame in all mixt bodies; ſome of them being reſoluble into more differing ſubſtances than three, and others not being reſoluble into ſo many as three;

AND ſince, laſtly, thoſe very ſubſtances, that are thus ſeparated, are not, for the moſt part, pure and elementary bodies, but new kinds of mixts;

SINCE, I ſay, theſe things are ſo, I hope you will allow me to infer, that the vulgar experiments (I might perchance have added, the arguments too) wont to be alledged by chymiſts to prove, that their three hypoſtatical principles do adequately compoſe all mixt bodies, are not ſo demonſtrative, as to induce a wary perſon to acquieſce in their doctrine, which, till they explain and prove it better, will, by its perplexing darkneſs, be more apt to puzzle than ſatisfy conſidering men, and will to them appear incumbered with no ſmall difficulties.

AND from what has been hitherto deduced (continues *Carneades*) we may learn, what to judge of the common practice of thoſe chymiſts, who, becauſe they have found, that divers compound bodies (for it will not hold in all) can be reſolved into, or rather can be brought to afford two or three differing ſubſtances more than the ſoot and aſhes, whereunto the naked fire commonly divides them in our chimneys, cry up their own ſect for the invention of a new philoſophy; ſome of them, as *Helmont*, *&c.* ſtyling themſelves philoſophers by the fire; and the moſt part not only aſcribing, but, as far as in them lies, engroſſing to thoſe of their ſect the title of Philoſophers.

BUT alas, how narrow is this philoſophy, that reaches but to ſome of thoſe compound bodies, which we find but upon, or in the cruſt or outſide of our terreſtrial globe, which is itſelf but a point in compariſon of the vaſt extended univerſe, of whoſe other and greater parts the doctrine of the *tria prima* does not give us an account! For what does it teach us, either of the nature of the ſun, which aſtronomers affirm to be eight ſcore and odd times bigger than the whole earth? or of that of thoſe numerous fixt ſtars, which, for aught we know, would very few, if any of them, appear inferior in bulk and brightneſs to the ſun, if they were as near us as he? What does the knowing that ſalt, ſulphur and mercury, are the principles of mixt bodies, inform us of the nature of that vaſt, fluid, and ætherial ſubſtance, that ſeems to make up the interſtellar, and conſequently much the greateſt part of the world? For as for the opinion commonly aſcribed to *Paracelſus*, as if he would have not only the four Peripatetick elements, but even the celeſtial parts of the univerſe, to conſiſt of his three principles, ſince the modern chymiſts themſelves have not thought ſo groundleſs a conceit worth their owning, I ſhall not think it worth my confuting.

BUT I ſhould, perchance, forgive the hypotheſis I have been all this while examining, if, though it reaches but to a very little part of the world, it did at leaſt give us a ſatisfactory account of thoſe things, to which it is ſaid to reach. But I find not, that it gives us any other than a very imperfect information even about mixt bodies themſelves: for how will the knowledge of the *tria prima* diſcover to us the reaſon, why the load-ſtone draws a needle, and diſpoſes it to reſpect the poles, and yet ſeldom preciſely points at them? How will this hypotheſis teach us, how a chick is formed in the egg, or how the ſeminal principles of mint, pompions, and other vegetables, that I mentioned to you above, can faſhion water into various plants, each of them endowed with its peculiar and determinate ſhape, and with divers ſpecifick and diſcriminating qualities? How does this hypotheſis ſhew us, how much ſalt, how much ſulphur, and how much mercury muſt be taken to make a chick or a pompion? And if we know that, what principle is it, that manages theſe ingredients, and contrives (for inſtance) ſuch liquors, as the white and yolk of an egg into ſuch a variety of textures, as is requiſite to faſhion the bones, veins, arteries, nerves, tendons, feathers, blood, and other parts of a chick? and not only to faſhion each limb, but to connect them all together, after that manner, that is moſt congruous to the perfection of the animal, which is to conſiſt of them? For to ſay, that ſome more fine and ſubtile part of either, or all the hypoſtatical principles is the director in all this buſineſs, and the architect of all this elaborate ſtructure, is to give one occaſion to demand again, what proportion and way of mixture of the *tria prima* afforded this architectonick ſpirit, and what agent made ſo ſkilful and happy a mixture? And the anſwer to this queſtion, if the chymiſts will keep themſelves within their three principles, will be liable to the ſame inconvenience, that the anſwer to the former was. And if it were not to intrench upon the theme of a friend of ours here preſent, I could eaſily proſecute the imperfections of the vulgar chymiſts philoſophy, and ſhew you, that by going about to explicate by their three principles, I ſay not, all the abſtruſe properties of mixt bodies, but even ſuch obvious and more familiar phænomena as fluidity and firmneſs, the colours and figures of ſtones, minerals, and other compound bodies, the nutrition of either plants or animals, the gravity of gold or quickſilver compared with wine or ſpirit of wine; by attempting, I ſay, to render a reaſon of theſe (to omit a thouſand others as difficult to account for) from any proportion of the three ſimple ingredients, chymiſts will be much more likely to diſcredit themſelves and their hypotheſis, than ſatisfy an intelligent inquirer after truth.

BUT (interpoſes *Eleutherius*) this objection ſeems no more than may be made againſt the four Peripatetick elements; and indeed almoſt againſt any other hypotheſis, that pretends by any determinate number of material ingredients to render a reaſon of the phænomena of nature. And as for the uſe of the chymical doctrine of the three principles, I ſuppoſe you need not be told by me, that the great champion of it, the learned *Sennertus*, aſſigns this noble uſe of the *tria prima*, that from them, as the neareſt and moſt proper principles, may be deduced and demonſtrated the properties, which are in mixt bodies, and which cannot be proximately (as they ſpeak) deduced from the elements. And this, ſays he, is chiefly apparent, when we inquire into the properties and faculties of medicines. And I know (continues *Eleutherius*) that the perſon you have aſſumed, of an opponent of the hermetick doctrine, will not ſo far prevail againſt your native and wonted equity, as to keep you from acknowledging, that philoſophy is much beholden to the notions and diſcoveries of chymiſts.

Senn. *de Conſ. & Diſſen.* p. 165.

IF the chymiſts you ſpeak of (replies *Carneades*) had been ſo modeſt, or ſo diſcreet, as to propoſe their opinion of the *tria prima*, but as a notion uſeful among others, to increaſe human knowledge, they had deſerved more of our thanks, and leſs of our oppoſition: but ſince the thing, that they pretend, is not ſo much to contribute a notion toward the improvement of philoſophy, as to make this notion (attended by a few leſs conſiderable ones) paſs for a new philoſophy itſelf; nay, ſince they boaſt ſo much of this fancy of theirs, that the famous *Quercetanus* ſcruples not to write, that if his moſt certain doctrine of the three principles were ſufficiently learned, examined, and cultivated, it would eaſily diſpel all the darkneſs, that benights our minds, and bring in a clear light, that would remove all difficulties; this ſchool affording theorems and axioms irrefragable, and to be admitted without diſpute by impartial judges, and ſo uſeful withal, as to exempt us from the neceſſity of having recourſe, for want of the knowledge of cauſes, to that ſanctuary of the ignorant, occult qualities: ſince I ſay, this domeſtick notion of the chymiſts is ſo much overvalued by them, I cannot think it unfit, they ſhould be made ſenſible of their miſtake; and be admoniſhed to take in more fruitful and comprehenſive principles, if they mean to give us an account of the phænomena of nature; and not confine themſelves, and (as far as they can) others, to ſuch narrow principles, as I fear will ſcarce enable them to give an account (I mean an intelligible one) of the tenth part (I ſay not) of all the phænomena of nature; but even of all ſuch, as by the Leucippian, or ſome of the other ſorts of principles, may be plauſibly enough explicated. And though I be not unwilling to grant, that the incompetency I impute to the chymical hypotheſis is but the ſame, which may be objected againſt that of the four elements, and divers other doctrines that have been maintained by learned men; yet ſince it is the chymical hypotheſis only, which I am now examining, I ſee not why, if what I impute to it be a real inconvenience, either it ſhould ceaſe to be ſo, or I ſhould ſcruple to object it, becauſe other theories are liable thereunto, as well as the hermetical. For I know not, why a truth ſhould be thought leſs a truth for the being fit to overthrow variety of errors.

I am obliged to you (continues *Carneades*, a little ſmiling) for the favourable opinion you are pleaſed to expreſs of my equity, if there be no deſign in it. But I need not be tempted by an artifice, or invited by a compliment, to acknowledge the great ſervice, that the labours of chymiſts have done the lovers of uſeful learning; nor even, on this occaſion, ſhall their arrogance hinder my gratitude. But ſince we are as well examining the truth of their doctrine, as the merit of their induſtry, I muſt, in order to the inveſtigation of the firſt, continue a reply, to talk at the rate of the part I have aſſumed; and tell you, that when I acknowledge the uſefulneſs of the labours of Spagyriſts to natural philoſophy, I do it upon the ſcore of their experiments, not upon that of their ſpeculations; for it ſeems to me, that their writings, as their furnaces, afford as well ſmoke as light; and do little leſs obſcure ſome ſubjects, than they illuſtrate others. And though I am unwilling to deny, that it is difficult for a man to be an accompliſhed naturaliſt, that is a ſtranger to chymiſtry; yet I look upon the common operations and practices of chymiſts, almoſt as I do on the letters of the alphabet, without whoſe knowledge it is very hard for a man to become a philoſopher; and yet that knowledge is very far from being ſufficient to make him one.

But (ſays *Carneades*, reſuming a more ſerious look) to conſider a little more particularly what you alledge in favour of the chymical doctrine of the *tria prima*, though I ſhall readily acknowledge it not to be unuſeful, and that the deviſers and embracers of it have done the commonwealth of learning ſome ſervice, by helping to deſtroy that exceſſive eſteem, or rather veneration, wherewith the doctrine of the four elements was almoſt as generally, as undeſervedly entertained; yet what has been alledged concerning the uſefulneſs of the *tria prima*, ſeems to me liable to no contemptible difficulties.

And firſt, as for the very way of probation, which the more learned and more ſober champions of the chymical cauſe employed to evince the chymical principles in mixt bodies, it ſeems to me to be far enough from being convincing. This grand and leading argument, your *Sennertus* himſelf, who lays great weight upon it, and tells us, that the moſt learned philoſophers employ this way of reaſoning to prove the moſt important things, propoſes thus: *Ubicunque* (ſays he) *pluribus eædem affectiones & qualitates inſunt, per commune quoddam principium inſint neceſſe eſt, ſicut omnia ſunt gravia propter terram, calida propter ignem. At colores, odores, ſapores, eſſe* φλογιςὸν, *& ſimilia alia, mineralibus, metallis, gemmis, lapidibus, plantis, animalibus inſunt. Ergo per commune aliquod principium & ſubjectum inſunt. At tale principium non ſunt elementa. Nullam enim habent ad tales qualitates producendas potentiam. Ergo alia principia, unde fluant, inquirenda ſunt.*

IN the recital of this argument, (ſays *Carneades)* I therefore thought fit to retain the language, wherein the author propoſes it, that I might alſo retain the propriety of ſome Latin terms, to which I do not readily remember any that fully anſwer in Engliſh. But as for the argumentation itſelf, it is built upon a precarious ſuppoſition, that ſeems to me neither demonſtrable nor true; for how does it appear, that where the ſame quality is to be met with in many bodies, it muſt belong to them upon the account of ſome one body whereof they all partake? (for that the major of our author's argument is to be underſtood of the material ingredients of bodies, appears by the inſtances of earth and fire he annexes to explain it.) For to begin with that very example, which he is pleaſed to alledge for himſelf; how can he prove, that the gravity of all bodies proceeds from what they participate of the element of earth, ſince we ſee, that not only common water, but the more pure diſtilled rain-water is heavy; and quickſilver is much heavier than earth itſelf, though none of my adverſaries has yet proved, that it contains any of that element. And I the rather make uſe of this example of quickſilver, becauſe I ſee not how the aſſertors of the elements will give any better account of it than the chymiſts. For if it be demanded, how it comes to be fluid, they will anſwer, that it participates much of the nature of water. And indeed, according to them, water may be the predominant element in it, ſince we ſee, that ſeveral bodies, which by diſtillation afford liquors, that weigh more than their *caput mortuum*, do not yet conſiſt of liquor enough to be fluid. Yet if it be demanded, how quickſilver comes to be ſo heavy, then it is replied, that it is by reaſon of the earth, that abounds in it; but ſince, according to them, it muſt conſiſt alſo of air, and partly of fire, which they affirm to be light elements, how comes it, that it ſhould be ſo much heavier than earth of the ſame bulk, though to fill up the poroſities and other cavities it be made up into a maſs or paſte with water, which itſelf they allow to be a heavy element. But to return to our Spagyriſts, we ſee, that chymical oils and fixt ſalts, though never ſo exquiſitely purified and freed from terreſtrial parts, do yet remain ponderous enough. And experience has informed me, that a pound (for inſtance) of ſome of the heavieſt woods, as guaiacum, that will ſink in water, being burnt to aſhes will yield a much leſs weight of them (whereof I found but a ſmall part to be alkalizate) than much lighter vegetables: as alſo that the black charcoal of it will not ſink as did the wood, but ſwims; which argues, that the differing gravity of bodies proceeds chiefly from the particular texture, as is manifeſt in gold, the cloſeſt and compacteſt of bodies, which is many times heavier than we can poſſibly make any parcel of earth of the ſame bulk. I will not examine, what may be argued touching the gravity, or quality analogous thereunto, of even celeſtial bodies, from the motion of the ſpots about the ſun, and from the appearing equality of the ſuppoſed ſeas in the moon; nor conſider, how little thoſe phænomena would agree with what *Sennertus* preſumes concerning gravity. But further to invalidate his ſuppoſition, I ſhall demand, upon what chymical principle fluidity depends? And yet fluidity is, two or three perhaps excepted, the moſt diffuſed quality of the univerſe, and far more general than almoſt any other of thoſe, that are to be met with in any of the chymical principles, or Ariſtotelian elements; ſince not only the air, but that vaſt expanſion we call heaven, in compariſon of which, our terreſtrial globe (ſuppoſing it were all ſolid) is but a point, and perhaps too, the ſun and the fixt ſtars, are fluid bodies. I demand alſo, from which of the chymical principles, motion flows; which yet is an affection of matter much more general than any, that can be deduced from any of the three chymical principles? I might aſk the like queſtion concerning light, which is not only to be found in the kindled ſulphur of mixt bodies, but (not to mention thoſe ſorts of rotten woods, and rotten fiſh, that ſhine in the dark) in the tails of living glow-worms, and in the vaſt bodies of the ſun and ſtars. I would gladly alſo know, in which of the three principles, the quality, we call ſound, reſides as in its proper ſubject; ſince either oil falling upon oil, or ſpirit upon ſpirit, or ſalt upon ſalt, in a great quantity, and from a conſiderable height, will make a noiſe, or, if you pleaſe, create a ſound, and (that the objection may reach the Ariſtotelians) ſo will alſo water upon water, and earth upon earth. And I could name other qualities to be met with in divers bodies, of which I ſuppoſe my adverſaries will not in haſte aſſign any ſubject, upon whoſe account it muſt needs be, that the quality belongs to all the other ſeveral bodies.

AND, before I proceed any further, I muſt here invite you to compare the ſuppoſition we are examining, with ſome other of the chymical tenets. For firſt they do in effect teach, that more than one quality may belong to, and be deduced from, one principle. For, they aſcribe to ſalt, taſtes, and the power of coagulation; to ſulphur, as well odours as inflammableneſs; and ſome of them aſcribe to mercury, colours; as all of them do effumability, as they ſpeak. And on the other ſide, it is evident, that volatility belongs in common to all the three principles, and to water too. For it is manifeſt, that chymical oils are volatile; that alſo divers ſalts emerging upon the analyſis of many concretes, are very volatile, is plain from the fugitiveneſs of ſalt, of hartſhorn, fleſh, *&c.* aſcending in the diſtillation of thoſe bodies. How eaſily water may be made to aſcend in vapours, there is ſcarce any body, that has not obſerved. And as for what they call the mercurial principle of bodies, that is ſo apt to be raiſed in the form of ſteam, that *Paracelſus* and others define it by that aptneſs to fly up; ſo that (to draw that inference by the way) it ſeems not, that chymiſts have been accurate in their doctrine of qualities, and their reſpective principles, ſince they both derive ſeveral qualities from the ſame principle, and muſt aſcribe the ſame quality to almoſt all their principles and other bodies beſides. And thus much for the firſt thing taken for granted, without ſufficient proof, by your *Sennertus:* and to add that upon the by (continues *Carneades*) we may hence learn, what to judge of the way of argumentation, which that fierce champion of the Ariſtotelians againſt the chymiſts, *Anthoninus Guntherus Billichius* employs, where he pretends to prove againſt *Beguinus*, that not only the four elements do immediately concur to conſtitute every mixt body, and are both preſent in it, and obtainable from it, upon its diſſolution; but that in the *tria prima* themſelves, whereunto chymiſts are wont to reſolve mixt bodies, each of them clearly diſcovers itſelf to conſiſt of four elements. The ratiociation itſelf (purſues *Carneades*) being ſomewhat unuſual, I did the other day tranſcribe it, and (ſays he, pulling a paper out of his pocket) it is this: *Ordinamur, cum Beguino, à ligno viridi, quod ſi concremetur, videbis in ſudore aquam, in fumo aërem, in flamma & prunis ignem, terram in cineribus: quod ſi Beguino placuerit ex eo colligere humidum aquoſum, cohibere humidum oleaginoſum, extrahere ex cineribus ſalem;*

In Theſſalo redivivo, cap. 10. pag. 73, 74 *ego ipſi in unoquoque horum ſeorſim quatuor elementa ad oculum demonſtrabo, eodem artificio, quo in ligno viridi ea demonſtravi. Humorem aquoſum admovebo igni. Ipſe aquam ebullire videbit, in vapore aërem conſpiciet, ignem ſentiet in æſtu, plus minus terræ in ſedimento apparebit. Humor porro oleaginoſus aquam humiditate & fluiditate per ſe, accenſus vero ignem flamma prodit, fumo aërem, fuligine, nidore, & amurca terram. Salem denique ipſe Beguinus ſiccum vocat & terreſtrem, qui tamen nec fuſus aquam, nec cauſtica vi ignem celare poteſt; ignis vero violentia in halitus verſus nec ab aëre ſe alienum eſſe demonſtrat; idem de lacte, de ovis, de ſemine lini, de garyophillis, de nitro, de ſale marino, denique de antimonio, quod fuit de ligno viridi judicium eadem de illorum partibus, quas Beguinus adducit, ſententia, quæ de viridis ligni humore aquoſo, quæ de liquore ejuſdem oleoſo, quæ de ſale fuit.*

THIS bold diſcourſe (reſumes *Carneades*, putting up again his paper) I think it were not very difficult to confute, if his arguments were as conſiderable, as our time will probably prove ſhort for the remaining and more neceſſary part of my diſcourſe: wherefore referring you for an anſwer to what was ſaid concerning the diſſipated parts of a burnt piece of green wood, to what I told *Themiſtius* on the like occaſion, I might eaſily ſhew you, how ſlightly and ſuperficially our *Guntherus* talks of the dividing the flame of green wood into his four elements; when he makes that vapour to be air, which being caught in glaſſes and condenſed, preſently diſcovers itſelf to have been but an aggregate of innumerable very minute drops of liquor: and when he would prove the phlegm's being compoſed of fire, by that heat, which is adventitious to the liquors, and ceaſes upon the abſence of what produced it (whether that be an agitation proceeding from the motion of the external fire, or the preſence of a multitude of igneous atoms pervading the pores of the veſſel, and nimbly permeating the whole body of the water) I might, I ſay, urge theſe and divers other weakneſſes of his diſcourſe. But I will rather take notice of what is more pertinent to the occaſion of this digreſſion; namely, that taking it for granted, that fluidity (with which he unwarily ſeems to confound humidity) muſt proceed from the element of water, he makes a chymical oil to conſiſt of that elementary liquor; and yet in the very next words proves, that it conſiſts alſo of fire, by its inflammability; not remembering, that exquiſitely pure ſpirit of wine is both more fluid than water itſelf, and yet will flame all away, without leaving the leaſt aqueous moiſture behind it, and without ſuch an amurca and ſoot as he would deduce the preſence of earth from. So that the ſame liquor may, according to his doctrine, be concluded by its great fluidity to be almoſt all water; and, by its burning all away, to be all diſguiſed fire. And by the like way of probation our author would ſhew, that the fixt ſalt of wood is compounded of the four elements. For (ſays he) being turned by the violence of the fire into ſteams, it ſhews itſelf to be of kin to air; whereas I doubt, whether he ever ſaw a true fixt ſalt (which to become ſo, muſt have already endured the violence of an incinerating fire) brought by the fire alone to aſcend in the form of exhalations; but I do not doubt, that if he did, and had caught thoſe exhalations in convenient veſſels, he would have found them as well as the ſteams of common ſalt, &c. of a ſaline, and not an aëreal nature. And whereas our author takes it alſo for granted, that the fuſibility of ſalt muſt be deduced from water, it is indeed ſo much the effect of heat variouſly agitating the minute parts of a body, without regard to water, that gold (which by its being the heavieſt and fixedeſt of bodies, ſhould be the moſt earthy) will be brought to fuſion by a ſtrong fire, which ſure is more likely to drive away, than increaſe its aqueous ingredient, if it have any. And on the other ſide, for want of a ſufficient agitation of its minute parts, ice is not fluid, but ſolid; though he preſumes alſo, that the mordicant quality of bodies muſt proceed from a fiery ingredient: whereas, not to urge, that the light and inflammable parts, which are the moſt likely to belong to the element of fire, muſt probably be driven away by that time the violence of the fire has reduced the body to aſhes; not to urge this, I ſay, nor that oil of vitriol, which quenches fire, burns the tongue and fleſh of thoſe, that unwarily taſte or apply it, as a cauſtic doth, it is precarious to prove the preſence of fire in fixt ſalts from their cauſtic power, unleſs it were firſt ſhewn, that all the qualities aſcribed to ſalts muſt be deduced from thoſe of the elements; which, had I time, I could eaſily manifeſt to be no eaſy taſk. And not to mention, that our author makes a body, as homogeneous, as any he can produce for elementary, belong both to water and fire, though it be neither fluid nor inſipid, like water; nor light and volatile, like fire; he ſeems to omit in this anatomy the element of earth, ſave that he intimates, that the ſalt may paſs for that: but ſince a few lines before, he takes aſhes for earth, I ſee not how he will avoid an inconſiſtency either betwixt the parts of his diſcourſe, or betwixt ſome of them and his doctrine. For ſince there is a manifeſt difference betwixt the ſaline and the inſipid parts of aſhes, I ſee not how ſubſtances, that diſagree in ſuch notable qualities, can be both ſaid to be portions of an element, whoſe nature requires that it be homogeneous, eſpecially in this caſe where an analyſis by the fire is ſuppoſed to have ſeparated it from the admixture of other elements, which are confeſſed by moſt Ariſtotelians to be generally found in common earth, and to render it impure. And ſure, if when we have conſidered, for how little a diſparity's ſake the Peripatetics make theſe ſymbolizing bodies, air and fire, to be two diſtinct elements, we ſhall alſo conſider, that the ſaline part of aſhes is very ſtrongly taſted, and eaſily ſoluble in water; whereas the other part of the ſame aſhes is inſipid and indiſſoluble in the ſame liquor: not to add, that the one ſubſtance is opacous, and the other ſomewhat diaphanous, nor that they differ in divers other particulars; if we conſider thoſe things, I ſay, we ſhall hardly think, that both theſe ſubſtances are elementary earth. And as to what is ſometimes objected, that their ſaline taſte is only an effect of incineration and aduſtion, it has been elſewhere fully replied to, when propoſed by *Themiſtius*, and where it has been proved againſt him, that however inſipid earth may perhaps, by additaments, be turned into ſalt, yet it is not like it ſhould be ſo by the fire alone; for we ſee, that when we refine gold and ſilver, the violenteſt fires we can employ on them give them not the leaſt reliſh of ſaltneſs. And I think *Philoponus* has rightly obſerved, that the aſhes of ſome concretes contain very little ſalt, if any at all: for refiners ſuppoſe, that bone-aſhes are free from it, and therefore make uſe of them for teſts and cupels; which ought to be deſtitute of ſalt, leſt the violence of the fire ſhould bring them to vitrification. And having purpoſely and heedfully taſted a cupel made of only bone-aſhes and fair water, which I had cauſed to be expoſed to a very violent fire, actuated by the blaſt of a large pair of double bellows, I could not perceive, that the force of the fire had imparted to it the leaſt ſaltneſs, or ſo much as made it leſs inſipid.

But (ſays *Carneades*) ſince neither you nor I love repetitions, I ſhall not now make any of what elſe was urged againſt *Themiſtius*, but rather invite you to take notice with me, that when our author, though a learned man, and one that pretends ſkill enough in chymiſtry to reform the whole art, comes to make good his confident undertaking, to give us an ocular demonſtration of the immediate preſence of the four elements in the reſolution of green wood, he is fain to ſay things, that agree very little with one another. For about the beginning of that paſſage of his, lately recited

cited to you, he makes the ſweat, as he call it, of the green wood, to be water; the ſmoke, air; the ſhining matter, fire; and the aſhes, earth: whereas a few lines after, he will, in each of theſe, nay (as I juſt now noted) in one diſtinct part of the aſhes, ſhew the four elements. So that either the former analyſis muſt be incompetent to prove that number of elements, ſince by it, the burnt concrete is not reduced into elementary bodies, but into ſuch as are yet each of them compounded of the four elements; or elſe theſe qualities, from which he endeavours to deduce the preſence of all the elements in the fixt ſalt, and each of the other ſeparated ſubſtances, will be but a precarious way of probation; eſpecially if you conſider, that the extracted alkali of wood, being, for aught appears, at leaſt as ſimilar a body, as any that the Peripatetics can ſhew us, if its differing qualities muſt argue the preſence of diſtinct elements, it will ſcarce be poſſible for them, by any way they know of employing the fire upon any body, to ſhew, that any body is a portion of a true element. And this recalls to my mind, that I am now but in an occaſional excurſion, which aiming only to ſhew, that the Peripatetics, as well as the chymiſts, take, in our preſent controverſy, ſomething for granted, which they ought to prove, I ſhall return to my exceptions, where I ended the firſt of them, and further tell you, that neither is that the only precarious thing, that I take notice of in *Sennertus* his argumentation: for when he infers, that becauſe the qualities he mentions, as colours, ſmells, and the like, belong not to the elements, they therefore muſt to the chymical principles, he takes that for granted, which will not in haſte be proved; as I might here manifeſt, but that I may by and by have a fitter opportunity to take notice of it. And thus much at preſent may ſuffice to have diſcourſed againſt the ſuppoſition, that almoſt every quality muſt have ſome δεκτικὸν πρῶτον, as they ſpeak, ſome native receptacle, wherein, as in its proper ſubject of inheſion, it peculiarly reſides; and on whoſe account that quality belongs to the other bodies, wherein, it is to be met with. Now this fundamental ſuppoſition being once deſtroyed, whatſoever is built upon it, muſt fall to ruin of itſelf.

BUT I conſider further, that chymiſts are (for aught I have found) far from being able to explicate by any of the *tria prima*, thoſe qualities, which they pretend to belong primarily unto it, and in mixt bodies to deduce from it. It is true indeed, that ſuch qualities are not explicable by the four elements; but it will not therefore follow, that they are ſo by the three Hermetical principles: and this is it, that ſeems to have deceived the chymiſts, and is indeed a very common miſtake amongſt moſt diſputants, who argue, as if there could be but two opinions concerning the difficulty about which they content; and conſequently they infer, that if their adverſaries opinion be erroneous, their's muſt needs be the truth; whereas many queſtions, and eſpecially in matters phyſiological, may admit of ſo many differing hypotheſes, that it will be very inconſiderate and fallacious to conclude (except where the opinions are preciſely contradictory) the truth of one from the falſity of another. And in our particular caſe it is no way neceſſary, that the properties of mixt bodies muſt be explicable either by the Hermetical, or the Ariſtotelian hypotheſis; there being divers other and more plauſible ways of explaining them, and eſpecially that, which deduces qualities from the motion, figure, and contrivance of the ſmall parts of bodies; as I think might be ſhewn, if the attempt were as ſeaſonable, as I fear it would be tedious.

I WILL allow then, that the chymiſts do not cauſeleſsly accuſe the doctrine of the four elements of incompetency to explain the properties of compound bodies. And for this rejection of a vulgar error, they ought not to be denied what praiſe men may deſerve for exploding a doctrine, whoſe imperfections are ſo conſpicuous, that men needed but not to ſhut their eyes, to diſcover them. But I am miſtaken, if our hermetical philoſophers themſelves need not, as well as the Peripatetics, have recourſe to more fruitful and comprehenſive principles than the *tria prima*, to make out the properties of the bodies they converſe with. Not to accumulate examples to this purpoſe, becauſe I hope for a fitter opportunity to proſecute this ſubject, let us at preſent only point at colour, that you may gueſs by what they ſay of ſo obvious and familiar a quality, how little inſtruction we are to expect from the *tria prima* in thoſe more abſtruſe ones, which they, with the Ariſtotelians, ſtyle occult. For about colours, neither do they at all agree among themſelves, nor have I met with any one, of which of the three perſuaſions ſoever, that does intelligibly explicate them. The vulgar chymiſts are wont to aſcribe colours to mercury; *Paracelſus* in divers places attributes them to ſalt; and *Sennertus*, having recited their differing opinions, diſſents from both; and refers colours rather unto ſulphur. But how colours do, nay, how they may, ariſe from either of theſe principles, I think you will ſcarce ſay, that any has yet intelligibly explicated. And if Mr. *Boyle* will allow me to ſhew you the experiments, which he has collected about colours, you will, I doubt not, confeſs, that bodies exhibit colours, not upon the account of the predominancy of this or that principle in them, but upon that of their texture, and expecially the diſpoſition of their ſuperficial parts; whereby the light rebounding thence to the eye is ſo modified, as by differing impreſſions variouſly to affect the organs of ſight. I might here take notice of the pleaſing variety of colours exhibited by the triangular glaſs (at it is wont to be called) and demand, what addition or decrement of either ſalt, ſulphur, or mercury, befalls the body of the glaſs by being priſmatically figured; and yet it is known, that without that ſhape it would not afford thoſe colours as it does. But becauſe it may be objected, that theſe are not real, but apparent colours; that I may not loſe time in examining the diſtinction, I will alledge againſt the chymiſts a couple of examples of real and permanent colours drawn from metalline bodies; and repreſent, that without the addition of any extraneous body, , quickſilver may by the fire alone, and that in glaſs-veſſels, be deprived of its ſilver-like colour, and be turned into a red body; and from this red body without addition likewiſe may be obtained a mercury bright and ſpecular as it was before. So that I have here a laſting colour generated and deſtroyed (as I have ſeen) at pleaſure, without adding or taking away either mercury, ſalt, or ſulphur. And if you take a clean and ſlender piece of hardened ſteel, and apply to it the flame of a candle at ſome little diſtance ſhort of the point, you ſhall not have held the ſteel long in the flame, but you ſhall perceive divers colours, as yellow, red and blue, to appear upon the ſurface of the metal, and, as it were, run along in chaſe of one another towards the point; ſo that the ſame body, and that in one and the ſame part, may not only have a new colour produced in it, but exhibit ſucceſſively divers colours within a minute of an hour, or thereabouts; and any of theſe colours may, by removing the ſteel from the fire, become permanent, and laſt many years. And this production and variety of colours cannot reaſonably be ſuppoſed to proceed from the acceſſion of any of the three principles, to which of them ſoever chymiſts will be pleaſed to aſcribe colours; eſpecially conſidering, that if you but ſuddenly refrigerate that iron, firſt made red-hot, it will be hardened and colourleſs again; and not only by the flame of a candle, but by any other equivalent heat conveniently applied, the like colours will again be made to appear and ſucceed one another, as at the firſt. But I muſt not any further proſecute an occaſional diſcourſe, though that were not ſo difficult for me to do, as I

De Conſ. & Diſſen. cap. 11. p. 286.

fear it would be for the chymiſts to give a better account of the other qualities, by their principles, than they have done of colours. And your *Sennertus* himſelf (though an author I much value) would I fear have been exceedingly puzzled to reſolve, by the *tria prima*, half that catalogue of problems, which he challenges the vulgar Peripatetics to explicate by their four elements. And ſuppoſing it were true, that ſalt or ſulphur were the principle, to which this or that quality may be peculiarly referred, yet though he, that teaches us this, teaches us ſomething concerning that quality, yet he teaches us but ſomething. For indeed he does not teach us that, which can in any tolerable meaſure ſatisfy an inquiſitive ſearcher after truth. For what is it to me to know, that ſuch a quality reſides in ſuch a principle or element, whilſt I remain altogether ignorant of the cauſe of that quality, and the manner of its production and operation? How little do I know more than any ordinary man of gravity, if I know but that the heavineſs of mixt bodies proceeds from that of the earth they are compoſed of, if I know not the reaſon, why the earth is heavy? and how little does the chymiſt teach the philoſopher of the nature of purgation, if he only tells him, that the purgative virtue of medicines reſides in their ſalt? For, beſides that this muſt not be conceded without limitation, ſince the purging parts of many vegetables extracted by the water, wherein they are infuſed, are at moſt but ſuch compounded ſalts (I mean mingled with oil, and ſpirit, and earth, as tartar and divers other ſubjects of the vegetable kingdom afford;) and ſince too that quickſilver precipitated either with gold, or without addition, into a powder, is wont to be ſtrongly enough cathartical, though the chymiſts have not yet proved, that either gold or mercury have any ſalt at all, much leſs any that is purgative; beſides this, I ſay, how little is it to me, to know, that it is the ſalt of the rhubarb (for inſtance) that purges, if I find, that it does not purge as ſalt; ſince ſcarce any elementary ſalt is in ſmall quantity cathartical; and if I know not how purgation in general is effected in a human body? In a word, as it is one thing to know a man's lodging, and another, to be acquainted with him; ſo it may be one thing to know the ſubject, wherein a quality principally reſides, and another thing to have a right notion and knowledge of the quality itſelf. Now that, which I take to be the reaſon of this chymical deficiency, is the ſame, upon whoſe account I think the Ariſtotelian and divers other theories incompetent to explicate the origin of qualities. For I am apt to think, that men will never be able to explain the phænomena of nature, while they endeavour to deduce them only from the preſence and proportion of ſuch or ſuch material ingredients, and conſider ſuch ingredients or elements as bodies in a ſtate of reſt; whereas indeed the greateſt part of the affections of matter, and conſequently of the phænomena of nature, ſeems to depend upon the motion and the contrivance of the ſmall parts of bodies. For it is by motion, that one part of matter acts upon another; and it is, for the moſt part, the texture of the body, upon which the moving parts ſtrike, that modifies the motion or impreſſion, and concurs with it to the production of thoſe effects, which make up the chief part of the naturaliſt's theme.

Senn. de Conſ. & Diſſen. p. 165, 166.

But (ſays *Eleutherius*) methinks for all this, you have left ſome part of what I alleged, in behalf of the three principles, unanſwered. For all that you have ſaid will not keep this from being a uſeful diſcovery, that ſince in the ſalt of one concrete, in the ſulphur of another, and the mercury of a third, the medicinal virtue of it reſides; that principle ought to be ſeparated from the reſt, and there the deſired faculty muſt be ſought for.

I never denied (replies *Carneades*) that the notion of the *tria prima* may be of ſome uſe; but (continues he, laughing) by what you now allege for it, it will but appear, that it is uſeful to apothecaries, rather than to philoſophers; the being able to make things operative being ſufficient to thoſe, whereas the knowledge of cauſes is the thing looked after by theſe. And let me tell you, *Eleutherius*, even this itſelf will need to be entertained with ſome caution.

For firſt, it will not preſently follow, that if the purgative or other virtue of a ſimple may be eaſily extracted by water or ſpirit of wine, it reſides in the ſalt or ſulphur of the concrete; ſince unleſs the body hath before been reſolved by the fire, or ſome other powerful agent, it will, for the moſt part, afford in the liquors I have named, rather the finer compounded parts of itſelf, than the elementary ones. As I noted before, that water will diſſolve not only pure ſalts, but cryſtals of tartar, gum arabic, myrrh and other compound bodies. As alſo ſpirit of wine will diſſolve not only the pure ſulphur of concretes, but likewiſe the whole ſubſtance of divers reſinous bodies, as benzoin, the gummous parts of jalap, gum lacca, and other bodies, that are counted perfectly mixt. And we ſee, that the extracts made either with water or ſpirit of wine are not of a ſimple and elementary nature, but maſſes conſiſting of the looſer corpuſcles, and finer parts of the concretes whence they are drawn; ſince by diſtillation they may be divided into more elementary ſubſtances.

Next, we may conſider, that even when there intervenes a chymical reſolution by the fire, it is ſeldom in the ſaline or ſulphureous principle, as ſuch, that the deſired faculty of the concrete reſides; but as that titular ſalt or ſulphur is yet a mixt body, though the ſaline or ſulphureous nature be predominent in it. For, if in chymical reſolutions, the ſeparated ſubſtances were pure and ſimple bodies, and of a perfect elementary nature; no one would be indued with more ſpecific virtues, than another; and their qualities would differ as little as do thoſe of water. And let me add this upon the by, that even eminent chymiſts have ſuffered themſelves to be reprehended by me for their over-great diligence in purifying ſome of the things they obtain by fire from mixt bodies. For though ſuch compleatly purified ingredients of bodies might perhaps be more ſatisfactory to our underſtanding, yet others are often more uſeful to our lives; the efficacy of ſuch chymical productions depending moſt upon what they retain of the bodies, whence they are ſeparated, or gain by the new aſſociations of the diſſipated among themſelves; whereas, if they were meerly elementary, their uſes would be comparatively very ſmall; and the virtues of ſulphurs, ſalts, or other ſuch ſubſtances of one denomination, would be the very ſame.

And by the way, *Eleutherius*, I am inclined upon this ground to think, that the artificial reſolution of compound bodies by fire does not ſo much enrich mankind, as it divides them into their ſuppoſed principles; as upon the ſcore of its making new compounds by new combinations of the diſſipated parts of the reſolved body. For by this means the number of mixt bodies is conſiderably increaſed; and many of thoſe new productions are endowed with uſeful qualities; divers of which they owe not to the body, from which they were obtained, but to their newly-acquired texture.

But thirdly, that, which is principally to be noted, is this, that as there are divers concretes, whoſe faculties reſide in ſome one or other of thoſe differing ſubſtances, that chymiſts call their ſulphurs, ſalts, and mercuries; and conſequently may be beſt obtained, by analyzing the concrete, whereby the deſired principles may be had ſevered or freed from the reſt; ſo there are others, wherein the nobleſt properties lodge not in the ſalt, or ſulphur, or mercury, but depend immediately upon the form, or (if you will) reſult from the determinate ſtructure of the whole concrete; and conſequently they, that go about to extract the virtues of ſuch bodies, by expoſing them to the violence of the fire, do exceedingly miſtake, and take the way to deſtroy what they would obtain.

I remember that *Helmont* himſelf ſomewhere confeſſes, that as the fire betters ſome things, and improves their virtues, ſo it ſpoils others, and makes them degenerate. And elſewhere he judiciouſly affirms, that there may be ſometimes greater virtue in a ſimple, ſuch as nature has made it, than in any thing, that can by the fire be ſeparated from it. And leſt you ſhould doubt, whether he means by the virtues of things thoſe, that are medical; he has in one place this ingenuous confeſſion; *Credo* (ſays he) *ſimplicia in ſua ſimplicitate eſſe ſufficientia pro ſanatione omnium morborum.* Nay, *Barthius*, even in a comment upon *Beguinus*, ſcruples not to make this acknowledgment; *Valde abſurdum eſt* (ſays he) *ex omnibus rebus extracta facere, ſalia, quintas eſſentias; præſertim ex ſubſtantiis per ſe plane vel ſubtilibus vel homogeneis, quales ſunt uniones, corallia, moſcus, ambra, &c.* Conſonantly whereunto he alſo tells us (and vouches the famous *Platerus*, for having candidly given the ſame advertiſement to his auditors) that ſome things have greater virtues, and better ſuited to our human nature, when unprepared, than when they have paſt the chymiſts fire: as we ſee, ſays my author, in pepper; of which ſome grains ſwallowed perform more towards the relief of a diſtempered ſtomach, than a great quantity of the oil of the ſame ſpice.

Helmont. *Pharm. & Diſpen. Nov.* pag. 498. *Vide* Jer. *ad* Begu. Lib. 1. Cap. 17.

It has been (purſues *Carneades*) by our friend here preſent obſerved concerning ſalt-petre, that none of the ſubſtances, into which the fire is wont to divide it, retains either the taſte, the cooling virtue, or ſome other of the properties of the concrete; and that each of thoſe ſubſtances acquires new qualities not to be found in ſalt-petre itſelf. The ſhining property of the tails of glow-worms does ſurvive but ſo ſhort a time the little animal made conſpicuous by it, that inquiſitive men have not ſcrupled publickly to deride *Baptiſta Porta* and others; who, deluded perhaps with ſome chymical ſurmiſes, have ventured to preſcribe the diſtillation of a water from the tails of glow-worms, as a ſure way to obtain a liquor ſhining in the dark. To which I ſhall now add no other example than that afforded us by amber; which, whilſt it remains an intire body, is endowed with an electrical faculty of drawing to itſelf feathers, ſtraws, and ſuch like bodies; which I never could obſerve either in its ſalt, its ſpirit, its oil, or in the body I remember I once made by the re-union of its divided elements; none of theſe having ſuch a texture as the intire concrete. And however chymiſts boldly deduce ſuch and ſuch properties from this or that proportion of their component principles; yet in concretes, that abound with this or that ingredient, it is not always ſo much by virtue of its preſence, nor its plenty, that the concrete is qualified to perform ſuch and ſuch effects; as upon the account of the particular texture of that and the other ingredients, aſſociated after a determinate manner into one concrete; though poſſibly ſuch a proportion of that ingredient may be more convenient than another, for the conſtituting of ſuch a body. Thus in a clock, the hand is moved upon the dial, the bell is ſtruck, and the other actions belonging to the engine are performed, not becauſe the wheels are of braſs or iron, or part of one metal and part of another, or becauſe the weights are of lead; but by virtue of the ſize, ſhape, bigneſs, and coaptation of the ſeveral parts; which would perform the ſame things, though the wheels were of ſilver, or lead, or wood, and the weights of ſtone or clay, provided the fabric or contrivance of the engine were the ſame: though it be not to be denied, that braſs and ſteel are more convenient materials to make clock-wheels of, than lead, or wood. And to let you ſee, *Eleutherius*, that it is ſometimes at leaſt, upon the texture of the ſmall parts of a body, and not always upon the preſence, or receſs, or increaſe, or decrement of any one of its principles, that it may loſe ſome ſuch qualities, and acquire ſome ſuch others, as are thought very ſtrongly inherent to the bodies they reſide in; I will add to what may, from my paſt diſcourſe, be referred to this purpoſe, this notable example, from my own experience; that lead may, without any additament, and only by various applications of the fire, loſe its colour; and acquire ſometimes a grey, ſometimes a yellowiſh, ſometimes a red, ſometimes an amethyſtine colour; and after having paſt through theſe, and perhaps divers others, again recover its leaden colour, and be made a bright body. That alſo this lead, which is ſo flexible a metal, may be made as brittle as glaſs, and preſently be brought to be again flexible and malleable as before. And beſides that the ſame lead, which I find by microſcopes to be one of the moſt opacous bodies in the world, may be reduced to a fine tranſparent glaſs; whence yet it may return to an opacous nature again; and all this, as I ſaid, without the addition of any extraneous body, and meerly by the manner and method of expoſing it to the fire.

But (ſays *Carneades*) after having already put you to ſo prolix a trouble, it is time for me to relieve you with a promiſe of putting ſpeedily a period to it; and to make good that promiſe, I ſhall, from all that I have hitherto diſcourſed with you, deduce but this one propoſition by way of corollary: *That it may as yet be doubted, whether or no there be any determinate number of elements; or, if you pleaſe, whether or no all compound bodies do conſiſt of the ſame number of elementary ingredients or material principles.*

This being but an inference from the foregoing diſcourſe, it will not be requiſite to inſiſt at large on the proofs of it; but only to point at the chief of them, and refer you for particulars to what has been already delivered.

In the firſt place then, from what has been ſo largely diſcourſed, it may appear, that the experiments wont to be brought, whether by the common Peripatetics, or by the vulgar chymiſts, to demonſtrate, that all mixt bodies are made up preciſely either of the four elements, or the three hypoſtatical principles, do not evince what they are alleged to prove. And as for the other common arguments, pretended to be drawn from reaſon in favour of the Ariſtotelian hypotheſis (for the chymiſts are wont to rely almoſt altogether upon experiments;) they are commonly grounded upon ſuch unreaſonable or precarious ſuppoſitions, that it is altogether as eaſy and as juſt for any man to reject them, as for thoſe, that take them for granted, to aſſert them, being indeed all of them as indemonſtrable as the concluſion to be inferred from them; and ſome of them ſo manifeſtly weak and proofleſs, that he muſt be a very courteous adverſary, that can be willing to grant them; and as unſkilful a one, that can be compelled to do ſo.

In the next place, it may be conſidered, if what thoſe patriarchs of the Spagyriſts, *Paracelſus* and *Helmont*, do on divers occaſions poſitively deliver, be true; namely, that the alkaheſt does reſolve all mixt bodies into other principles than the fire, it muſt be decided, which of the two reſolutions (that made by the alkaheſt, or that made by the fire) ſhall determine the number of the elements, before we can be certain how many there are.

And in the mean time, we may take notice in the laſt place, that as the diſtinct ſubſtances, whereinto the alkaheſt divides bodies, are affirmed to be differing in nature

ture from thoſe, whereunto they are wont to be reduced by fire, and * to be obtained from ſome bodies more in number than from ſome others; ſince he tells us, he could totally reduce all ſorts of ſtones into ſalt only, whereas of a coal he had two diſtinct liquors: ſo, although we ſhould acquieſce in that reſolution which is made by fire, we find not, that all mixt bodies are thereby divided into the ſame number of elements and principles; ſome concretes affording more of them than others do; nay, and ſometimes this or that body affording a greater number of differing ſubſtances by one way of management, than the ſame yields by another. And they, that out of gold, or mercury, or *Muſcovy*-glaſs, will draw me as many diſtinct ſubſtances, as I can ſeparate from vitriol, or from the juice of grapes variouſly ordered, may teach me that, which I ſhall very thankfully learn. Nor does it appear more congruous to that variety, that ſo much conduceth to the perfection of the univerſe, that all elemented bodies be compounded of the ſame number of elements, than it would be for a language, that all its words ſhould conſiſt of the ſame number of letters.

THE
SCEPTICAL CHYMIST:
OR,
A Paradoxical Appendix to the foregoing Treatiſe.

PART VI.

HERE *Carneades* having diſpatched what he thought requiſite to oppoſe againſt what the chymiſts are wont to allege for proof of their three principles, pauſed a while, and looked about him to diſcover, whether it were time for him and his friend to rejoin the reſt of the company. But *Eleutherius* perceiving nothing yet to forbid them to proſecute their diſcourſe a little further, ſaid to his friend (who had likewiſe taken notice of the ſame thing) I half expected, *Carneades*, that after you had ſo freely declared your doubting, whether there be any determinate number of elements, you would have proceeded to queſtion, whether there be any elements at all. And I confeſs it will be a trouble to me, if you defeat me of my expectation; eſpecially ſince you ſee the leiſure we have allowed us may probably ſuffice to examine that paradox; becauſe you have ſo largely deduced already many things pertinent to it, that you need but intimate how you would have them applied, and what you would infer from them.

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* *Novi ſaxum & lapides omnes in merum ſalem ſuo ſaxo aut lapidi & æquiponderantem reducere abſque omni prorſus ſulphure aut mercurio.* Helmont, pag. 490.

CARNEADES having in vain repreſented, that their leiſure could be but very ſhort, that he had already prated very long, that he was unprepared to maintain ſo great and ſo invidious a paradox, was at length prevailed with to tell his friend; ſince, *Eleutherius*, you will have me diſcourſe *extempore* of the paradox you mention, I am content (though more perhaps to expreſs my obedience, than my opinion) to tell you, that (ſuppoſing the truth of *Helmont*'s and *Paracelſus*'s alkaheſtical experiments, if I may ſo call them) though it may ſeem extravagant, yet it is not abſurd to doubt, whether, for aught has been proved, there be a neceſſity to admit any elements, or hypoſtatical principles, at all.

AND, as formerly, ſo now, to avoid the needleſs trouble of diſputing ſeverally with the Ariſtotelians and the chymiſts, I will addreſs myſelf to oppoſe them I have laſt named, becauſe their doctrine about the elements is more applauded by the moderns, as pretending highly to be grounded upon experience. And, to deal not only fairly, but favourably with them, I will allow them to take in earth and water to their other principles. Which I conſent to the rather, that my diſcourſe may the better reach the tenets of the Peripatetics; who cannot plead for any ſo probably, as for thoſe two elements; that of fire above the air being generally by judicious men exploded as an imaginary thing; and the air not concurring to compoſe mixt bodies as one of their elements, but only lodging in their pores, or rather repleniſhing, by reaſon of its weight and fluidity, all thoſe cavities of bodies here below, whether compounded or not, that are big enough to admit it, and are not filled up with any groſſer ſubſtance.

AND, to prevent miſtakes, I muſt advertiſe you, that I now mean by elements, as thoſe chymiſts, that ſpeak plaineſt, do by their principles, certain primitive and ſimple, or perfectly unmingled bodies; which not being made of any other bodies, or of one another, are the ingredients, of which all thoſe called perfectly mixt bodies are immediately compounded, and into which they are ultimately reſolved: now whether there be any one ſuch body to be conſtantly met with in all, and each, of thoſe, that are ſaid to be elemented bodies, is the thing I now queſtion.

BY this ſtate of the controverſy you will, I ſuppoſe, gueſs, that I need not be ſo abſurd, as to deny, that there are ſuch bodies as earth and water, and quickſilver and ſulphur: but I look upon earth and water, as component parts of the univerſe, or rather of the terreſtrial globe, not of all mixt bodies. And though I will not peremptorily deny, that there may ſometimes either a running mercury, or a combuſtible ſubſtance be obtained from a mineral, or even a metal; yet I need not concede either of them to be an element in the ſenſe above declared; as I ſhall have occaſion to ſhew you by and by.

To give you then a brief account of the grounds I intend to proceed upon, I muſt tell you, that in matters of philoſophy, this ſeems to me a ſufficient reaſon to doubt of a known and important propoſition, that the truth of it is not yet by any competent proof made to appear. And congruouſly hereunto, if I ſhew, that the grounds, upon which men are perſuaded, that there are elements, are unable to ſatisfy a conſidering man, I ſuppoſe my doubts will appear rational.

Now the conſiderations, that induce men to think, that there are elements, may be conveniently enough referred to two heads: namely, the one, that it is neceſſary, that nature make uſe of elements to conſtitute the bodies that are reputed mixt. And the other, that the reſolution of ſuch bodies manifeſts, that nature had compounded them of elementary ones.

In reference to the former of theſe conſiderations, there are two or three things, that I have to repreſent. And I will begin with reminding you of the experiments I not long ſince related to you concerning the growth of pompions, mint, and other vegetables out of fair water. For by thoſe experiments it ſeems evident, that water may be tranſmuted into all the other elements; from whence it may be inferred, both, that it is not every thing chymiſts will call ſalt, ſulphur, or ſpirit, that needs always be a primordiate and ingenerable body; and that nature may contex a plant (though that be a perfectly mixt concrete) without having all the elements previouſly preſented to her to compound it of. And, if you will allow the relation I mentioned out of Monſieur *de Rochas* to be true; then may not only plants, but animals and minerals too, be produced out of water. And however there is little doubt to be made, but that the plants my trials afforded me, as they were like in ſo many other reſpects to the reſt of the plants of the ſame denomination; ſo they would, in caſe I had reduced them to putrefaction, have likewiſe produced worms or other inſects, as well as the reſembling vegetables are wont to do: ſo that water may, by various ſeminal principles, be ſucceſſively tranſmuted into both plants and animals. And if we conſider, that not only men, but even ſucking children are, but too often, tormented with ſolid ſtones; and that divers ſorts of beaſts themſelves (whatever *Helmont* againſt experience thinks to the contrary) may be troubled with great and heavy ſtones in their kidneys and bladders, though they feed but upon graſs and other vegetables, that are perhaps but diſguiſed water; it will not ſeem improbable, that even ſome concretes of a mineral nature may likewiſe be formed of water.

We may further take notice, that as a plant may be nouriſhed, and conſequently may conſiſt of common water; ſo may both plants and animals (perhaps even from their ſeminal rudiments) conſiſt of compound bodies, without having any thing merely elementary brought them by nature to be compounded by them: this is evident in divers men, who whilſt they were infants were fed only with milk, afterwards live together upon fleſh, fiſh, wine, and other perfectly mixt bodies. It may be ſeen alſo in ſheep, who, on ſome of our Engliſh downs or plains, grow very fat by feeding upon the graſs, without ſcarce drinking at all; and yet more manifeſtly in the maggots, that breed and grow up to their full bigneſs within the pulps of apples, pears, or the like fruit. We ſee alſo, that dungs, that abound with a mixt ſalt, give a much more ſpeedy increment to corn and other vegetables, than water alone would do: and it hath been aſſured me by a man experienced in ſuch matters, that ſometimes, when, to bring up roots very early, the mould they were planted in was made over-rich, the very ſubſtance of the plant has taſted of the dung. And let us alſo conſider a graft of one kind of fruit upon the upper bough of a tree of another kind; as (for inſtance) the ſcion of a pear upon a white-thorn; for there the aſcending liquor is already altered, either by the root, or in its aſcent by the bark, or both ways, and becomes a new mixt body: as may appear by the differing qualities to be met with in the ſaps of ſeveral trees; as particularly, the medicinal virtue of birch-water, which I have ſometimes drunk upon *Helmont's* great and not undeſerved commendation. Now the graft, being faſtened to the ſtock, muſt neceſſarily nouriſh itſelf, and produce its fruit, only out of this compound juice prepared for it by the ſtock, being unable to come at any other aliment. And if we conſider, how much of the vegetable he feeds upon may (as we noted above) remain in an animal; we may eaſily ſuppoſe, that the blood of that animal, who feeds upon this, though it be a well-conſtituted liquor, and have all the differing corpuſcles, that make it up, kept in order by one preſiding form, may be a ſtrangely decompounded body, many of its parts being themſelves decompounded. So little is it neceſſary, that even in the mixtures, which nature herſelf makes in animal and vegetable bodies, ſhe ſhould have pure elements at hand to make her compoſitions of.

Having ſaid thus much touching the conſtitution of plants and animals, I might perhaps be able to ſay as much touching that of minerals, and even metals, if it were as eaſy for us to make experiment in order to the production of theſe, as of thoſe. But the growth or increment of minerals being uſually a work of exceſſively long time, and for the moſt part performed in the bowels of the earth, where we cannot ſee it, I muſt inſtead of experiments make uſe, on this occaſion, of obſervations.

That ſtones were not all made at once, but that ſome of them are now-a-days generated, may (though it be denied by ſome) be fully proved by ſeveral examples, of which I ſhall now ſcarce alledge any other, than that famous place in *France* known by the name of *Les Caves Goutieres*, where the water falling from the upper parts of the cave to the ground, does preſently there condenſe into little ſtones, of ſuch figures, as the drops, falling either ſeverally or upon one another, and coagulating preſently into ſtone, chance to exhibit. Of theſe ſtones ſome ingenious friends of ours, that went a while ſince to viſit that place, did me the favour to preſent me with ſome, that they brought thence. And I remember, that both that ſober relator of his voyages, *Van Linſchoten*, and another good author, inform us, that in the diamond mines (as they call them) in the *Eaſt-Indies*, when having digged the earth, though to no great depth, they find diamonds, and take them quite away; yet in a very few years they find in the ſame place new diamonds produced there ſince. From both which relations, eſpecially the firſt, it ſeems probable, that nature does not always ſtay for divers elementary bodies, when ſhe is to produce ſtones. And as for metals themſelves, authors of good note aſſure us, that even they were not in the beginning produced at once altogether, but have been obſerved to grow; ſo that what was not a mineral or metal before, became one afterwards. Of this it were eaſy to alledge many teſtimonies of profeſſed chymiſts. But that they may have the greater authority, I ſhall rather preſent you with a few borrowed from more unſuſpected writers: *Sulphuris mineram* (as the inquiſitive *P. Fallopius* notes) *quæ nutrix eſt caloris ſubterranei fabri ſeu archæi fontium & mineralium, infra terram citiſſimè renaſci teſtantur hiſtoriæ metallicæ. Sunt enim loca, è quibus ſi hoc anno ſulphur effoſum fuerit, intermiſſa, foſſione per quadriennium, redeunt foſſores, & omnia ſulphure, ut antea, rurſus inveniunt plena.* *Pliny* relates, *in Italiæ inſula Ilva, gigni ferri metallum.* Strabo *multo expreſſius*; *effoſſum ibi metallum ſemper regenerari. Nam ſi effoſſio ſpatio centum annorum intermittebatur, & iterum illuc revertebantur, foſſores reperiſſe maximam copiam ferri regeneratum.* Which hiſtory not only is countenanced by *Fallopius*, from the income, which the iron of that iſland yielded the duke of *Florence* in his time; but is mentioned more expreſsly to our purpoſe by the learned *Ceſalpinus*: *Vena* (ſays he) *ferri copioſiſſima eſt in Italia*; *ob eam nobilitata Ilva Tyrrheni meris inſula incredibili copia etiam noſtris temporibus eam gignens: Nam terra, quæ eruitur, dum vena effoditur tota, procedente tempore in venam convertitur.* Which laſt clauſe is therefore very notable, becauſe from thence we may deduce, that earth, by a metalline plaſtick principle latent in it, may be in proceſs of time changed into a metal. And even * *Agricola* himſelf, though the chymiſts complain of him as their adverſary, acknowledges thus much and more; by telling us, that at a town called *Saga* in *Germany*, they dig up iron in the fields, by ſinking ditches two foot deep; and adding, that within the ſpace of ten years the ditches are digged again for iron ſince produced, as the ſame metal is wont to be obtained in *Ilva*. Alſo concerning lead, not to mention what even *Galen* notes, that it will increaſe both in bulk and weight, if it be long kept in vaults or cellars, where the air is groſs and thick, as he collects from the ſwelling of thoſe pieces of lead, that were employed to faſten together the parts of old ſtatues. Not to mention this, I ſay, *Boccacius Certaldus*, as I find him quoted by a diligent writer, has this paſſage touching the growth of lead: *Feſſularum mons* (ſays he) *in Hetruria, Florentiæ civitati imminens, lapides plumbarios habet; qui ſi excidantur, brevi temporis ſpatio, novis incrementis inſtaurantur*; *ut* (annexes my author) *tradit Boccacius Certaldus, qui id compertiſſimum eſſe ſcribit. Nihil hoc novi eſt; ſed de eodem Plinius, lib.* 34. *Hiſt. Natur. cap.* 17. *dudum prodidit, inquiens, mirum in his ſolis plumbi metallis, quod derelicta fertilius reviviſcunt. In plumbariis ſecundo lapide ab Amberga dictis ad aſylum recrementa congeſta in cumulos, expoſita ſolibus pluviiſque paucis annis, reddunt ſuum metallum cum fœnore.* I might add to theſe (continues *Carneades*) many things, that I have met with concerning the generation of gold and ſilver. But for fear of wanting time, I ſhall mention but two or three narratives. The firſt you may find recorded by *Gerhardus*, the phyſick profeſſor, in theſe words: *In valle* (ſays he) *Joachimica argentum graminis modo & more è lapidibus mineræ velut è radice excreviſſe digiti longitudine, teſtis eſt Dr. Sehreterus, qui ejuſmodi venas aſpectu jucundas & admirabiles domi ſuæ aliis ſæpe monſtravit & donavit. Item aqua cærulea inventa eſt Annebergæ, ubi argentum erat adhuc in primo ente, quæ coagulata redacta eſt in calcem fixi & boni argenti.*

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* *In Lygiis, ad Sagam oppidum, in pratis eruitur ferrum, foſſis ad altitudinem bipedaneam actis. Id decennio renatum, denuo foditur, non aliter ac Ilvæ ferrum.*

THE other two relations I have not met with in Latin authors, and yet they are both very memorable in themſelves, and pertinent to our preſent purpoſe.

THE firſt I met with in the commentary of *Johannes Valehius* upon the *Kleine Baur*, in which that induſtrious chymiſt relates, with many circumſtances, that at a mine-town (if I may ſo Engliſh the German *Bergſtat*) eight miles or leagues diſtant from *Straſburg* called *Mariakirch*, a workman came to the overſeer, and deſired employment; but he telling him, that there was not any of the beſt ſort at preſent for him, added, that till he could be preferred to ſome ſuch, he might in the mean time, to avoid idleneſs, work in a grove or mine-pit thereabouts, which at that time was little eſteemed. This workman, after ſome weeks labour, had by a crack appearing in the ſtone upon a ſtroke given near the wall, an invitation given him to work his way through; which as ſoon as he had done, his eyes were ſaluted by a mighty ſtone or lump, which ſtood in the middle of the cleft (that had a hollow place behind it) upright, and in ſhew like an armed man; but conſiſted of pure fine ſilver having no vein or ore by it, or any other additament, but ſtood there free, having only under foot ſomething like a burnt matter; and yet this one lump held in weight above 1000 marks, which, according to the Dutch account, makes 500 pound weight of fine ſilver. From which and other circumſtances my author gathers, that by the warmth of the place, the noble metalline ſpirits, (ſulphureous and mercurial) were carried from the neighbouring galleries or vaults, through other ſmaller cracks and clefts into that cavity, that there collected as in a cloſe chamber or cellar; whereinto when they were gotten, they did in proceſs of time ſettle into the fore-mentioned precious maſs of metal.

The other German relation is of that great traveller and laborious chymiſt *Joannes* (not *Georgius) Agricola*; who, in his notes upon what *Poppius* has written of antimony, relates, that when he was among the Hungarian mines in the deep groves, he obſerved, that there would often ariſe in them a warm ſteam, (not of that malignant ſort, which the Germans call *Shwadt*, which, ſays he, is a mere poiſon, and often ſuffocates the diggers) which faſtened itſelf to the walls; and that coming again to review it after a couple of days, he diſcerned, that it was all very faſt, and gliſtering: whereupon, having collected it and diſtilled it *per retortam*, he obtained from it a fine ſpirit; adding, that the mine-men informed him, that this ſteam or damp (as the Engliſhmen alſo call it, retaining the Dutch term) would at laſt have become a metal, as gold or ſilver.

I refer (ſays *Carneades)* to another occaſion, the uſe that may be made of theſe narratives towards the explicating the nature of metals; and that of fixedneſs, malleableneſs, and ſome other qualities conſpicuous in them. And in the mean time, this I may at preſent deduce from theſe obſervations; that it is not very probable, that whenſoever a mineral, or even a metal, is to be generated in the bowels of the earth, nature needs to have at hand both ſalt and ſulphur, and mercury to compound it of: for, not to urge, that the two laſt relations ſeem leſs to favour the chymiſts than *Ariſtotle*, who would have metals generated of certain halitus or ſteams, the fore-mentioned obſervations together make it ſeem more likely, that the mineral earths, or thoſe metalline ſteams (wherewith probably ſuch earths are plentifully imbued) do contain in them ſome ſeminal rudiment, or ſomething equivalent thereunto; by whoſe plaſtick power the reſt of the matter, though perhaps terreſtrial and heavy, is, in tract of time, faſhioned into this or that metalline ore; almoſt (as I formerly noted) as that fair water was by the ſeminal principle of mint, pompions, and other vegetables, contrived into bodies anſwerable to ſuch ſeeds. And that ſuch alterations of terreſtrial matter are not impoſſible, ſeems evident from that notable practice of the boilers of ſalt-petre, who unanimouſly obſerve, as well here in *England* as in other countries, that if an earth pregnant with nitre be deprived, by the affuſion of water, of all its true and diſſoluble ſalt, yet the earth will, after ſome years, yield them ſalt-petre again: for which reaſon, ſome of the eminent and ſkilfulleſt of them keep it in heaps as a perpetual mine of ſalt-petre. Whence it may appear, that the ſeminal principle of nitre, latent in the earth, does, by degrees, transform the neighbouring matter into a nitrous body: for though I deny, that ſome volatile nitre may, by ſuch earths, be attracted (as they ſpeak) out of the air, yet that the innermoſt parts of ſuch great heaps, that lie ſo remote from the air, ſhould borrow from it all the nitre they abound with, is not probable, for other reaſons beſides the remoteneſs of the air, though I have not the leiſure to mention them.

And I remember, that a perſon of great credit, and well acquainted with the ways of making vitriol, affirmed to me, that he had obſerved, that a kind of mineral, which abounds in that ſalt, being kept within doors, and not expoſed (as is uſual) to the free air and rains, did of itſelf in no very long time turn into vitriol, not only in the outward or ſuperficial, but even in the internal and moſt central parts.

And I alſo remember, that I met with a certain kind of marchaſite, that lay together in great quantities under ground, which did, even in my chamber, in ſo few hours begin of itſelf to turn into vitriol, that we need not diſtruſt the newly recited narrative. But to return to what I was ſaying of nitre; as nature made this ſalt-petre out of the once almoſt inodorous earth it was bred in, and did not find a very ſtinking and corroſive acid liquor, and a ſharp alcalizate ſalt to compound it of, though theſe be the bodies, into which the fire diſſolves it; ſo it were not neceſſary, that nature ſhould make up all metals and other minerals of pre-exiſtent ſalt, and ſulphur, and mercury, though ſuch bodies might by fire be obtained from it. Which one conſideration duly weighed is very conſiderable in the preſent controverſy: and to this agree well the relations of our two German chymiſts; for beſides that it cannot be convincingly proved, it is not ſo much as likely, that ſo languid and moderate a heat, as that within the mines, ſhould carry up to ſo great a height, though in the form of fumes, ſalt, ſulphur, and mercury; ſince we find in our diſtillations, that it requires a conſiderable degree of fire to raiſe, ſo much as to the height of one foot, not only ſalt, but even mercury itſelf in cloſe veſſels. And if it be objected, that it ſeems by the ſtink, that is ſometimes obſerved, when lightning falls down here below, that ſulphureous ſteams may aſcend very high without any extraordinary degree of heat; it may be anſwered, among other things, that the ſulphur of ſilver is by chymiſts ſaid to be a fixt ſulphur, though not altogether ſo well digeſted as that of gold.

BUT (proceeds *Carneades)* if it had not been to afford you ſome hints concerning the origin of metals, I need not have deduced any thing from theſe obſervations; it not being neceſſary to the validity of my argument, that my deductions from them ſhould be irrefragable, becauſe my adverſaries the Ariſtotelians and vulgar chymiſts do not, I preſume, know any better than I, *à priori*, of what ingredients Nature compounds metals and minerals. For their argument to prove, that thoſe bodies are made up of ſuch principles, is drawn *à poſteriori*; I mean from this, that upon the analyſis of mineral bodies they are reſolved into thoſe differing ſubſtances. That we may therefore examine this argument, let us proceed to conſider, what can be alledged in behalf of the elements from the reſolutions of bodies by the fire; which you remember was the ſecond topick, whence I told you the arguments of my adverſaries were deſumed.

AND that I may firſt diſpatch what I have to ſay concerning minerals, I will begin the remaining part of my diſcourſe with conſidering how the fire divides them.

AND firſt, I have partly noted above, that though chymiſts pretend from ſome to draw ſalt, from others running mercury, and from others a ſulphur; yet they have not hitherto taught us, by any way in uſe among them, to ſeparate any one principle, whether ſalt, ſulphur, or mercury, from all ſorts of minerals without exception. And thence I may be allowed to conclude, that there is not any of the elements, that is an ingredient of all bodies, ſince there are ſome, of which it is not ſo.

IN the next place, ſuppoſing, that either ſulphur or mercury were obtainable from all ſorts of minerals; yet ſtill this ſulphur or mercury would be but a compounded, not an elementary body, as I told you already on another occaſion. And certainly he, that takes notice of the wonderful operations of quickſilver, whether it be common, or drawn from mineral bodies, can ſcarce be ſo inconſiderate as to think it of the very ſame nature with that immature and fugitive ſubſtance, which in vegetables and animals chymiſts have been pleaſed to call their mercury. So that when mercury is got by the help of the fire out of a metal or other mineral body, if we will not ſuppoſe, that it was not pre-exiſtent in it, but produced by the action of the fire upon the concrete, we may at leaſt ſuppoſe this quickſilver to have been a perfect body of its own kind (though perhaps leſs heterogeneous than more ſecondary mixts) which happened to be mingled *per minima*, and coagulated with the other ſubſtances, whereof the metal or mineral conſiſted. As may be exemplified, partly by native vermilion, wherein the quickſilver and ſulphur being exquiſitely blended both with one another, and that other coarſe mineral ſtuff (whatever it be) that harbours them, make up a red body differing enough from both; and yet from which part of quickſilver, and of the ſulphur, may be eaſily enough obtained; partly by thoſe mines, wherein nature has ſo curiouſly incorporated ſilver with lead, that it is extremely difficult, and yet poſſible to ſeparate the former out of the latter; and partly too by native vitriol, wherein the metalline corpuſcles are by ſkill and induſtry ſeparable from the ſaline ones, though they be ſo con-coagulated with them, that the whole concrete is reckoned among ſalts.

AND here I further obſerve, that I never could ſee any earth or water, properly ſo called, ſeparated from either gold or ſilver, (to name now no other metalline bodies;) and therefore to retort the argument upon my adverſaries, I may conclude, that ſince there are ſome bodies, in which, for aught appears, there is neither earth nor water; I may be allowed to conclude, that neither of thoſe two is an univerſal ingredient of all thoſe bodies, that are counted perfectly mixt, which I deſire you would remember againſt anon.

IT may indeed be objected, that the reaſon, why from gold or ſilver we cannot ſeparate any moiſture, is, becauſe that when it is melted out of the ore, the vehement fire requiſite to its fuſion forced away all the aqueous and fugitive moiſture; and the like fire may do from the materials of glaſs. To which I ſhall anſwer, that I remember I read not long ſince in the learned *Joſephus Acoſta* *, who relates it upon his own obſervation, that in *America* (where he long lived) there is a kind of ſilver, which the Indians call *papas*, and ſometimes (ſays he) they find pieces very fine and pure like to ſmall round roots, the which is rare in that metal, but uſual in gold; concerning which metal he tells us, that beſides this they find ſome, which they call gold in grains, which he tells us are ſmall morſels of gold, that they find whole without mixture of any other metal, which hath no need of melting or refining in the fire.

I REMEMBER, that a very ſkilful and credible perſon affirmed to me, that being in the Hungarian mines he had the good fortune to ſee a mineral, that was there digged up, wherein pieces of gold of the length, and alſo almoſt of the bigneſs of a human finger, grew in the ore, as if they had been parts and branches of trees.

AND I have myſelf ſeen a lump of whitiſh mineral, that was brought as a rarity to a great and knowing prince, wherein there grew here and there in the ſtone, which looked like a kind of ſpar, divers little lumps of fine gold, (for ſuch I was aſſured that trial had manifeſted it to be) ſome of them ſeeming to be about the bigneſs of peas.

BUT that is nothing to what our *Acoſta* † ſubjoins, which is indeed very memorable; namely, that of the morſels of native and pure gold, which we lately heard him mentioning, he had now and then ſeen ſome weighed many pounds: to which I ſhall add, that I myſelf have ſeen a lump of ore not long ſince digged up, in whoſe ſtony part there grew, almoſt like trees, divers parcels, though not of gold, yet of (what perhaps mineraliſts will more wonder at) another metal, which ſeemed to be very pure or unmixt with any heterogeneous ſubſtances, and were ſome of them as big as my finger, if not bigger. But upon obſervations of this kind, though perhaps I could, yet I muſt not at preſent, dwell any longer.

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* *Acoſta's* Natural and Moral Hiſtory of the *Indies*, *L.* 3. *c.* 5. *p.* 212.

† See *Acoſta* in the fore-cited place, and the paſſage of *Pliny* quoted by him.

To proceed therefore now (ſays *Carneades)* to the conſideration of the analyſis of vegetables, although my trials give me no cauſe to doubt, but that out of moſt of them five differing ſubſtances may be obtained by the fire, yet I think it will not be ſo eaſily demonſtrated, that theſe deſerve to be called elements in the notion above explained.

AND before I deſcend to particulars, I ſhall repeat and premiſe this general conſideration, that theſe differing ſubſtances, that are called elements or principles, differ not from each other as metals, plants, and animals, or as ſuch creatures as are immediately produced each by its peculiar ſeed, and conſtitutes a diſtinct propagable ſort of creatures in the univerſe; but theſe are only various ſchemes of matter or ſubſtances, that differ from each other, but in conſiſtence (as running mercury and the ſame metal congealed by the vapour of lead) and ſome very few other accidents, as taſte, or ſmell, or inflammability, or the want of them. So that by a change of texture, not impoſſible to be wrought by the fire and other agents, that have the faculty, not only to diſſociate the ſmall parts of bodies, but afterwards to connect them after a new manner, the ſame parcel of matter may acquire or loſe ſuch accidents, as may ſuffice to denominate it ſalt, or ſulphur, or earth. If I were fully to clear to you my apprehenſions concerning this matter, I ſhould perhaps be obliged to acquaint you with divers of the conjectures (for I muſt yet call them no more) I have had concerning the principles of things purely corporeal: for though, becauſe I ſeem not ſatisfied with the vulgar doctrines, either of the Peripatetic or Paracelſian ſchools many of thoſe, that know me (and perhaps among them, *Eleutherius* himſelf) have thought me wedded to the Epicurean hypotheſis (as others have miſtaken me for an Helmontian) yet if you knew how little converſant I have been with Epicurean authors, and how great a part of *Lucretius* himſelf I never yet had the curioſity to read, you would perchance be of another mind: eſpecially if I were to entertain you at large, I ſay not with my preſent notions, but with my former thoughts concerning the principles of things. But, as I ſaid above, fully to clear my apprehenſions would require a longer diſcourſe than we can now have.

FOR I ſhould tell you, that I have ſometimes thought it not unfit, that to the principles, which may be aſſigned to things, as the world is now conſtituted, we ſhould, if we conſider the great maſs of matter, as it was whilſt the univerſe was in making, add another, which may conveniently enough be called an architectonic principle or power; by which I mean thoſe various determinations, and that ſkilful guidance of the motions of the ſmall parts of the univerſal matter by the moſt wiſe author of things, which were neceſſary at the beginning to turn that confuſed chaos into this orderly and beautiful world; and eſpecially to contrive the bodies of animals and plants, and the ſeeds of thoſe things, whoſe kinds were to be propagated. For I confeſs I cannot well conceive, how from matter, barely put into motion, and then left to itſelf, there could emerge ſuch curious fabricks, as the bodies of men and perfect animals, and ſuch yet more admirably contrived parcels of matter, as the ſeeds of living creatures.

I SHOULD likewiſe tell you upon what grounds, and in what ſenſe, I ſuſpected the principles of the world, as it now is, to be three, matter, motion and reſt: I ſay, as the world now is, becauſe the preſent fabric of the univerſe, and eſpecially the ſeeds of things, together with the eſtabliſhed courſe of nature, is a requiſite or condition, upon whoſe account divers things may be made out by our three principles, which otherwiſe would be very hard, if poſſible, to explicate.

I SHOULD moreover declare in general (for I pretend not to be able to do it otherwiſe) not only why I conceive that colours, odours, taſtes, fluidneſs and ſolidity, and thoſe other qualities, that diverſify and denominate bodies, may intelligibly be deduced from theſe three; but how two of the three Epicurean principles (which, I need not tell you, are magnitude, figure, and weight) are themſelves deducible from matter and motion; ſince the latter of theſe variouſly agitating, and, as it were, diſtracting the former, muſt needs disjoin its parts; which being actually ſeparated, muſt each of them neceſſarily both be of ſome ſize, and obtain ſome ſhape or other. Nor did I add to our principles the Ariſtotelian privation, partly for other reaſons, which I muſt not now ſtay to inſiſt on; and partly, becauſe it ſeems to be rather an antecedent, or a *terminus. à quo*, than a true principle, as the ſtarting-poſt is none of the horſe's legs or limbs.

I SHOULD alſo explain, why and how I made reſt to be, though not ſo conſiderable a principle of things, as motion, yet a principle of them; partly, becauſe it is (for aught we know) as antient at leaſt as it, and depends not upon motion, nor any other quality of matter; and partly, becauſe it may enable the body, in which it happens to be, both to continue in a ſtate of reſt, till ſome external force put it out of that ſtate, and to concur to the productions of divers changes in the bodies, that hit againſt it, by either quite ſtopping or leſſening their motion (whilſt the body formerly at reſt receives all or part of it into itſelf) or elſe by giving a new biaſs, or ſome other modification, to motion, that is, to the grand and primary inſtrument, whereby nature produces all the changes and other qualities, that are to be met with in the world.

I SHOULD likewiſe, after all this, explain to you how, although matter, motion, and reſt, ſeemed to me to be the catholic principles of the univerſe, I thought the principles of particular bodies might be commodiouſly enough reduced to two; namely matter, and (what comprehends the two other, and their effects) the reſult, or aggregate, or complex of thoſe accidents, which are the motion or reſt (for in ſome bodies both are not to be found) the bigneſs, figure, texture, and the thence reſulting qualities of the ſmall parts, which are neceſſary to entitle the body, whereto they belong, to this or that peculiar denomination; and diſcriminating it from others, to appropriate it to a determinate kind of things (as yellowneſs, fixedneſs, ſuch a degree of weight, and of ductility, do make the portion of matter wherein they concur, to be reckoned among perfect metals, and obtain the name of gold;) this aggregate or reſult of accidents you may, if you pleaſe, call either ſtructure, or texture (though indeed that do not ſo properly comprehend the motion of the conſtituent parts, eſpecially in caſe ſome of them be fluid) or what other appellation ſhall appear moſt expreſſive. Or if, retaining the vulgar term, you will call it the form of the thing it denominates, I ſhall not much oppoſe it; provided the word be interpreted to mean but what I have expreſſed, and not a ſcholaſtic ſubſtantial form, which ſo many intelligent men profeſs to be to them altogether unintelligible.

BUT (ſays *Carneades*) if you remember, that it is a ſceptic ſpeaks to you, and that it is not ſo much my preſent taſk to make aſſertions as to ſuggeſt doubts, I hope you will look upon what I have propoſed, rather as a narrative of my former conjectures touching the principles of things, than as a reſolute declaration of my preſent opinions of them; eſpecially ſince although they cannot but appear very much to their diſadvantage, if you conſider them as they are propoſed, without thoſe reaſons and explanations, by which I could perhaps make them appear much leſs extravagant; yet

I want time to offer you what may be alleged to clear and countenance theſe notions; my deſign in mentioning them unto you at preſent being, partly, to bring ſome light and confirmation to divers paſſages of my diſcourſe to you; partly, to ſhew you, that I do not (as you ſeem to have ſuſpected) embrace all *Epicurus*'s principles, but diſſent from him in ſome main things, as well as from *Ariſtotle* and the chymiſts, in others; and partly alſo, or rather chiefly, to intimate to you the grounds, upon which I likewiſe differ from *Helmont* in this, that whereas he aſcribes almoſt all things, and even diſeaſes themſelves, to their determinate ſeeds, I am of opinion, that beſides the peculiar fabrics of the bodies of plants and animals (and perhaps alſo of ſome metals and minerals) which I take to be effects of ſeminal principles, there are many other bodies in nature, which have and deſerve diſtinct and proper names, but yet do but reſult from ſuch contextures of the matter they are made of, as may without determinate ſeeds be effected by heat, cold, artificial mixtures and compoſitions, and divers other cauſes, which ſometimes nature imploys of her own accord, and oftentimes man by his power and ſkill makes uſe of, to faſhion the matter according to his intentions. This may be exemplified both in the productions of nature, and in thoſe of art: of the firſt ſort I might name multitudes; but to ſhew how ſlight a variation of textures, without addition of new ingredients, may procure a parcel of matter divers names, and make it be looked upon as different things;

I SHALL invite you to obſerve with me, that clouds, rain, hail, ſnow, froſt, and ice, may be but water, having its parts varied as to their ſize and diſtance in reſpect of each other, and as to motion and reſt. And among artificial productions we may take notice (to ſkip the cryſtals of tartar) of glaſs, regulus martis ſtellatus, and particularly of the ſugar of lead; which, though made of that inſipid metal and ſour ſalt of vinegar, has in it a ſweetneſs ſurpaſſing that of common ſugar, and divers other qualities, which being not to be found in either of its two ingredients, muſt be confeſſed to belong to the concrete itſelf, upon the account of its texture.

THIS conſideration premiſed, it will be, I hope, the more eaſy to perſuade you, that the fire may as well produce ſome new textures in a parcel of matter, as deſtroy the old.

WHEREFORE hoping, that you have not forgot the arguments formerly employed againſt the doctrine of the *tria prima*; namely, that the ſalt, ſulphur, and mercury, into which the fire ſeems to reſolve vegetable and animal bodies, are yet compounded, not ſimple and elementary ſubſtances; and that (as appeared by the experiment of pompions) the *tria prima* may be made out of water; hoping, I ſay, that you remember theſe, and the other things, that I formerly repreſented to the ſame purpoſe, I ſhall now only add, that if we doubt not the truth of ſome of *Helmont*'s relations, we may well doubt, whether any of theſe heterogeneities be (I ſay, not pre-exiſtent, ſo as to convene together, when a plant or animal is to be conſtituted, but) ſo much as in-exiſtent in the concrete, whence they are obtained, when the chymiſt firſt goes about to reſolve it. For, not to inſiſt upon the un-inflammable ſpirit of ſuch concretes, becauſe that may be pretended to be but a mixture of phlegm and ſalt, the oil or ſulphur of vegetables or animals is, according to him, reducible by the help of lixiviate ſalts into ſoap; as that ſoap is by the help of repeated diſtillations from a *caput mortuum* of chalk into inſipid water. And as for the ſaline ſubſtance, that ſeems ſeparable from mixt bodies, the ſame * *Helmont*'s trials give us cauſe to think, that it may be a production of the fire, which, by tranſporting and otherwiſe altering the particles of the matter, does bring it to a ſaline nature.

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* *Omne autem alcali addita pinguedine in aqueum liquorem, qui tandem mera & ſimplex aqua ſit, reducitur (ut videre eſt in ſapone, lazurio lapide, &c.) quoties per adjuncta fixa ſemen pinguedinis deponit.* Helmont.

FOR I know (ſays he, in the place formerly alleged to another purpoſe) a way to reduce all ſtones into a mere ſalt of equal weight with the ſtone, whence it was produced, and that without any of the leaſt either ſulphur or mercury; which aſſeveration of my author would perhaps ſeem leſs incredible to you, if I durſt acquaint you with all I could ſay upon that ſubject. And hence by the way you may alſo conclude, that the ſulphur and mercury, as they call them, that chymiſts are wont to obtain from compound bodies by the fire, may poſſibly in many caſes be the productions of it; ſince if the ſame bodies had been wrought upon by the agents employed by *Helmont*, they would have yielded neither ſulphur nor mercury; and thoſe portions of them, which the fire would have preſented us in the form of ſulphureous and mercurial bodies, would have, by *Helmont*'s method, been exhibited to us in the form of ſalt.

BUT though (ſays *Eleutherius)* you have alleged very plauſible arguments againſt the *tria prima*, yet I ſee not how it will be poſſible for you to avoid acknowledging, that earth and water are elementary ingredients, though not of mineral concretes, yet of all animal and vegetable bodies; ſince if any of theſe, of what ſort ſoever, be committed to diſtillation, there is regularly and conſtantly ſeparated from it a phlegm or aqueous part, and a *caput mortuum* or earth.

I READILY acknowledge (anſwers *Carneades)* it is not ſo eaſy to reject water and earth (and eſpecially the former) as it is to reject the *tria prima*, from being the elements of mixt bodies; but it is not every difficult thing, that is impoſſible.

I CONSIDER then, as to water, that the chief qualities, which make men give that name to any viſible ſubſtance, are, that it is fluid or liquid, and that it is inſipid and inodorous. Now, as for the taſte of theſe qualities, I think you have never ſeen any of thoſe ſeparated ſubſtances, that the chymiſts call phlegm, which was perfectly devoid both of taſte and ſmell: and if you object, that yet it may be reaſonably ſuppoſed, that ſince the whole body is liquid, the maſs is nothing but elementary water faintly imbued with ſome of the ſaline or ſulphureous parts of the ſame concrete, which it retained with it upon its ſeparation from the other ingredients: to this I anſwer, that this objection would not appear ſo ſtrong as it is plauſible, if chymiſts underſtand the nature of fluidity and compactneſs; and that, as I formerly obſerved, to a body's being fluid there is nothing neceſſary, but that it be divided into parts ſmall enough; and that theſe parts be put into ſuch a motion among themſelves, as to glide ſome this way, and ſome that way, along each other's ſurfaces. So that although a concrete were never ſo dry, and had not any water or other liquor in-exiſtent in it, yet ſuch a comminution of its parts may be made, by the fire or other agents, as to turn a great portion of them into liquor. Of this truth I will give an inſtance, employed by our friend here preſent, as one of the moſt conducive of his experiments to illuſtrate the nature of ſalts. If you take, then, ſea-ſalt, and melt it in the fire, to free it from the aqueous parts, and afterwards diſtil it with a vehement fire from burnt clay, or any other, as dry a *caput mortuum* as you pleaſe, you will, as chymiſts confeſs by teaching it, drive over a good part of the ſalt in the form of a liquor. And to ſatisfy ſome ingenious men, that a great part of this liquor was ſtill true ſea-ſalt, brought by the operation of the fire into corpuſcles ſo ſmall, and perhaps ſo advantageouſly ſhaped, as to be capable of the form of a fluid body, he did in my preſence pour to ſuch ſpiritual ſalts a due proportion of the ſpirit (or ſalt and phlegm) of urine, whereby having evaporated the ſuperfluous moiſture, he ſoon obtained ſuch another concrete, both as to taſte and ſmell, and eaſy ſublimableneſs, as common ſalt armoniac, which you know is made up of groſs and undiſtilled ſea-ſalt, united with the ſalts of urine and of ſoot, which too are very near of kin to each other. And further, to manifeſt, that the corpuſcles of ſea-ſalt and the ſaline ones of the urine retain their ſeveral natures in this concrete, he mixt it with a convenient quantity of ſalt of tartar, and committing it to diſtillation, ſoon regained his ſpirit of urine in a liquid form by itſelf, the ſea-ſalt ſtaying behind with the ſalt of tartar. Wherefore it is very poſſible, that dry bodies may by the fire be reduced to liquors without any ſeparation of elements, but barely by a certain kind of diſſipation and comminution of the matter, whereby its parts are brought into a new ſtate. And if it be ſtill objected, that the phlegm of mixt bodies muſt be reputed water, becauſe ſo weak a taſte needs but a very ſmall proportion of ſalt to impart it; it may be replied, that, for aught appears, common ſalt and divers other bodies, though they be diſtilled never ſo dry, and in never ſo cloſe veſſels, will yield each of them pretty ſtore of a liquor, wherein though (as I lately noted) ſaline corpuſcles abound, yet there is beſides a large proportion of phlegm, as may eaſily be diſcovered by coagulating the ſaline corpuſcles with any convenient body; as I lately told you, our friend coagulated part of the ſpirit of ſalt with ſpirit of urine; and as I have divers times ſeparated a ſalt from oil of vitriol itſelf (though a very ponderous liquor, and drawn from a ſaline body) by boiling it with a juſt quantity of mercury, and then waſhing the newly coagulated ſalt from the precipitate with fair water. Now to what can we more probably aſcribe this plenty of aqueous ſubſtance afforded us by the diſtillation of ſuch bodies, than unto this, that among the various operations of the fire upon the matter of a concrete, divers particles of that matter are reduced to ſuch a ſhape and bigneſs, as is requiſite to compoſe ſuch a liquor, as chymiſts are wont to call phlegm or water? How I conjecture this change may be effected, it is neither neceſſary for me to tell you, nor poſſible to do ſo without a much longer diſcourſe than were now ſeaſonable. But I deſire you would, with me, reflect upon what I formerly told you, concerning the change of quickſilver into water; for that water having but a very faint taſte, if any whit more than divers of thoſe liquors, that chymiſts refer to phlegm, by that experiment it ſeems evident, that even a metalline body, and therefore much more ſuch as are but vegetable or animal, may by a ſimple operation of the fire be turned in great part into water. And ſince thoſe I diſpute with are not yet able out of gold, or ſilver, or divers other concretes, to ſeparate any thing like water; I hope I may be allowed to conclude againſt them, that water itſelf is not an univerſal and pre-exiſtent ingredient of mixt bodies.

But as for thoſe chymiſts, that, ſuppoſing with me the truth of what *Helmont* relates of the alkaheſt's wonderful effects, have a right to preſs me with his authority concerning them, and to allege, that he could tranſmute all reputed mixt bodies into inſipid and mere water; to thoſe I ſhall repreſent, that though his affirmations conclude ſtrongly againſt the vulgar chymiſts (againſt whom I have not therefore ſcrupled to employ them) ſince they evince, that the commonly reputed principles or ingredients of things are not permanent and indeſtructible, ſince they may be further reduced into inſipid phlegm differing from them all; yet till we can be allowed to examine this liquor, I think it not unreaſonable to doubt whether it be not ſomething elſe than mere water. For I find not any other reaſon given by *Helmont* of his pronouncing it ſo, than that it is inſipid. Now ſapour being an accident or an affection of matter, that relates to our tongue, palate, and other organs of taſte, it may very poſſibly be, that the ſmall parts of a body may be of ſuch a ſize and ſhape, as either by their extreme littleneſs, or by their ſlenderneſs, or by their figure, to be unable to pierce into and make perceptible impreſſion upon the nerves or membranous parts of the organs of taſte, and yet may be fit to work otherwiſe upon divers other bodies than mere water can, and conſequently to diſcloſe itſelf to be of a nature far enough from elementary. In ſilk dyed red or of any other colour, whilſt many contiguous threads make up a ſkein, the colour of the ſilk is conſpicuous; but if only a very few of them be looked upon, the colour will appear much fainter than before. But if you take out one ſimple thread, you ſhall not eaſily be able to diſcern any colour at all; ſo ſubtile an object having not the force to make upon the optic nerve an impreſſion great enough to be taken notice of. It is alſo obſerved, that the beſt ſort of oil-olive is almoſt taſteleſs; and yet I need not tell you how exceedingly diſtant in nature oil is from water. The liquor, into which I told you, upon the relation of *Lully* an eye-witneſs, that mercury might be tranſmuted, has ſometimes but a very languid, if any taſte; and yet its operations, even upon ſome mineral bodies, are very peculiar. Quickſilver itſelf alſo, though the corpuſcles it conſiſts of be ſo very ſmall, as to get into the pores of that cloſeſt and compacteſt of bodies, gold, is yet (you know) altogether taſteleſs. And our *Helmont* ſeveral times tells us, that fair water, wherein a little quantity of quickſilver has lain for ſome time, though it acquire no certain taſte or other ſenſible quality from the quickſilver; yet it has a power to deſtroy worms in human bodies; which he does much, but not cauſeleſsly extol. And I remember, a great lady, that had been eminent for her beauty in divers courts, confeſſed to me, that this inſipid liquor was of all innocent waſhes for the face the beſt, that ſhe ever met with.

And here let me conclude my diſcourſe, concerning ſuch waters or liquors as I have hitherto been examining, with theſe two conſiderations. Whereof the firſt is, that by reaſon of our being wont to drink nothing but wine, beer, cyder, or other ſtrongly taſted liquors, there may be in ſeveral of thoſe liquors, that are wont to paſs for inſipid phlegm, very peculiar and diſtinct taſtes, though unheeded (and perhaps not to be perceived) by us. For to omit what naturaliſts affirm of apes (and which probably may be true of divers other animals) that they have a more exquiſite palate than men; among men themſelves, thoſe, that are wont to drink nothing but water, may (as I have tried in myſelf) diſcern very ſenſibly a great difference of taſtes in ſeveral waters, which one unaccuſtomed to drink water would take to be all alike inſipid. And this is the firſt of my two conſiderations. The other is, that it is not impoſſible, that the corpuſcles, into which a body is diſſipated by the fire, may by the operation of the ſame fire have their figures ſo altered, or may be by aſſociations with one another brought into little maſſes of ſuch a ſize and ſhape, as not to be fit to make ſenſible impreſſions on the tongue. And that you may not think ſuch alterations impoſſible, be pleaſed to conſider with me, that not only the ſharpeſt ſpirit of vinegar having diſſolved as much coral as it can, will coagulate with it into a ſubſtance, which, though ſoluble in water like ſalt, is incomparably leſs ſtrongly taſted than the vinegar was before; but (what is more conſiderable) though the acid ſalts, that are carried up with quickſilver, in the preparation of common ſublimate, are ſo ſharp, that being moiſtened with water it will corrode ſome of the metals themſelves; yet this corroſive ſublimate being twice or thrice re-ſublimed with a full proportion of inſipid quickſilver, conſtitutes (as you know) that factitious concrete, which the chymiſts call *mercurius dulcis*; not becauſe it is ſweet, but becauſe the ſharpneſs of the corroſive ſalts is ſo taken away by their combination with the mercurial corpuſcles, that the whole mixture, when it is prepared, is judged to be inſipid.

And thus (continues *Carneades*) having given you ſome reaſons, why I refuſe to admit elementary water for a conſtant ingredient of mixt bodies, it will be eaſy for me to give you an account, why I alſo reject earth.

For firſt, it may well be ſuſpected, that many ſubſtances paſs among chymiſts under the name of earth, becauſe, like it, they are dry, and heavy, and fixt; which yet are very far from an elementary nature. This you will not think improbable, if you recall to mind what I formerly told you concerning what chymiſts call the dead earth of things, and eſpecially touching the copper to be drawn from the *caput mortuum* of vitriol; and if alſo you allow me to ſubjoin a caſual, but memorable experiment, made by *Johannes Agricola* upon the *terra damnata* of brimſtone. Our author then tells us (in his notes upon *Popius*) that in the year 1621 he made an oil of ſulphur; the remaining fæces he reverberated in a moderate fire fourteen days; afterwards he put them well luted up in a wind oven, and gave them a ſtrong fire for ſix hours, purpoſing to calcine the fæces to a perfect whiteneſs, that he might make ſomething elſe out of them. But coming to break the pot, he found above but very little fæces, and thoſe grey and not white; but beneath there lay a fine red regulus, which he firſt marvelled at, and knew not what to make of, being well aſſured, that not the leaſt thing, beſides the fæces of the ſulphur, came into the pot; and that the ſulphur itſelf had only been diſſolved in linſeed-oil. This regulus he found heavy and malleable almoſt as lead; having cauſed a goldſmith to draw him a wire of it, he found it to be of the faireſt copper, and ſo rightly coloured, that a Jew of *Prague* offered him a great price for it. And of this metal he ſays he had twelve *loth* (or ſix ounces) out of one pound of aſhes or fæces. And this ſtory may well incline us to ſuſpect, that ſince the *caput mortuum* of the ſulphur was kept ſo long in the fire, before it was found to be any thing elſe than a *terra damnata*, there may be divers other reſidences of bodies, which are wont to paſs only for the terreſtrial fæces of things, and therefore to be thrown away, as ſoon as the diſtillation or calcination of the body that yielded them is ended; which yet, if they were long and ſkilfully examined by the fire, would appear to be differing from elementary earth. And I have taken notice of the unwarrantable forwardneſs of common chymiſts to pronounce things uſeleſs fæces, by obſerving how often they reject the *caput mortuum* of verdigreaſe; which is yet ſo far from deſerving that name, that not only by ſtrong fires, and convenient additaments, it may in ſome hours be reduced into copper, but with a certain flux powder I ſometimes make for recreation, I have in two or three minutes obtained that metal from it. To which I may add, that having for trial's ſake kept Venerian talc in no leſs a heat than that of a glaſs-furnace, I found, after all the brunt of the fire it had indured, the remaining body, though brittle and diſcoloured, had not loſt very much of its former bulk, and ſeemed ſtill to be nearer of kin to talc, than to meer earth. And I remember too, that a candid mineraliſt, famous for his ſkill in trying of ores, requeſting me one day to procure him a certain American mineral earth of a virtuoſo, who, he thought, would not refuſe me; I inquired of him, why he ſeemed ſo greedy of it: he confeſſed to me, that this gentleman having brought that earth to the public ſay-maſters; and upon their being unable by any means to bring it to fuſion, or make it fly away, he (the relator) had procured a little of it; and having tried it with a peculiar flux, ſeparated from it near a third part of pure gold; ſo great miſtakes may be committed in haſtily concluding things to be uſeleſs earth.

Next, it may be ſuppoſed, that as in the reſolution of bodies by the fire, ſome of the diſſipated parts may, by their various occurſion occaſioned by the heat, be brought to ſtick together ſo cloſely, as to conſtitute corpuſcles too heavy for the fire to carry away; the aggregate of which corpuſcles is wont to be called aſhes of earth; ſo other agents may reſolve the concrete into minute parts after ſo differing a manner, as not to produce any *caput mortuum*, or dry and heavy body. As you may remember *Helmont* above informed us, that with his great diſſolvent he divided a coal into two liquid and volatile bodies equiponderant to the coal, without any dry or fixt reſidence at all.

AND indeed, I ſee not, why it ſhould be neceſſary, that all agents, that reſolve bodies into portions of differing qualified matter, muſt work on them the ſame way, and divide them into juſt ſuch parts, both for nature and number, as the fire diſſipates them into. For ſince (as I noted before) the bulk and ſhape of the ſmall parts of bodies, together with their fitneſs and unfitneſs to be eaſily put into motion, may make the liquors, or other ſubſtances ſuch corpuſcles compoſe, as much to differ from each other, as do ſome of the chymical principles: why may not ſomething happen in this caſe, not unlike what is uſual in the groſſer diviſions of bodies by mechanical inſtruments? where we ſee, that ſome tools reduce wood, for inſtance, into parts of ſeveral ſhapes, bigneſs, and other qualities, as hatchets and wedges divide it into groſſer parts; ſome more long and ſlender, as ſplinters; and ſome more thick and irregular, as chips; but all of conſiderable bulk: but files and ſaws make a comminution of it into duſt; which, as all the others, is of the more ſolid ſort of parts; whereas others divide it into long and broad, but thin and flexible parts, as do planes. And of this kind of parts itſelf there is alſo a variety, according to the difference of the tools employed to work on the wood; the ſhavings made by the plane being in ſome things differing from thoſe ſhives or thin and flexible pieces of wood, that are obtained by borers, and theſe from ſome others obtainable by other tools. Some chymical examples applicable to this purpoſe I have elſewhere given you. To which I may add, that whereas, in a mixture of ſulphur and ſalt of tartar well melted and incorporated together, the action of pure ſpirit of wine digeſted on it is to ſeparate the ſulphureous from the alcalizate parts, by diſſolving the former, and leaving the latter, the action of wine (probably upon the ſcore of its copious phlegm) upon the ſame mixture, is to divide it into corpuſcles conſiſting of both alcalizate and ſulphureous parts united. And if it be objected, that this is but a factitious concrete; I anſwer, that however the inſtance may ſerve to illuſtrate what I propoſed, if not to prove it; and that nature herſelf doth in the bowels of the earth make decompounded bodies, as we ſee in vitriol, cinnabar, and even in ſulphur itſelf; I will not urge, that the fire divides new milk into five differing ſubſtances; but rennet and acid liquors divide it into a coagulated matter and a thin whey: and on the other ſide, churning divides it into butter and butter-milk, which may either of them yet be reduced to other ſubſtances differing from the former. I will not preſs this, I ſay, nor other inſtances of this nature, becauſe I cannot in few words anſwer what may be objected, that theſe concretes ſequeſtered without the help of the fire may by it be further divided into hypoſtatical principles. But I will rather repreſent, that whereas the ſame ſpirit of wine will diſſociate the parts of camphire, and make them one liquor with itſelf; aqua fortis will alſo disjoin them, and put them into motion; but ſo as to keep them together, and yet alter their texture into the form of an oil. I know alſo an uncompounded liquor, that an extraordinary chymiſt would not allow to be ſo much as ſaline, which doth (as I have tried) from coral itſelf (as fixt as divers judicious writers aſſert that concrete to be) not only obtain a noble tincture, without the intervention of nitre or other ſalts, but will carry over the tincture in diſtillation. And if ſome reaſons did not forbid me, I could now tell you of a menſtruum I make myſelf, that doth more oddly diſſociate the parts of minerals very fixt in the fire. So that it ſeems not incredible, that there may be ſome agent or way of operation found, whereby this or that concrete, if not all firm bodies, may be reſolved into parts ſo very minute and ſo apt to ſtick cloſe to one another, that none of them may be fixt enough to ſtay behind in a ſtrong fire, and to be incapable of diſtillation; nor conſequently to be looked upon as earth. But to return to *Helmont*, the ſame author ſomewhere ſupplies me with another argument againſt the earth's being ſuch an element, as my adverſaries would have it. For he ſomewhere affirms, that he can reduce all the terreſtrial parts of mixt bodies into inſipid water; whence we may argue againſt the earth's being one of their elements, even from that notion of elements, which you may remember *Philoponus* recited out of *Ariſtotle* himſelf, when he lately diſputed for his chymiſts againſt *Themiſtius*. And here we may on this occaſion conſider, that ſince a body, from which the fire hath driven away its looſer parts, is wont to be looked upon as earth, upon the account of its being endowed with both theſe qualities, taſtleſsneſs and fixedneſs (for ſalt of tartar, though fixed, paſſes not among the chymiſts for earth, becauſe it is ſtrongly taſted) if it be in the power of natural agents to deprive the *caput mortuum* of a body of either of thoſe two qualities, or to give them both to a portion of matter, that had them not both before, the chymiſts will not eaſily define what part of a reſolved concrete is earth, and make out, that that earth is a primary, ſimple, and indeſtructable body. Now there are ſome caſes, wherein the more ſkilful of the vulgar chymiſts themſelves pretend to be able, by repeated cohobations, and other fit operations, to make the diſtilled parts of a concrete bring its own *caput mortuum* over the helm, in the form of a liquor; in which ſtate being both fluid and volatile; you will eaſily believe it would not be taken for earth. And indeed by a ſkilful, but not vulgar, way of managing ſome concretes, there may be more effected in this kind, than you perhaps would eaſily think. And on the other ſide, that either earth may be generated, or at leaſt bodies, that did not before appear to be near totally earth, may be ſo altered as to paſs for it, ſeems very poſſible, if * *Helmont* have done that by art, which he mentions in ſeveral places; eſpecially where he ſays, that he knows ways, whereby ſulphur once diſſolved is all of it fixed into a terreſtrial powder, and the whole body of ſalt-petre may be turned into earth: which laſt he elſewhere ſays is done by the odour only of a certain ſulphureous fire. And in another place he mentions one way of doing this, which I cannot give you an account of; becauſe the materials I had prepared for trying it, were by a ſervant's miſtake unhappily thrown away.

AND, theſe laſt arguments may be confirmed by the experiment I have often had occaſion to mention, concerning the mint I produced out of water. And partly by an obſervation of *Rondoletius* concerning the growth of animals alſo, nouriſhed but by water, which I remembered not to mention, when I diſcourſed to you about the production of things out of water. This diligent writer then, in his inſtructive book of fiſhes, affirms that his wife kept a fiſh in a glaſs of water without any other food for three years; in which ſpace it was conſtantly augmented, till at laſt it could not come out of the place, at which it was put in, and at length was too big for the

Lib. 1. cap. 2.

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* *Novi item modos, quibus totum ſal-petræ in terram convertitur, totumque ſulphur ſemel diſſolutum fixetur in pulverem terreum.* Helmont. in Compl. atque Miſt. Elementor. ſect. 24.

glaſs itſelf, though that were of a large capacity. And becauſe there is no juſt reaſon to doubt, that this fiſh, if diſtilled, would have yielded the like differing ſubſtances with other animals; and however, becauſe the mint, which I had out of water, afforded me upon diſtillation a good quantity of charcoal; I think I may from thence infer, that earth itſelf may be produced out of water; or if you pleaſe, that water may be tranſmuted into earth: and conſequently, that though it could be proved, that earth is an ingredient actually in-exiſtent in the vegetable and animal bodies, whence it may be obtained by fire; yet it would not neceſſarily follow, that earth, as a pre-exiſtentt element, does with other principles convene to make up thoſe bodies, whence it ſeems to have been ſeparated.

After all is ſaid (ſays *Eleutherius*) I have yet ſomething to object, that I cannot but think conſiderable, ſince *Carneades* himſelf alleged it as ſuch; for, (continues *Eleutherius* ſmiling) I muſt make bold to try, whether you can as luckily anſwer your own arguments, as thoſe of your antagoniſts; I mean (purſues he) that part of your conceſſions, wherein you cannot but remember, that you ſupplied your adverſaries with an example to prove, that there may be elementary bodies, by taking notice, that gold may be an ingredient in a multitude of differing mixtures, and yet retain its nature, notwithſtanding all the chymiſts by their fires and corroſive waters are able to do to deſtroy it.

I sufficiently intimated to you at that time (replies *Carneades*) that I propoſed this example, chiefly to ſhew you, how nature may be conceived to have made elements, not to prove, that ſhe actually has made any; and you know, that *à poſſe ad eſſe* the inference will not hold. But (continues *Carneades*) to anſwer more directly to the objection drawn from gold, I muſt tell you, that though I know very well, that divers of the more ſober chymiſts have complained of the vulgar chymiſts, as of mountebanks or cheats, for pretending ſo vainly, as hitherto they have done, to deſtroy gold; yet I know a certain menſtruum (which our friend has made, and intends ſhortly to communicate to the ingenious) of ſo piercing and powerful a quality; that if notwithſtanding much care, and ſome ſkill, I did not much deceive myſelf, I have with it really deſtroyed even refined gold, and brought it into a metalline body of another colour and nature, as I found by trials purpoſely made. And if ſome juſt conſiderations did not for the preſent forbid it, I could perchance here ſhew you by another experiment or two of my own trying, that ſuch menſtruums may be made, as to entice away and retain divers parts from bodies, which even the more judicious and experienced Spagyriſts have pronounced irreſoluble by the fire; though (which I deſire you would mark) in neither of theſe inſtances, the gold or precious ſtones be analyſed into any of the *tria prima*, but only reduced to new concretes. And indeed there is a great diſparity betwixt the operations of the ſeveral agents, whereby the parts of a body come to be diſſipated. As if (for inſtance) you diſſolve the purer ſort of vitriol in common water, the liquor will ſwallow up the mineral, and ſo diſſociate its corpuſcles, that they will ſeem to make up but one liquor with thoſe of the water; and yet each of theſe corpuſcles retains its nature and texture, and remains a vitriolate and compounded body. But if the ſame vitriol be expoſed to a ſtrong fire, it will then be divided, not only, as before, into ſmaller parts, but into heterogeneous ſubſtances, each of the vitriolate corpuſcles, that remained entire in the water, being itſelf upon the deſtruction of its former texture diſſipated or divided into new particles of differing qualities. But inſtances more fitly applicable to this purpoſe I have already given you. Wherefore to return to what I told you about the deſtruction

tion of gold; that experiment invites me to repreſent to you, that though there were either ſaline, or ſulphureous, or terreſtrial portions of matter, whoſe parts were ſo ſmall, ſo firmly united together, or of a figure ſo fit to make them cohere to one another (as we ſee, that in quickſilver, broken into little globes, the parts brought to touch one another do immediately re-imbody) that neither the fire, nor the uſual agents, employed by chymiſts, are piercing enough to divide their parts, ſo as to deſtroy the texture of the ſingle corpuſcles; yet it would not neceſſarily follow, that ſuch permanent bodies were elementaty; ſince it is poſſible, there may be agents found in nature, ſome of whoſe parts may be of ſuch a ſize and figure, as to take better hold of ſome parts of theſe ſeemingly elementary corpuſcles, than theſe parts do of the reſt; and conſequently may carry away ſuch parts with them, and ſo diſſolve the texture of the corpuſcle by pulling its parts aſunder. And if it be ſaid, that at leaſt we may this way diſcover the elementary ingredients of things, by obſerving into what ſubſtances theſe corpuſcles, that were reputed pure, are divided; I anſwer, that it is not neceſſary, that ſuch a diſcovery ſhould be practicable. For if the particles of the diſſolvent do take ſuch firm hold of thoſe of the diſſolved body, they muſt conſtitute together new bodies, as well as deſtroy the old; and the ſtrict union, which, according to this hypotheſis, may well be ſuppoſed betwixt the parts of the emergent bodies will make it as little to be expected, that they ſhould be pulled aſunder, but by little parts of matter, that to divide them, aſſociate themſelves, and ſtick extremely cloſe to thoſe of them, which they ſever from their former adherents. Beſides that it is not impoſſible, that a corpuſcle, ſuppoſed to be elementary, may have its nature changed, without ſuffering a divorce of its parts, barely by a new texture effected by ſome powerful agent; as I formerly told you, the ſame portion of matter may eaſily, by the operation of the fire, be turned at pleaſure into the form of a brittle and tranſparent, or an opacous and malleable body.

AND indeed, if you conſider how far the bare change of texture, whether made by art or nature (or rather by nature with or without the aſſiſtance of man) can go in producing ſuch new qualities in the ſame parcel of matter, and how many inanimate bodies (ſuch as are all the chymical productions of the fire) we know are denominated and diſtinguiſhed, not ſo much by any imaginary ſubſtantial form, as by the aggregate of theſe qualities; if you conſider theſe things, I ſay, and that the varying of either figure, or the ſize, or the motion, or the ſituation, or connexion of the corpuſcles whereof any of theſe bodies is compoſed, may alter the fabric of it, you will poſſibly be invited to ſuſpect with me, that there is no great need, that nature ſhould always have elements before-hand, whereof to make ſuch bodies as we call mixts. And that it is not ſo eaſy as chymiſts and others have hitherto imagined, to diſcern, among the many differing ſubſtances, that may without any extraordinary ſkill be obtained from the ſame portion of matter, which ought to be eſteemed, excluſively to all the reſt, its in-exiſtent elementary ingredients, much leſs to determine, what primogeneal and ſimple bodies convened together to compoſe it. To exemplify this, I ſhall add, to what I have already on ſeveral occaſions repreſented, but this ſingle inſtance:

You may remember (*Eleutherius*) that I formerly intimated to you, that beſides mint and pompions, I produced divers other vegetables of very differing natures out of water. Wherefore you will not, I preſume, think it incongruous to ſuppoſe, that when a ſlender vine-ſlip is ſet into the ground, and takes root there, it may likewiſe receive its nutriment from the water attracted out of the earth by its roots, or impell-

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ed by the warmth of the ſun, or preſſure of the ambient air into the pores of them. And this you will the more eaſily believe, if you ever obſerved, what a ſtrange quantity of water will drop out of a wound given to the vine, in a convenient place, at a ſeaſonable time in the ſpring; and how little of taſte or ſmell this aqua vitæ, as phyſicians call it, is endowed with, notwithſtanding what concoction or alteration it may receive in its paſſage through the vine, to diſcriminate it from common water. Suppoſing then this liquor, at its firſt entrance into the roots of the vine, to be common water; let us a little conſider, how many various ſubſtances may be obtained from it; though, to do ſo, I muſt repeat ſomewhat, that I had a former occaſion to touch upon. And firſt, this liquor being digeſted in the plant, and aſſimilated by the ſeveral parts of it, is turned into the wood, bark, pith, leaves, *&c.* of the vine; the ſame liquor may be further dried, and faſhioned into vine-buds, and theſe a while after are advanced unto ſour grapes, which expreſſed yield verjuice, a liquor very differing in ſeveral qualities both from wine and other liquors obtainable from the vine: theſe ſour grapes, being by the heat of the ſun concocted and ripened, turn to well taſted grapes; theſe, if dried in the ſun and diſtilled, afford a fœtid oil and a piercing empyreumatical ſpirit, but not a vinous ſpirit; theſe dried grapes or raiſins, boiled in a convenient proportion of water, make a ſweet liquor, which, being betimes diſtilled, afford an oil and ſpirit much like thoſe of the raiſins themſelves; if the juice of the grapes be ſqueezed out, and put to ferment, it firſt becomes a ſweet and turbid liquor, then grows leſs ſweet and more clear, and then affords in common diſtillations not an oil, but a ſpirit, which, though inflammable like oil, differs much from it, in that it is not fat, and that it will readily mingle with water. I have likewiſe without addition obtained in proceſs of time (and by an eaſy way, which I am ready to teach you) from one of the nobleſt ſorts of wine, pretty ſtore of pure and curiouſly figured cryſtals of ſalt, together with a great proportion of a liquor as ſweet almoſt as honey; and theſe I obtained not from muſt, but true and ſprightly wine; beſides the vinous liquor, the fermented juice of grapes is partly turned into liquid drops or lees, and partly into that cruſt or dry feculency, that is commonly called tartar; and this tartar may by the fire be eaſily divided into five differing ſubſtances; four of which are not acid, and the other not ſo manifeſtly acid as the tartar itſelf. The ſame vinous juice after ſome time, eſpecially if it be carefully kept, degenerates into that very ſour liquor called vinegar; from which you may obtain by the fire a ſpirit and a cryſtalline ſalt, differing enough from the ſpirit and lixiviate ſalt of tartar. And if you pour the dephlegmed ſpirit of the vinegar upon the ſalt of tartar, there will be produced ſuch a conflict or ebullition, as if there were ſcarce two more contrary bodies in nature; and oftentimes in this vinegar you may obſerve part of the matter to be turned into an innumerable company of ſwimming animals; which our friend having divers years ago obſerved, hath in one of his papers taught us, how to diſcover clearly without the help of a microſcope.

Into all theſe various ſchemes of matter, or differingly-qualified bodies, beſides divers others, that I purpoſely forbear to mention, may the water, that is imbibed by the roots of the vine, be brought; partly by the formative power of the plant, and partly by ſupervenient agents or cauſes, without the viſible concurrence of any extraneous ingredient. But if we be allowed to add to the productions of this tranſmuted water a few other ſubſtances, we may much increaſe the variety of ſuch bodies; although in this ſecond ſort of productions, the vinous parts ſeem ſcarce to retain any thing of the much more fixed bodies, wherewith they were mingled, but only to have, by their mixture with them, acquired ſuch a diſpoſition, that in their receſs, occaſioned by the fire, they came to be altered as to ſhape, or bigneſs, or both, and aſſociated after a new manner. Thus, as I formerly told you, I did, by the addition of a *caput mortuum* of antimony, and ſome other bodies unfit for diſtillation, obtain from crude tartar ſtore of a very volatile and cryſtalline ſalt, differing very much in ſmell and other qualities from the uſual ſalts of tartar.

BUT (ſays *Eleutherius)* interrupting him at theſe words) if you have no reſtraint upon you, I would very gladly, before you go any further, be more particularly informed, how you make this volatile ſalt; becauſe, you know, that ſuch multitudes of chymiſts have, by a ſcarce imaginable variety of ways, attempted in vain the volatilization of the ſalt of tartar, that divers learned Spagyriſts ſpeak, as if it were impoſſible to make any thing out of tartar, that ſhall be volatile in a ſaline form, or, as ſome of them expreſs it, *in forma ſicca*. I am very far from thinking (anſwers *Carneades)* that the ſalt I have mentioned is that, which *Paracelſus* and *Helmont* mean, when they ſpeak of *ſal tartari volatile*, and aſcribe ſuch great things to it. For the ſalt I ſpeak of falls extremely ſhort of thoſe virtues, not ſeeming in its taſte, ſmell, and other obvious qualities, to differ very much (though ſomething it does differ) from ſalt of hartſhorn, and other volatile ſalts drawn from the diſtilled parts of animals. Nor have I yet made trials enough to be ſure, that it is a pure ſalt of tartar, without participating any thing at all of the nitre, or antimony. But becauſe it ſeems more likely to proceed from the tartar, than from any of the other ingredients, and becauſe the experiment is in itſelf not ignoble, and luciferous enough (as ſhewing a new way to produce a volatile ſalt, contrary to acid ſalts, from bodies, that otherwiſe are obſerved to yield no ſuch liquor, but either only, or chiefly acid ones;) I ſhall, to ſatisfy you, acquaint you, before any of my other friends, with the way I now uſe (for I have formerly uſed ſome others) to make it.

TAKE then, of good antimony, ſalt-petre, and tartar, of each an equal weight, and of quick-lime half the weight of any one of them; let theſe be powdered and well mingled. This done, you muſt have in readineſs a long neck or retort of earth, which muſt be placed in a furnace for a naked fire, and have at the top of it a hole of a convenient bigneſs, at which you may caſt in the mixture, and preſently ſtop it up again. This veſſel being fitted with a large receiver, muſt have fire made under it, till the bottom of the ſides be red hot, and then you muſt caſt in the above prepared mixture, by about half a ſpoonful (more or leſs) at a time, at the hole made for that purpoſe; which being nimbly ſtopt, the fumes will paſs into the receiver, and condenſe there into a liquor, that being rectified will be of a pure golden colour, and carry up that colour to a great height. This ſpirit abounds in the ſalt I told you of, part of which may eaſily enough be ſeparated by the way I uſe in ſuch caſes, which is, to put the liquor into a glaſs egg, or bolt-head with a long and narrow neck. For if this be placed a little inclining in hot ſand, there will ſublime up a fine ſalt, which, as I told you, I find to be much of kin to the volatile ſalts of animals: for like them it has a ſaltiſh, not an acid ſalt; it hiſſes upon the affuſion of ſpirit of nitre, or oil of vitriol; it precipitates corals diſſolved in ſpirit of vinegar; it turns the blue ſyrup of violets immediately green; it preſently turns the ſolution of ſublimate into a milky whiteneſs; and in ſum, has divers operations like thoſe, that I have obſerved in that ſort of ſalts, to which I have reſembled it; and is ſo volatile, that, for diſtinction-ſake, I call it *ſal tartari fugi-*

fugitivus. What virtues it may have in phyſic, I have not yet had the opportunity to try; but I am apt to think they will not be deſpicable. And beſides that, a very ingenious friend of mine tells me, he hath done great matters againſt the ſtone with a preparation not very much differing from ours; a very experienced German chymiſt finding, that I was unacquainted with the ways of making this ſalt, told me, that in a great city in his country, a noted chymiſt prizes it ſo highly, that he had a while ſince procured a privilege from the magiſtrates, that none but he, or by his licence, ſhould vend a ſpirit made almoſt after the ſame way with mine, ſave that he leaves out one of the ingredients, namely the quick-lime. But (continues *Carneades)* to reſume my former diſcourſe, where your curioſity interrupted it;

It is alſo a common practice in *France* to bury thin plates of copper in the marc (as the French call it) or huſks of grapes, whence the juice hath been ſqueezed out in the vine-preſs; and by this means the more ſaline parts of thoſe huſks, working by little and little upon the copper, coagulate themſelves with it into that blueiſh green ſubſtance we in Engliſh call verdigreaſe. Of which I therefore take notice, becauſe having diſtilled it in a naked fire, I found, as I expected, that by the aſſociation of the ſaline with the metalline parts, the former were ſo altered, that the diſtilled liquor, even without rectification, ſeemed by ſmell and taſte, ſtrong almoſt like aqua fortis, and very much ſurpaſſed the pureſt and moſt rectified ſpirit of vinegar, that ever I made. And this ſpirit I therefore aſcribe to the ſalt of the huſks altered by their co-mixture with the copper (though the fire afterwards divorce and tranſmute them) becauſe I found this latter in the bottom of the retort in the form of a crocus or reddiſh powder; and becauſe copper is of too ſluggiſh a nature to be forced over in cloſe veſſels by no ſtronger a heat. And that, which is alſo ſomewhat remarkable in the diſtillation of good verdigreaſe (or at leaſt of that ſort that I uſed) is this, that I never could obſerve, that it yielded me any oil (unleſs a little black ſlime, which was ſeparated in rectification may paſs for oil) though both tartar and vinegar (eſpecially the former) will by diſtillation yield a moderate proportion of it. If likewiſe you pour ſpirit of vinegar upon calcined lead, the acid ſalt of the liquor will by its commixture with the metalline parts, though inſipid, acquire in few hours a more than ſaccharine ſweetneſs; and theſe ſaline parts being by a ſtrong fire diſtilled from the lead, wherewith they were imbodied, will, as I formerly alſo noted to a different purpoſe, leave the metal behind them altered in ſome qualities from what it was, and will themſelves aſcend, partly in the form of an unctuous body or oil, partly in that of phlegm, but for the greateſt part in the form of a ſubtile ſpirit, indowed, beſides divers new qualities, which I am not now willing to take notice of, with a ſtrong ſmell very much other than that of vinegar, and a piercing taſte quite differing both from the ſourneſs of the ſpirit of vinegar, and the ſweetneſs of the ſugar of lead.

To be ſhort, as the difference of bodies may depend meerly upon that of the ſchemes, whereinto their common matter is put; ſo the ſeeds of things, the fire and the other agents are able to alter the minute parts of a body (either by breaking them into ſmaller ones of differing ſhapes, or by uniting together theſe fragments with the unbroken corpuſcles, or ſuch corpuſcles among themſelves) and the ſame agents, partly by altering the ſhape or bigneſs of the conſtituent corpuſcles of a body, partly by driving away ſome of them, partly by blending others with them, and partly by ſome new manner of connecting them, may give the whole portion of matter a new texture of its minute parts, and thereby make it deſerve a new and diſtinct name. So that, according as the ſmall parts of matter recede from each other, or work upon each other, or are connected together after this or that determinate manner, a body of this or that denomination is produced, as ſome other body happens thereby to be altered or deſtroyed.

SINCE then thoſe things, which chymiſts produce by the help of the fire, are but inanimate bodies; ſince ſuch fruits of the chymiſts ſkill differ from one another but in ſo few qualities, that we ſee plainly, that by fire, and other agents we can employ, we can eaſily enough work as great alterations upon matter, as thoſe, that are requiſite to change one of theſe chymical productions into another; ſince the ſame portion of matter may, without being compounded with any extraneous body, or at leaſt element, be made to put on ſuch a variety of forms, and conſequently to be (ſucceſſively) turned into ſo many differing bodies; and ſince the matter, cloathed with ſo many differing forms, was originally but water, and that in its paſſage through ſo many transformations, it was never reduced into any of thoſe ſubſtances, which are reputed to be the principles or elements of mixt bodies, except the violence of the fire, which itſelf divides not bodies into perfectly ſimple or elementary ſubſtances, but into new compounds; ſince, I ſay, theſe things are ſo, I ſee not, why we muſt needs believe, that there are any primogeneal and ſimple bodies, of which, as of pre-exiſtent elements, nature is obliged to compound all others. Nor do I ſee, why we may not conceive, that ſhe may produce the bodies accounted mixt out of one another, by variouſly altering and contriving their minute parts, without reſolving the matter into any ſuch ſimple or homogeneous ſubſtances, as are pretended. Neither, to diſpatch, do I ſee, why it ſhould be counted abſurd to think, that when a body is reſolved by the fire into its ſuppoſed ſimple ingredients, thoſe ſubſtances, are not true and proper elements, but rather were, as it were, accidentally produced by the fire, which by diſſipating a body into minute parts does, if thoſe parts be ſhut up in cloſe veſſels, for the moſt part neceſſarily bring them to aſſociate themſelves after another manner than before, and ſo bring them into bodies of ſuch different conſiſtences, as the former texture of the body and concurrent circumſtances make ſuch diſbanded particles apt to conſtitute; as experience ſhews us (and I have both noted it, and proved it already) that as there are ſome concretes whoſe parts, when diſſipated by fire, are fitted to be put into ſuch ſchemes of matter as we call oil, and ſalt, and ſpirit; ſo there are others, ſuch as are eſpecially the greateſt part of minerals, whoſe corpuſcles being of another ſize or figure, or perhaps contrived another way, will not in the fire yield bodies of the like conſiſtencies, but rather others of differing textures; not to mention, that from gold and ſome other bodies, we ſee not, that the fire ſeparates any diſtinct ſubſtances at all; nor that even thoſe ſimilar parts of bodies, which the chymiſts obtain by the fire, are the elements, whoſe names they bear, but compound bodies, upon which, for their reſemblance to them in conſiſtence, or ſome other obvious quality, chymiſts have been pleaſed to beſtow ſuch appellations.

The CONCLUSION.

THESE laſt words of *Carneades* being ſoon after followed by a noiſe, which ſeemed to come from the place, where the reſt of the company was, he took it for a warning, that it was time for him to conclude or break off his diſcourſe; and told his friend, by this time, I hope, you ſee, *Eleutherius*, that if *Helmont*'s experiments be true, it is no abſurdity to queſtion, whether that doctrine be one, that doth not aſſert any elements in the ſenſe before explained. But becauſe that as divers of my arguments ſuppoſe the marvellous power of the alkaheſt in the analyzing of bodies, ſo the effects aſcribed to that power are ſo unparallelled and ſtupendous, that though I am not ſure, but that there may be ſuch an agent, yet little leſs than ἀυτοψία ſeems requiſite to make a man ſure there is. And conſequently I leave it you to judge, how far thoſe of my arguments, that are built upon alkaheſtical operations, are weakened by that liquor's being matchleſs; and ſhall therefore deſire you not to think, that I propoſe this paradox, that rejects all elements, as an opinion equally probable with the former part of my diſcourſe. For by that, I hope, you are ſatisfied, that the arguments, wont to be brought by chymiſts to prove, that all bodies conſiſt of either three principles, or five, are far from being ſo ſtrong as thoſe, that I have employed to prove, that there is not any certain and determinate number of ſuch principles or elements to be met with univerſally in all mixt bodies. And, I ſuppoſe, I need not tell you, that theſe anti-chymical paradoxes might have been managed more to their advantage; but that having not confined my curioſity to chymical experiments, I, who am but a young man, and younger chymiſt, can yet be but ſlenderly furniſhed with them, in reference to ſo great and difficult a taſk as you impoſed upon me: beſides that, to tell you the truth, I durſt not employ ſome even of the beſt experiments I am acquainted with, becauſe I muſt not yet diſcloſe them. But however, I think I may preſume, that what I have hitherto diſcourſed, will induce you to think, that chymiſts have been much more happy in finding experiments than the cauſes of them; or in aſſigning the principles, by which they may beſt be explained. And indeed, when in the writings of *Paracelſus* I meet with ſuch phantaſtic and unintelligible diſcourſes, as that writer often puzzles and tires his reader with, fathered upon ſuch excellent experiments, as though he ſeldom clearly teaches, I often find he knew; methinks the chymiſts, in their ſearches after truth, are not unlike the navigators of *Solomon*'s *Tarſhiſh* fleet, who brought home from their long and tedious voyages, not only gold, and ſilver, and ivory, but apes and peacocks too: for ſo the writings of ſeveral (for I ſay not, all) of your hermetic philoſophers preſent us, together with divers ſubſtantial and noble experiments, theories, which either like peacocks feathers make a great ſhew, but are neither ſolid nor uſeful; or elſe like apes, if they have ſome appearance of being rational, are blemiſhed with ſome abſurdity or other, that, when they are attentively conſidered, make them appear ridiculous.

CARNEADES having thus finiſhed his diſcourſe againſt the received doctrines of the elements, *Eleutherius* judging he ſhould not have time to ſay much to him before their ſeparation, made ſome haſte to tell him; I confeſs, *Carneades*, that you have ſaid more in favour of your paradoxes than I expected. For though divers of the experiments you have mentioned are no ſecrets, and were not unknown to me; yet beſides that you have added many of your own unto them, you have laid them together in ſuch a way, and applied them to ſuch purpoſes, and made ſuch deductions from them, as I have not hitherto met with.

BUT though I be therefore inclined to think, that *Philoponus*, had he heard you, would ſcarce have been able in all points to defend the chymical hypotheſis againſt the arguments, wherewith you have oppoſed it; yet methinks, that however your objections ſeem to evince a great part of what they pretend to, yet they evince it not all; and the numerous trials of thoſe you call the vulgar chymiſts, may be allowed to prove ſomething too.

WHEREFORE, if it be granted you, that you have made it probable;

FIRST, that the differing ſubſtances, into which mixt bodies are wont to be reſolved by the fire, are not of a pure and an elementary nature, eſpecially for this reaſon, that they yet retain ſo much of the nature of the concrete that afforded them, as to appear to be yet ſomewhat compounded, and oftentimes to differ in one concrete from principles of the ſame denomination in another;

NEXT, that as to the number of theſe differing ſubſtances, neither is it preciſely three, becauſe in moſt vegetable and animal bodies earth and phlegm are alſo to be found among their ingredients; nor is there any one determinate number, into which the fire (as it is wont to be employed) does preciſely and univerſally reſolve all compound bodies whatſoever, as well minerals as others, that are reputed perfectly mixt:

LASTLY, that there are divers qualities, which cannot well be referred to any of theſe ſubſtances, as if they primarily reſided in it and belonged to it; and ſome other qualities, which, though they ſeem to have their chief and moſt ordinary reſidence in ſome one of theſe principles or elements of mixt bodies, are not yet ſo deducible from it, but that alſo ſome more general principles muſt be taken in, to explicate them:

IF, I ſay, the chymiſts (continues *Eleutherius)* be ſo liberal as to make you theſe three conceſſions, I hope you will, on your part, be ſo civil and equitable, as to grant them theſe three other propoſitions, namely;

FIRST, that divers mineral bodies, and therefore probably all the reſt, may be reſolved into a ſaline, a ſulphureous, and a mercurial part; and that almoſt all vegetable and animal concretes may, if not by the fire alone, yet by a ſkilful artiſt employing the fire as his chief inſtrument, be divided into five differing ſubſtances, ſalt, ſpirit, oil, phlegm and earth; of which the three former, by reaſon of their being ſo much more operative than the two latter, deſerve to be looked upon as the three active principles, and by way of eminence to be called the three principles of mixt bodies.

NEXT, that theſe principles, though they be not perfectly devoid of all mixture, yet may without inconvenience be ſtiled the elements of compounded bodies, and bear the names of thoſe ſubſtances, which they moſt reſemble, and which are manifeſtly predominant in them; and that eſpecially for this reaſon, that none of theſe elements is diviſible by the fire into four or five differing ſubſtances, like the concrete, whence it was ſeparated.

LASTLY, that divers of the qualities of a mixt body, and eſpecially the medical virtues, do for the moſt part lodge in ſome one or other of its principles, and may therefore uſefully be ſought for in that principle ſevered from the others.

AND in this alſo (purſues *Eleutherius)* methinks both you and the chymiſts may eaſily agree, that the ſureſt way is to learn by particular experiments, what differing parts particular bodies do conſiſt of, and by what ways (either actual or potential fire) they may beſt and moſt conveniently be ſeparated, as without relying too much upon the fire alone, for the reſolving of bodies, ſo without fruitleſsly contending to force them into more elements than nature made them up of, or ſtrip the ſevered principles ſo naked, as by making them exquiſitely elementary, to make them almoſt uſeleſs.

THESE things (ſubjoins *Eleutherius)* I propoſe, without deſpairing to ſee them granted by you; not only becauſe I know, that you ſo much prefer the reputation of candour before that of ſubtilty, that your having once ſuppoſed a truth would not hinder you from imbracing it, when clearly made out to you; but becauſe, upon the preſent occaſion, it will be no diſparagement to you to recede from ſome of your paradoxes, ſince the nature and occaſion of your paſt diſcourſe did not oblige you to declare your own opinions, but only to perſonate an antagoniſt of the chymiſts. So that (concludes he, with a ſmile) you may now, by granting what I propoſe, add the reputation of loving the truth ſincerely to that of having been able to oppoſe it ſubtilely.

CARNEADES's haſte forbidding him to anſwer this crafty piece of flattery; till I ſhall (ſays he) have an opportunity to acquaint you with my own opinions about the controverſies I have been diſcourſing of, you will not, I hope, expect I ſhould declare my own ſenſe of the argument I have employed. Wherefore I ſhall only tell you thus much at preſent; that though not only an acute naturaliſt, but even I myſelf could take plauſible exceptions at ſome of them; yet divers of them too are ſuch as will not perhaps be readily anſwered, and will reduce my adverſaries, at leaſt, to alter and reform their hypotheſis, I perceive I need not mind you, that the objections I made againſt the quaternary of elements and ternary of principles, needed not to be oppoſed ſo much againſt the doctrines themſelves; either of which, eſpecially the latter, may be much more probably maintained than hitherto it ſeems to have been, by thoſe writers for it (I have met with) as againſt the unaccurateneſs and the unconcludingneſs of the analytical experiments vulgarly relied on to demonſtrate them.

AND therefore, if either of the two examined opinions, or any other theory of elements, ſhall, upon rational and experimental grounds, be clearly made out to me; it is obliging, but not irrational, in you to expect, that I ſhall not be ſo far in love with my diſquieting doubts, as not to be content to change them for undoubted truths. And (concludes *Carneades* ſmiling) it were no great diſparagement for a ſceptic to confeſs to you, that as unſatisfied as the paſt diſcourſe may have made you think me with the doctrines of the Peripatetics, and the chymiſts, about the elements and principles, I can yet ſo little diſcover what to acquieſce in, that perchance the enquiries of others have ſcarce been more unſatisfactory to me, than my own have been to myſelf.

EXPERIMENTS
AND
NOTES
ABOUT THE
Producibleneſs of Chymical Principles;
Being Parts of an Appendix, deſigned to be added to the SCEPTICAL CHYMIST.

The Author's Preface.

HAVING long ſince obſerved, that a great part of the erroneous reaſonings and concluſions of learned men, as well about phyſical, as other ſubjects, proceeds not ſo much from their making bad illations, as from their aſſuming falſe or uncertain principles, to draw their conſequences from; I thought, I could ſcarce miſſpend the time I allowed myſelf for chymical ſtudies, if I employed ſome part of it, in examining the doctrine about the principles of natural bodies. Upon this account I did, in the year 1661, venture abroad my *Sceptical Chymiſt*, to acquaint the inquiſitive with my doubts, and excite them to a more thorough diſquiſition of a ſubject, ſo conſiderable, as well to natural philoſophy, as to phyſic. This diſcourſe being once publiſhed in Engliſh, and ſoon after in Latin; I thought fit to wait a while, that I might learn what judgment would be made of it, and whether any of the chymiſts would return an anſwer to it: and in the mean while, to gratify thoſe, that appeared deſirous of having it ſoon reprinted, I gathered divers notes (ſome of them conſiderable for bulk) to be inſerted here and there, as inlargements in the next edition, whoſe volume I was not unwilling ſomewhat to increaſe, not only becauſe I thought truth in general, a thing worthy, that the lovers of it ſhould take pains to diſcover, and eſtabliſh it, but becauſe I looked upon the truth inquired after in the *Sceptical Chymiſt*, as of no mean importance; eſpecially ſince the miſtakes, that very many have made about it, have, I fear, not only been prejudicial to natural philoſophy, but have, by ſeveral men, as well learned as ignorant, been adopted both into the ſpeculations, and practice of phyſicians; whoſe art being converſant about the health and life of man, doctrinal errors in it cannot but be dangerous, and therefore fit, as much as is poſſible, to be ſolicitouſly avoided, or removed. Theſe inconveniencies I hoped might in ſome meaſure be obviated, if it were further made appear by experiments as well as reaſonings, that the vulgar doctrine of the *tria prima* is, at leaſt, very queſtionable, or uncertain and very narrow. For the contrary perſuaſions about theſe principles have miſled divers learned men to give, and take up with precarious and ſuperficial accounts of divers phænomena of nature; by which means they have been diverted from employing their wits (wherein divers of them are happy) in the inveſtigation of the true and fundamental cauſes, the diſcovery whereof would have enabled them, inſtead of dark and ſuperficial, to give intelligible and particular explications of thoſe phænomena, and many others. The difference between the accounts given of the ſame phænomena, by the hypoſtatical and by the mechanical principles, may be ſeen exemplified by particular inſtances in other papers; wherefore, I ſhall proceed to obſerve as to phyſic, that beſides the miſtakes, which, I doubt, divers learned men have, by another valuation of the doctrine of the *tria prima*, been led into, in relation to the cauſes of divers things, that occur to human bodies, and even in chymical operations; beſides this, I ſay, I fear, that the too confident opinion of the doctrine I queſtion has made divers practitioners of phyſic make wrong eſtimates of medicines. But after I had waited a competent time, I perceived no author vouchſafed the *Sceptical Chymiſt* an anſwer; but a very ingenious man, from whom I chiefly expected it, told me, that he had indeed deſigned to write one, but was hindered by conſidering, that I had ſo ſtated the caſe, that an anſwer could not confute that book by any mere juſtification of the chymiſts principles, ſince he would be obliged alſo to defend the chymical doctrine, as it is generally taught by the vulgar chymiſts; and make good the arguments, by which they are wont to maintain it. Since it is only that doctrine and theſe arguments, that I declare myſelf in that diſcourſe to queſtion; and he himſelf did not think them ſound and valid. By theſe encouragements I was induced to comply with the earneſt ſolicitations of the printer for another edition; but he dying ſoon after, and the perſon, to whom the right to diſpoſe of the Engliſh copy legally came, having left *England*, and continued out of it, for divers years, the diſpute between the Stationers that pretended to it, and treated about it, laſted ſo long, that a traveller, who paſſed this way, told an acquaintance of mine, that he had then (which was two or three years ago) ſeen nine ſeveral Latin impreſſions of it; ſince when another has been brought me made at *Geneva*. This number of editions (in none of which I have added or altered a word) and the numerous citations I have met with of it, in favourable writers, made me unwilling to confound, or trouble readers, by interweaving additional notes with the body of the diſcourſe; and ſo by obliging thoſe that ſhould hereafter vouchſafe to mention any of the inſerted paſſages of it, to cite the edition as well as the book. And therefore I was eaſily inclined, by want of health and leiſure, to peruſe again deliberately the whole treatiſe, to ſuppreſs all thoſe notes, that I could not readily and conveniently refer to three or four of the chief heads, I intended to enlarge upon; and without altering the form of the book, wherein it has proved ſo fortunate, to leave it intire, and publiſh my additions alſo by themſelves by way of appendix. This in my intention was to conſiſt of four heads, The *Producibleneſs of chymical Principles*; The *Uncertainty of the vulgar Analyſes made by Diſtillation*; The *various Effects of the Fire according to the differing ways of employing it*, and *Doubts whether there be any Elements, or material Principles of mixt Bodies, one or more, in the ſenſe vulgarly received*. But finding by the Stationer's eſtimate, that the notes referred to the three laſt titles, are not near ſo large as thoſe that belong to the firſt; yet they would make the book, to which they ſhould be added, and which is already printed, of too great a thickneſs in proportion to its other dimenſions; I thought fit to reſerve the other papers for another opportunity, and at this time annex nothing, but what concerns *the Producibleneſs of the chymical Principles.*

But yet becauſe there are ſome general advertiſements that do ſomewhat more belong to this part of our deſigned Appendix, that now comes forth, than to any of the reſt, I muſt not deny them a room in this preface, which I ſhall conclude with them.

I might juſtly enough allege, in excuſe of incoherence of ſome of the particulars, that follow next after one another in the ſubſequent diſcourſe, that this being confuſedly but a collection (or if you pleaſe a rhapſody) of looſe notes, it is more pardonable, than ſtrange, that ſome of them ſhould want apt connections, and the ſtyle of the diſcourſe they compoſe ſhould want uniformity. But it is not ſo much my preſent work to make apologies, as to give advertiſements; and therefore I ſhall proceed to tell you in the firſt place, that though the following diſcourſe have in ſome places a ſomewhat dogmatical dreſs, yet it is chiefly meant (as becomes an appendix to a ſceptical book) to excite and aſſiſt a further inquiry; and accordingly the reader may perceive it to have been my care, not ſo much to play the part of a logical opponent, as to take occaſion to ſet down variety of experiments and obſervations, that whatever hypotheſis about the material principles of mixed bodies ſhall prove fit to be pitched upon, it may be founded on a leſs inſufficient hiſtory of matters of fact (relating to that ſubject) than chymiſts have been wont to take in; and may be ſo framed, as not to be liable to thoſe objections and difficulties, that will be here met with, and yet perhaps were not thought of, or at leaſt were not duly taken into conſideration, when the vulgar hypotheſis of the *tria prima* was eſtabliſhed. Upon this account I am not without hope, that the following experiments and conſiderations, though propoſed by way of objections, may do ſome ſervice to the inquirers into the material principles of things; by obliging the chymiſts, at leaſt, to reform their doctrine about them, and build it more cautiouſly, and that upon a larger, as well as more ſolid foundation of natural hiſtory.

The ſecond thing, whereof I am to advertiſe the reader, is, that I would not have him infer from any thing, that (prompted by the exigencies of my deſign) I have ſaid in the following papers, that I do either undervalue, or would decry chymiſtry, or chymiſts themſelves indiſcriminately. For I have a very differing eſteem of the notional and of the practical part of chymiſtry. For divers of the opinions maintained by Spagyriſts, without excepting their grand hypotheſis of the three principles, I have been inclined to queſtion, not only as a naturaliſt, but as a chymiſt; as ſeeing great cauſe to doubt, whether they be agreeable, either to the true grounds of philoſophy, or the exploring experiments of the fire. But as for chymical operations, ſuch as diſtillation, ſolution, ſublimation, precipitation, and the reſt; eſpecially thoſe ſeldom ſufficiently valued ones, digeſtion and cohobation; I take them to be excellent tools in the hands of a natural philoſopher, and to be by him applicable to many other, and perhaps ſome nobler uſes, than they are wont to be put to, in laboratories; ſince if they be ſkilfully employed, they may be ſucceſsfully ſo, as well to diſcover nature, as to correct, to imitate, and, in ſome caſes, to out-do her. Nor do I only thus diſtinguiſh between the ſpeculative and operative part of chymiſtry, but I make a great difference between the avowed cultivators of that art; and look not with the ſame eyes on the opinions and performances of vulgar chymiſts, and chymical philoſophers. For we are told, that there lives concealed in the world a ſett of Spagyriſts of a much higher order than thoſe, that are wont to write courſes of chymiſtry or other books of that nature; being able to tranſmute baſer metals into perfect ones, and do ſome other things, that the generality of chymiſts confeſs to be extremely difficult, and divers of the more judicious, even among the Spagyriſts themſelves, have judged impoſſible. The declaration of what I think of theſe latent philoſophers, belongs to another paper. Yet in this, I ſhall not deny, but that what I have heard from divers very credible eye-witneſſes, and perhaps ſome more immediate arguments, ſtrongly incline me to think, that there may have been, and may yet be, ſome ſuch men: and whatever be to be thought of what they call the philoſopher's ſtone, I confeſs myſelf convinced by what I have ſeen, that there are in the world as difficult arcana as divers of thoſe, which have been (perhaps not all of them juſtly) derided under the name of chymical *non-entia.* Now if there be really ſuch adept philoſophers as we are told of; I am apt to think, that among their other arcana, they are maſters of extremely potent menſtruums (which may, as far as I can gueſs, be ſome of their chiefeſt tools) and may by the help of theſe and other means peculiar to themſelves, of working upon bodies, be able to produce in them ſuch alterations, as we have no examples of, and ſo obtain from them ſuch ſimilar ſubſtances, as either for number, or quality, or both, may be very different from the vulgar *tria prima*, or thoſe ſubſtances chymiſts are wont to obtain (for that word I chuſe to employ rather than the word ſeparate or extract) by the common ways of what they call analyſis. For if a man have an inſtrument, which other men have not, and much more, if it be an excellent one, he may be able with it to perform other things, than they can without. The Europeans by the help of ſo ſlight an engine as a mill, aſſiſted by a far ſlighter inſtrument a ſieve, can eaſily divide corn into bran, and meal, and flour, which even thoſe Americans, for want of thoſe helps, were not able to do, who could do other things, that are thought far more difficult. And he, that has a file and a good turning lath with its appurtenances, may obtain from a piece of iron both filings and ſhavings, and concave hemiſpheres, and ellipſes, and globes, and cylinders, and other ſorts of bodies, which could not be obtained from that iron, even by good artificers, that were not furniſhed (as till of late very few were) with thoſe inſtruments. And he, that firſt found the uſe of aqua fortis in diſſolving ſilver, and that though it were mixt with gold, had, by his menſtruum, an eaſy way of ſeparating thoſe two metals, though ancienter mineraliſts, nor chymiſts themſelves had no liquor that would perform that work. But *Helmont*'s writings will ſupply me with a far nobler inſtance to my preſent purpoſe, if the truth of all that he delivers concerning the effects of his alkaheſt be admitted; about the poſſibility of which ſtrange ſolvent, having elſewhere written a ſhort enquiry, I ſhall forbear to ſay any thing of it here, but rather intimate, that if there be ſuch adept philoſophers as ſome ſpeak of, (which I think not incredible) and if they have (which, ſuppoſing there be ſuch, I think not unlikely) among other rare things, ſome alkaheſtial or other extraordinarily potent menſtruum, or way of penetrating and working upon mixt bodies; they may, for aught I know, be able to obtain ſuch ſubſtances from them, as may induce me, and perhaps the chymiſts too, to entertain other thoughts about the conſtitution of compounded bodies (as they are wont to be called) than either I, or they now have. And therefore, though as to natural philoſophy in general, I do not expect to ſee any principles propoſed more comprehenſive and intelligible than the corpuſcularian or mechanical; yet as to the ſubordinate theory of mixt bodies in particular, I, that have diſputed only againſt the vulgar hypotheſis of the chymiſts, can eaſily retain a diſpoſition to receive further light in this matter, when thoſe, that are the beſt able to afford it us, and from whom it will be no diſparagement for much greater proficients than I, to learn, ſhall think fit to oblige us by doing ſo. In the mean time, to end this advertiſement as I begun it; I ſhould not need to ſay much to ſatisfy chymiſts, that I neither hate nor deſpiſe their art, even in its preſent ſtate, if ſome things, and chiefly want of leiſure, would permit me to publiſh an eſſay that I wrote many years ſince, *Of the Uſefulneſs of Chymiſtry to the Empire of Man.* Nor is it only to the practical part of natural philoſophy, that I take chymiſtry, as it may be managed, to be highly uſeful, but I confeſs, I think alſo, that being ordered by a ſkilful naturaliſt, it may far more conduce, than thoſe, that are ſtrangers to it are wont to think, to the ſpeculative part of phyſicks; and that as the Bolonian ſtone, without being chymically prepared, would never be made luminous, but being ſo prepared is brought to ſhine; ſo, many other natural bodies never afford much light to philoſophy till chymical operations have qualified them to do ſo.

The laſt advertiſement I deſire to give the reader, concerns the intention, with which I call in queſtion the hypotheſis of the *tria prima*, and ſome other of the chymiſts doctrines. For though ſometimes I have had occaſion to diſcourſe like a Sceptick, yet I am far from being one of that ſect; which I take to have been little leſs prejudicial to natural philoſophy than to divinity itſelf. I do not with the true Scepticks propoſe doubts to perſuade men, that all things are doubtful and will ever remain ſo (at leaſt) to human underſtandings; but I propoſe doubts not only with deſign, but with hope, of being at length freed from them by the attainment of undoubted truth; which I ſeek, that I may find it; though if I miſs of it in one opinion, I proceed to ſearch after it in the oppoſite, or in any other where it ſeems more likely I ſhould meet with it. And to declare my mind to the diſciples of the fire, by a ſimilitude not alien from their profeſſion; ſuppoſe a man, more rich than ſkilful, ſhould bequeath me a purſe of guineas, and that I ſhould have ſtrong preſumptions, that ſome of them are counterfeit, what in this caſe would a chymiſt have me do? To take them all for good, in ſpite of contrary preſumptions againſt ſome of them, were very imprudent. On the other ſide, to throw them all away, becauſe it is probable ſome may prove counterfeit, were downright folly. That then, which common prudence would direct me, would be to take them all out, and examine them one by one, firſt with the touch-ſtone, and then, if need be, by the cupel, and by aqua fortis too: and this I ſhould do with deſire to find all the pieces true, having alſo care not only to preſerve and put back into the purſe thoſe that prove right; but if any be but partly adulterated, to preſerve the good portion by purifying it (by the cupel or ſome other fit way) from the falcifying alloy, by whoſe admixture it had been imbaſed. The application of this I leave to be made by chymiſts. And having, in another paper, purpoſely diſcourſed of the cautions and limitations, without which I diſallow ſcepticiſm, I ſhall only in general profeſs, that I more willingly embrace the truths taught by the chymiſts, than I endeavour to diſprove their errors. For I look upon truth, as one of the chief of thoſe goods, that God has of all others laid the moſt in common; ſince truth does not only, like deſert iſlands in *America*, belong to him, that firſt finds it and ſeizes on it; but even, when another has lighted on it, and is in poſſeſſion of it, any man may, without treſpaſs or injury, make himſelf a ſharer in it. To conclude; I am glad to find truth in the doctrines of the chymiſts; but when I cannot diſcern it there, I chuſe rather to ſeek it elſewhere, than ſit down without it. And if I any where ſeem to be ſomewhat too indulgent to ſuſpicions againſt their hypotheſis, or arguments, I hope the uſual confidence, to be met with among moſt of them, conſidered, it will be looked upon but as a compliance with the advice of *Ariſtotle*, of bending a crooked ſtick the contrary way, to reduce it at length to ſtraitneſs. And I did, with the leſs ſcruple, allow myſelf this way of writing, becauſe experience having taught me, that ſome Spagyriſts (for I ſpeak not of all) that keep their beſt things cloſe, will do more to vindicate their art, or oppoſe their antagoniſts, than to gratify the curious, or benefit mankind; I thought the rouſing ſtyle, I ſometimes wrote in, might prove no unhopeful way to procure ſomewhat conſiderable from thoſe great maſters, and orders of chymical arcana, that muſt be provoked, before they will come out with them; as the ſea is obſerved not to give us one of its preciouſeſt treaſures, ambergreaſe, till it have been agitated by winds and ſtorms.

The Publiſher's Advertiſement to the Reader.

I SHALL not entertain the reader with any thoughts of my own, about the following Appendix, which, without deſiring to prepoſſeſs him, I ſhall willingly (and I think may ſafely) leave to ſpeak for itſelf. But yet I think it may not be amiſs, if I premiſe ſomething to the reader, about the publication of theſe notes, as having been particularly concerned in it.

By the opportunity I had of ſeeing ſome papers of the honourable author of the enſuing Appendix; I perceived, that the notes, which he deſigned it ſhould conſiſt of, were indeed moſt of them laid together in ſome (though but a careleſs) order, and ſo were without much difficulty fitted for the preſs: but others of them lay ſcattered up and down amongſt many others, about differing ſubjects in his philoſophical memorials, which particulars not being ready at hand, when the enſuing Notes were ſent to *Oxford* to the printer, they could not be publiſhed with the reſt, but muſt expect ſome other opportunity to appear abroad, either alone, or in their company.

Perhaps the reader will not need to be told, that beſides the application of ſome of the experiments contained in the following Notes, moſt of the experiments themſelves are new. But ſo many years are paſt betwixt the firſt edition of the *Sceptical Chymiſt*, and the ſecond, that now comes forth; that it may be requiſite (though otherwiſe it would be improper) to advertiſe this reader, that he is not to think, that the author has borrowed from others thoſe experiments and notions, that may be met with in books written in later years, as well as in the *Sceptical Chymiſt*. For the firſt Engliſh edition having been put forth in the year 1661, and never ſince by the author at all enlarged, or altered; it will ſufficiently ſhew, that this book could not borrow from thoſe, that never were ſeen till after, and perhaps long after his was publiſhed. Which advertiſement may be particularly applied to the late learned treatiſe, intitled *Philoſophia Vetus & Nova*, wherein in one long chapter may be met with an abridgement of a great part of the notions, experiments, and ratiocinations of the *Sceptical Chymiſt*, without any mention there made, either of the great and famous author's name, or his book, in which they firſt appear; though the Latin verſion of that treatiſe was publiſhed many years ago, and reprinted many times ſince. And though this be not the only writer, that hath thought fit to make uſe of conſiderable portions of the *Sceptical Chymiſt*, without owning it, I thought, what he has been pleaſed to do, required to have particular notice taken of it, becauſe, though his modeſty hath perſuaded him to conceal his name, his learned book hath made him ſo juſtly famous, that if the reader were not advertiſed, he might eaſily ſuſpect, that Mr. *Boyle* had not lent to, but borrowed of an author, who appears ſo capable of enriching the curious with excellent things of his own. And upon the ſame grounds I think it neceſſary to obſerve, that the experiments to be met with in Mr. *Boyle's Hiſtory of Colours* having been publiſhed many years ago, could not be borrowed from that moſt ingenious treatiſe, though in that chapter of it, which treats *de coloribus*, between 20 and 30 experiments (if I miſ-remember not the number) will be found the ſame with the like number of Mr. *Boyle*'s; whoſe name, though elſewhere very civilly taken notice of on ſome other occaſion, is in the whole chapter left unmentioned.

I MIGHT here inform the reader, that the *Sceptical Chymiſt* having been many years out of print, it chanced, that when the notes, that make up the following Appendix, were drawn together for the preſs, this author had not a book at hand, by comparing whereof with the particulars of his deſigned Appendix, he might be ſure to avoid (what he now but hopes he hath) the ſuffering any thing to paſs in the latter, that is truly coincident with what was already extant in the former: (I mean, to the ſame purpoſe, and on the ſame occaſion; for otherwiſe an experiment or notion may be more than once employed without mere repetition.)

AND laſtly I dare not omit to let the reader know, that ſince the Appendix was printed, it appears, that by an overſight, ſome leaves were left behind, that treating of the difference of common mercuries themſelves, ſhould have been annexed, as a kind of appendix to the laſt of the three mercurial tracts, to be met with among the following papers: from whoſe peruſal the reader ſhall no longer be detained by

His Humble Servant,

I. M.

The Introduction to the following NOTES.

THOUGH the pompous title of *Hypoſtatical Principles*, which chymiſts have beſtowed upon the ingredients they would have mixt bodies to conſiſt of, has perhaps ſerved to procure them a veneration from vulgar heads, that are wont to eſteem things the more becauſe they underſtand them leſs; yet the main thing, that has recommended the chymical principles to more diſcerning men, ſeems to be, that by the help of a few ſimple ingredients (whereof nature is ſuppoſed to have laid up great magazines at the beginning of things) aſſociated in differing proportions, all mixt bodies may be compounded; and ſo men may acquaint themſelves with the natures of a multitude of bodies, by firſt knowing the natures but of a few. He therefore, that acknowledges he does not acquieſce in the chymical hypotheſis of the *tria prima*, or their's, that add to them water and earth, can ſcarce imploy a more proper argument to ſhake it, than, upon good ground, to call in queſtion what they teach when they affirm, that their principles are ingenerable and incorruptible; and that nature does only compound and diſſociate them, without either producing or deſtroying any of them. It will be therefore very well worth while to examine, what evidence there is in an aſſertion, which, in ſo many of the chymiſts reaſonings and explications, is either manifeſtly imployed, or not obſcurely ſuppoſed. And indeed this tenet of theirs is ſo principal a pillar of their hypotheſis, that, in caſe it fail them, the whole ſtructure will be in danger of ruin. For if the bodies they call principles be produced *de novo*, how will it be demonſtrable, that nature was obliged to take thoſe principles made ready to her hand, when ſhe was to compound a mixt body? and how will it appear in every analyſis made by fire, that the ſalt (for inſtance) thereby obtained, was not produced by the chymical operations, but was was pre-exiſtent in the body in minute parts, which, by the action of the fire, were only extricated and ſeparated from the other principles or ingredients, and afterwards brought together? Since, in caſe the chymical ſuppoſition be erroneous, not only the obtained ſalt may be in part due to a new production or tranſmutation, but part of that, which was really ſalt, if any ſuch thing there were antecedently to the analyſis, might be either deſtroyed by the operation, or made to appear under ſome other form.

OF THE

Producibleneſs of Chymical Principles.

PART I.

Of the Producibleneſs of Salt.

AMONG the ſubſtances, upon which chymiſts have conferred the title of principles, ſalt ſeems in their eſtimate to have had the precedency, ſince they are wont to name it firſt in the enumeration of their *tria prima*. And it is generally granted, that ſalts are wont to be the moſt conſiderable and active parts obtained by chymiſts from mixt bodies. And yet perhaps the inviſible particles, that compoſe the viſible portions of a ſalt, may be ſuch, and ſo contrived, as to be fit to make, and to have perhaps actually made, other portions of matter endowed with thoſe qualities, for which chymiſts are wont to call a body ſulphureous or mercurial, as may be inſtanced in the inflammability of nitre. Wherefore it may deſerve a greater meaſure of curioſity, than ſeems to have been employed, or even deſigned, by vulgar chymiſts, to enquire, whether ſalt, indefinitely ſpeaking, may be produced *de novo*, (as they phraſe it) or deſtroyed; and whether at leaſt the particular, and much differing ſpecies of ſalts may be changed into one another, and thereby, after a manner, be produced in reference to the acquired ſpecies of ſalt, and deſtroyed in relation to that, which the ſame portion of matter belonged to before.

To premiſe ſomewhat in general, to render it probable, that ſalts may be produced *de novo*, I ſhall briefly repreſent two things; the firſt is, that ſince ſalts differ much in ſeveral other attributes, ſome being mixt, ſome volatile, ſome acid, ſome urinous, *&c.* the two qualities, wherein they agree, and which therefore make up the common and moſt received notion of ſalt in general, are, that it is eaſily diſſoluble in water, and that it affects the palate with a ſapor, whether good or evil: and the other thing is, that, whether we allow the Epicurean hypotheſis, or the Carteſian, the firſt ſaline concretions, that were produced by nature, muſt be confeſſed to have been made of atoms, or of particles, that before their conjunction, were not ſaline. And therefore there appears no abſurdity in conceiving, that by the action of the fire or other fit agents, ſmall portions of matter may be ſo broken into minute parts, and theſe fragments may be ſo ſhaped and connected, as, when they are duly aſſociated, to compoſe a body capable of being diſſolved in water, and of affecting the organs of taſte.

THAT a diſpoſition to be diſſoluble in this or that liquor may be acquired by mixture, and the new texture of parts, is not without example; for, as I elſewhere obſerve, though powdered ſulphur will lie in well rectified ſpirit of wine, ſome weeks or months, without being at all viſibly diſſolved in it; and though the ſame liquor will for as long a time ſwim upon ſalt of tartar, without making a ſolution of it; yet if this ſalt and ſulphur be mixt together, ſpirit of wine will in leſs than an hour, and ſometimes in leſs than a quarter of that time, diſſolve enough of this matter to be richly coloured by it, and this without the help of external heat. And I ſee not, why it ſhould be impoſſible, that the action of the fire may reduce the corpuſcles of bodies to ſuch a minuteneſs, and aſſociate them either among themſelves, or with the corpuſcles of other bodies, which, without preparation, will not diſſolve in water, that the pores intercepted between them may be entered, and their looſe texture diſſolved by that menſtruum. Of which conjecture though we have not a perfect inſtance, yet we have a probable one in that, which I ſhall hereafter deliver concerning the making of fixt nitre. For though the cryſtals of well purified ſalt-petre may be kept many weeks or months in an ordinary lodging-chamber (for I had not occaſion to try it in a cellar) without relenting by the moiſture of the air; yet if without the addition of any body diſſoluble in water, or moiſt air, it be in great part reduced, as perhaps it may be almoſt in a trice, to a fixt alkali, this ſalt will be eaſily enough penetrable by the vapours, that rove up and down in the air; and will by that moiſture, in no long time, be brought to relent, and at length will be reſolved into a liquor very analogous to that, which the chymiſts make of ſalt of tartar left in moiſt cellars to deliquate.

As for the ſapor, which is the ſecond qualification to be conſidered in the vulgar notion of a ſaline body, I doubt, whether the neceſſity of it be agreeable to another principle of theirs, and to experience. For it is plain, that chymical oils, even thoſe pure ones they call *eſſential*, or even ethereal ones, are highly ſapid: and yet theſe not diſſolving in water, it ſeems there is no ſtrict connection betwixt being ſaporous, and being ſoluble in that liquor; and that, if bodies be reduced into a multitude of parts minute and ſharp enough, it is very poſſible, that ſome of theſe, either in part, or in conjunction with others, may acquire a ſize and ſhape, that fits them ſenſibly to affect the organ of taſte, though perhaps the bodies themſelves, or perhaps thoſe bodies, that afforded them, are more of ſome other nature than of a ſaline. This may be illuſtrated by theſe groſs examples; that a ball of glaſs, for inſtance, though, whilſt it is entire, it will not prick and hurt the ſkin, yet if it be broken and beaten, the little fragments will, not as they are glaſs, but as they have points or edges. And ſo, though a ſtick, being graſped in a man's hand, will not pierce the ſkin, or put him to pain; yet if it be cut into tooth-picks or reduced to ſplinters, their ſharpneſs and ſtiffneſs gives them a power to wound, that they had not before. Something analogous to what we, in theſe examples, ſee to be done upon the organs of touch, may be conceived to be done upon the organs of taſte (which is a kind of touch) of which an almoſt full inſtance may be given in purified ſalt-petre. For though this concrete have but a faint and languid taſte, yet if it be carefully diſtilled with ſome additament, that is not diſſoluble in water, and is inſipid, the parts of it being, by the action of the fire, either broken aſunder, or cleft, or rubbed, or ground againſt one another, till they are reduced to edged and pointed corpuſcles; ſalt-petre, I ſay, thus treated will be reſolved into differing ſubſtances, each of which has an extremely ſtrong and penetrant taſte, which, whence it ſhould proceed but from ſome ſuch mechanical change, as we have been deſcribing, is not eaſy to declare: and perhaps alſo the phlegmatic liquor, that is wont to come over in this analyſis, may, at leaſt as to part of it, be produced by the operation of the fire, and ſo the phlegm being inſipid, the taſte, I mean as much as was in the unanalyzed nitre, may be as well deſtroyed, as thoſe of the ſpirit and alkali are generated by the operation of the fire. But perhaps there needs no other argument, that the ſame parts of matter, according to its differing ſtates, may have the qualities, that chymiſts would have to be proper to this or that principle, than what we have took notice of in chymical oils, which do more ſtrongly affect the taſte, than the moſt of ſalts themſelves are found to do. And to confirm our doctrine of the produciblenеſs of ſalts by the authority of *Helmont*, which is very great, at leaſt with the chymiſts of his own ſect, I ſhall obſerve, that he aſſures us, that by *Paracelſus*'s *ſal circulatum* ſolid bodies, among which he particularly, and in the firſt place, inſtances ſtones, may be tranſmuted into actual ſalt equiponderant to the body whereof it was made. So that upon the chymiſts ſuppoſition, that in theſe mixt bodies there is both ſulphur and mercury, beſides a *terra damnata*, the ſame portions of matter, that preexiſted in the form of either of thoſe ſimple ingredients, muſt, by the operation of the fire, and an anomalous menſtruum, have been turned into ſalt; and if the Helmontian experiment be allowed of, whatever becomes of the chymical ſuppoſition, we may ſafely conclude, that ſalt may be made of matter, that was not ſalt before, and conſequently that ſalt may be *de novo* produced. And thus much of the poſſible origination of ſalts in general, which I thought fit to premiſe to what I am going to offer about the production of the particular ſorts of ſalt. Though I have elſewhere enumerated and diſtinguiſhed ſeveral kinds of theſe bodies, whereto chymiſts have given the title of ſalts; yet thoſe, that more properly deſerve that name, and more directly appertain to our preſent diſquiſition, ſeem to be chiefly theſe three: the acid, ſuch as vinegar, ſpirit of ſalt, *&c.* the alcalizate or fixed lixiviate ſalts, made by burning, ſuch as ſalt of tartar, and of wormwood, barillia, pot-aſhes, *&c.* and the volatile and urinous ſalts, ſuch as ſalt of hartſhorn, of urine, of blood, of ſoot, *&c.* which taſte and ſmell like that of urine. Wherefore, if we can ſhew, that theſe may be produced *de novo*, or (which we have intimated to be equivalent for our purpoſe) tranſmuted into one another, we ſhall, I hope, be thought to have ſucceeded in our preſent attempt.

Vide Helmontium in tractatu dict. Elementa, No 11. & *alibi*.

SECT I.

Of the production of acid ſalts.

AND to begin with acid ſalts, we ſee, that even ſweet wines will but too often, without addition, degenerate into ſour vinegar, which will diſſolve coral and divers ſtones, calcined lead, and ſeveral other minerals. The rain-water, that is imbibed by the roots of trees, is in thoſe, that bear limons and barberries changed into liquors, abounding with ſaline corpuſcles, that enable them to affect the taſte, and act on powdered pearls, and ſeveral other bodies, as acids are wont to do. Alſo guaiacum and divers other woods, that do not at all taſte ſour, will, being diſtilled in retorts, afford ſpirits, that are furniſhed with ſtore of acid particles, which, as I have tried, will hiſs upon alcalies, and will diſſolve coral, and even lead itſelf calcined into minium, and make *ſaccharum Saturni* of it. Many other vegetable bodies alſo do, without addition, afford the like acetous liquors. And if it be objected, that theſe were pre-exiſtent in the bodies, whence they were obtained, and were only extricated by the operation of the fire; it will concern thoſe, that affirm this, to prove it (which no body, that I have met with, hath yet done): and I ſhall the rather require it, becauſe I find, that the ſweeteſt bodies, and thoſe of differing kinds, as (to omit raiſins of the ſun) ſugar and honey themſelves, afford ſuch a ſort of ſpirits, which the trials, I elſewhere mention, ſhew to be ſharp and piercing enough. To which may be added, that in divers caſes, where we are ſure, that acid ſpirits were plentiful ingredients of a compoſition, as in *ſaccharum Saturni*, and that magiſtery, which the chymiſts call ſalt of coral (which are not the only mixtures I have made trial of) experience witneſſeth, that the liquor, which comes over by diſtillation in retorts, is not acid, but quite of another kind. I would not, by what has been ſaid, be concluded to deny, that acid ſalts may, in ſome mixt bodies, be ſo aſſociated with others, and obſcured by them, as not to be diſcernible by the taſte, till they be ſeparated by the operation of the fire. But to ſhew, that ſuch acid ſalts were *de facto* pre-exiſtent, as acid ones in the body, that affords them, there muſt be ſome poſitive proof, other than the liquors diſtilled from them, ſince they, as we have already argued, may be not barely extricated, but may have their acidity produced by the operation of the fire. And we ſee, that ſalt-petre, though it have no acid taſte, may be made to afford, by (a certain way of) diſtillation, above three quarters of its weight of a highly acid liquor; and yet it appears not, that ſuch a great proportion of acid particles, or poſſibly any conſiderable proportion at all, was employed by nature in the compoſition of ſalt-petre. At leaſt, having diſtilled earth, that I cauſed in my own preſence to be dug out of a pigeon-houſe below the dung; I had from it a ſalt indeed, and ſome little ſaline liquor, but of a nature, as far as I obſerved, very differing from that of the acid ſpirit of nitre. (But this experiment I mention occaſionally, without building upon it.)

NOR do I think, it ought to ſeem incredible, that acid ſalts, as well as others, ſhould be producible by the various ſplittings, attritions, coalitions, and changes of texture, which may be cauſed ſeveral ways, and eſpecially by the operations of the fire; which moſt active agent, making a vehement and various agitation of all the minute parts, that a body conſiſts of, may, conſonantly to what hath above been intimated, ſplit or break ſome of them, and as it were grind others againſt one another; and in ſhort, ſo alter their bulk, figure and motions, as to make them fit to ſtab or cut the tongue, and the other bodies, that they work on, after the manner of thoſe bodies we call acid. But of this you may find more in our notes about the mechanical origin of taſtes; wherefore I now proceed to the ſecond part of my taſk.

SECT. II.

Of the production of volatile ſalts.

As to the production of volatile ſalts, we have an eminent inſtance of it in the ſalt obtainable by diſtillation from ſoot; for though the woods we burn in our chimneys ſeem not to have any thing of the taſte or ſmell of urinous ſalt, nor have the diſſolutions of the ſaline parts of ſuch woods communicated to water by their infuſion in it, been obſerved (that I know of) to be of affinity in taſte or odour with the ſalt of ſoot; yet when wood is firſt burnt in the fire, and then the ſoot afforded by it is duly diſtilled and rectified in fitly ſhaped veſſels, there is obtained a ſpirit and a white volatile ſalt, that in ſmell, taſte, and divers operations, by which we have examined them, appear to be of great affinity with thoſe of human blood, or urine, and may be eaſily enough miſtaken for them.

But this double operation of the fire is not always neceſſary to the production of volatile ſalts out of vegetables. For, though by their diſtillation in retorts we generally obtain from them no dry ſalt at all, but a ſouriſh ſpirit, with which I have diſſolved coral, lead, and other hard bodies, that urinous ſpirits have not been obſerved to work on; and they will, being put upon urinous ſalts, make ſuch an hiſſing and conflict, as are looked upon as great tokens of antipathy: yet I remember, that ſeveral years ago, I did from muſtard-ſeed, that had been kept for a convenient time, obtain by diſtillation a volatile ſalt, that faſtened itſelf in prettily figured grains to the upper part of the receiver, and this at the very firſt diſtillation; ſo that there was no need of rectifying the diſtilled matter, to ſeparate that ſalt. And to enforce this proof by ſomething more conſiderable than itſelf, I ſhall add, that by an eaſy way by word of mouth communicated to me by a very ingenious perſon (Dr. *D. E.*) one may, out of very many vegetables firſt duly prepared, without adding any thing to them, by bare diſtillations in retorts, obtain good ſtore of volatile ſpirits and ſalts, which by their fugacity, colour, ſmell, taſte, and divers experiments, that I purpoſely made to examine them, were ſo like the ſalt and ſpirit of urine, ſoot, *&c.* that one, that knew nothing of the way they were made by, would readily have concluded they belonged to one or other of the newly named ſorts of bodies.

I remember, that I have alſo ſometimes produced a volatile ſalt, that one would readily have pronounced urinous, of a mineral itſelf; nor was that the only foſſile, from which experience perſuaded me, that ſalt of this kind might be obtained.

Some other particulars relating to the production of volatile ſalts, I think fit to reſerve, till I ſhall have occaſion to mention them in another ſection (as inſtances of the production of urinous ſpirits). Only there is one thing, that I think not fit here to pretermit, becauſe I have not met with it in any chymical writer, the contrary being rather generally taken for granted. I ſhall add then, that it is not univerſally true, that ſaline ſubſtances, that are volatile and aſcend in the form of ſalt, are of an urinous nature, and enemies to acids. For I have had from verdigreaſe, diſtilled *per ſe* with a ſtrong fire, a very acid ſpirit; which being warily rectified, afforded firſt a ſour phlegm, and then a penetrant ſpirit ſharper than it, leaving behind it in the veſſel ſome few ſpoonfuls of a dark-coloured liquor, which being ſet aſide, and ſuffered to reſt, did in a great part ſhoot into tranſparent cryſtals, large but thin, almoſt like thoſe of ſilver, diſſolved in aqua fortis. They appeared prettily figured at the edges, but were ſo oddly connected among themſelves, that I was not able to refer them to any of the known geometrical figures; and their brittleneſs made them the leſs tractable, but their ſmell, which was ſtrangely piercing and not inoffenſive, argued them to be of the ſame nature with the acid ſpirit, which had come over with them.

But there is a more conſtant and eaſy way of producing ſuch a volatile ſalt, as my obſervation mentions. For if amber be gradually and warily diſtilled, it will afford, beſides the phlegm, ſpirit and oil, a dry ſubſtance, which, though the chymiſts call the volatile ſalt of amber, I found to be really of an acid nature, by ſeveral of thoſe trials, by which we are wont to diſcern, that a body belongs to the family of acids.

SECT. III.

Of the production of alkalies or lixiviate ſalts.

THE third and laſt ſort of ſalts, which we are to endeavour to ſhew to be producible, are the alkalies or fixt ſalts; which ſeem to have an antipathy with acid ones, by making a conflict with them, and exerciſing divers operations contrary to theirs (as I have in another diſcourſe more fully declared.)

As for the origin of theſe fixt ſalts of burnt bodies, the Spagyriſts are not of the ſame mind about it. For, the almoſt univerſal opinion of the chymiſts, that preceded *Helmont*, and the more common opinion even of later chymiſts, ſeem to have been, that theſe fixt alkalies are pre-exiſtent in mixt bodies; and that the fire does but ſeparate or extricate them from the other parts of the compounded body. But *Helmont* (followed in that by ſeveral chymiſts, that diſſent from him in other points) has ingeniouſly conjectured, that theſe lixivial ſalts do not pre-exiſt in their alcalizate form in the bodies, that afford them, but are productions of the fire, by whoſe violent action a part of the ſalt, which, in the concrete, is naturally all volatile, lays hold of ſome parts of the ſulphur of the ſame body, and both together are colliquated and fixt into an alkali; which fixation he ſomewhere exemplifies by that, which happens, when ſalt-petre and arſenic, that are both volatile, being expoſed to the fire, are by its operation fluxt and made to fix one another. But though this account be ingenious, yet I doubt, whether it be ſo clear and ſatisfactory, eſpecially ſince it is applied to all fixt alkalies, as the embracers of it think it. For beſides, that it may be queſtioned, whether it have yet been well proved (what *Helmont* teaches) that all the ſalt of mixt bodies before their combuſtion is volatile, it is not declared, what volatile ſalt is meant; though it be plain, that ſome bodies, that afford a fixt ſalt, do abound in acid ſpirits, as oak, box, and many other vegetables; and others, as hartſhorn, blood, urine, *&c.* abound with urinous ſalts, that exerciſe hoſtility with acids: and I have found, that from ſome bodies I could obtain both acid ſpirits, and ſuch as are wont to be called urinous. It is not eaſy to explain how the volatile ſalt comes to unite itſelf ſo intimately with the oil (or ſulphur) and though it be alſo volatile itſelf, to compoſe with it a body capable of enduring the violence of the fire, ſince we have more than once tried, that the volatile ſalt of urine, or of hartſhorn, and a chymical oil, as of turpentine or any ſuch, being put together, the ſalt will indeed aſſociate to itſelf ſome particles of the oil, but will nevertheleſs in their company ſublime in the form of a ſalt, with a very gentle fire. And the

Vide Helmontium in Blas humano, No. 38, & 43.

Vide Blas humanum, No. 41.

Vide Helmontium Complex. atque Miſt. &c.

the example, that *Helmont* ſomewhere gives of arſenic and nitre, does not ſatisfy me, becauſe that when I made equal parts of thoſe two bodies to be mingled, and in a ſtrong crucible fulminated together, a great part of the mixture was driven away by the fire, ſo little altered, that it is very dangerous to be too bold with the fumes, and a good part of what remained was fixt only, in compariſon of the crude arſenic, but not comparably to ſalt of tartar, or ſome ſuch other true alkali: and the conſtancy of the part, that was more fixt, may probably be aſcribed to the ſalt-petre, which we know will, without the help of arſenic, afford a great deal of fixt ſalt, if about half of it be burnt away, by the help of powdered charcoal, or ſome other convenient additament. It may alſo ſerve to weaken this inſtance of *Helmont*'s, that there are other inſtances, in which we may obſerve, that no ſuch thing happens, as his hypotheſis may make one expect. For common ſulphur is by chymiſts ſaid to abound in an oily part, upon whoſe account it is very inflammable, inſomuch that they would have other inflammable bodies to be ſo, by their participating of ſulphur. That this concrete alſo abounds in ſalt, is evident, according to their principles, by the acid menſtruum afforded by it, that goes under the abuſive name of *oleum ſulphuris per campanam*. And yet theſe ingredients, combined by nature, make up a concrete, which is ſo volatile, that both in cloſe veſſels and the open fire, it is almoſt totally volatile. And in that mixture of highly dephlegmed ſpirits of wine and urine, that *Helmont* calls the *Offa alba*, though the urinous ſalts do manifeſtly combine themſelves with the ſpirit of wine, which being totally inflammable, the chymiſts refer to their oil, or ſulphur; yet the coagulated mixture does not, by this aſſociation of ingredients, grow fixt, but proves very volatile. I will not here urge, in favour of the common opinion of the chymiſts of the pre-exiſtence of alkalies in mixt bodies, that a corpuſcularian may ſay two not inconſiderable things; whereof the firſt is, That there is no need of ſuppoſing a colliquation of ſalts with ſulphurs, oils or any thing elſe to produce fixt ſalts; ſince, beſides that that ſuppoſition does not explain, how two volatile bodies come to compoſe one that is fixt, it is plain, that a body yet more fixt may be made without any aſſociation of differing principles. For the earth, that together with the alkali remains in the aſhes of a burnt body, is more fixt than the alkali itſelf, and yet derives not its fixity from any combination of elements, or principles, but from the groſſneſs, ſolidity, or weight, and unfitneſs for avolation of the corpuſcles it conſiſts of. And the corpuſcularian may add in the ſecond place, that whereas ſome inſtances are alleged, wherein there is ſuppoſed a leſſening of the quantity of the fixt alkali of the concrete, by operations, that are ſaid to carry off the volatile ſalt, before the body comes to be incinerated; it may be anſwered, that perhaps thoſe very operations did but rarify and volatilize part of the pre-exiſtent alkali, and ſo left the leſs of it to be recovered by burning; as the chymiſts tell us, that fermentation rarifies the oily parts of the juice of grapes, and ſubtilizes them into vinous ſpirits, and ſo does much leſſen the quantity of the oil. And when wood is burnt in a chimney, it is not in the form of an acid ſalt, which is the only, that is commonly obſerved to be driven away by diſtillation in cloſe veſſels, but in that of an urinous ſalt (which is a-kin to alkalies, and an enemy to acids) that the ſaline part of the wood is made to aſcend; as may appear by the diſtillation of ſoot. Such arguments as theſe a corpuſcular philoſopher might, as I was ſaying, urge in favour of the more received ſpagyrical opinions. But inſtead of inſiſting on them, I ſhall only invite you to take notice of what I obſerve in ſalt-petre. For, though by diſtillation, or any other way, that we yet know, there is no oil to be ſeparated from it; yet above half the body of it may be eaſily and quickly turned into a fixt ſalt, in colour, taſte, and operation, much like that of tartar, and other incinerated vegetables. And ſuch an alkali I have made without the help of injected coals, or any other body, furniſhed with a combuſtible ſulphur: ſo that it ſeems not, at leaſt univerſally, true that to the production of an alkali there is neceſſary to be at hand an oil, or ſulphur, to be laid hold on by the volatile ſalt, and fixt together with it. But this experiment is far more congruous to our doctrine, which derives all thoſe ſalts from the ſize, ſhape, and ſolidity or weight of the ſame corpuſcles; ſince the ſame ſalt-petre, whoſe greater portion may, by the operations newly mentioned, be reduced to a fixt alkali, may, by being diſtilled with a convenient bolus, have its greater portion brought over in the form of an acid ſpirit or ſalt, which itſelf may afterwards be made materially to concur to the production of an alkali. I might add, that even from one of * *Helmont*'s own experiments, my concluſion may be inferred, ſince he ſomewhere, and, if I miſtake not, in more places than one, affirms, that by the addition of more alkalizate ſalt and the operation of the fire, the earth itſelf, that is in the aſhes, may be turned into ſalt; which, if true, argues, that a fixt ſalt may be made of that, which was not before, either of ſaline, or of an oleous nature; and conſequently without any ſuch combination of ſalt and oil, or ſulphur, as his hypotheſis ſuppoſes. From which experiment I may alſo infer the poſſible origination of alkalies, by the mechanical changes, that, without the addition of oil or ſulphur, the operations of the fire may produce in the part of a mixt body; ſince earth (excepting water) ſeems the moſt indiſpoſed of any part of the concrete to be turned into fixt ſalt.

I MUST nor here pretermit an obſervation, that I have made, which ſeems to overthrow the opinion of thoſe learned chymiſts, who will have the violence of the fire to be always a neceſſary agent, as I allow it to be ordinarily, to the production of a fixt or lixivial alkali. I ſaid ſeems to overthrow, becauſe I had not the opportunity to repeat my trials, and am not ſure, that the ſalt I employed was altogether genuine, in regard I cannot here in *London* meet with it, at any rate; but I have great cauſe to think it was right, both for other reaſons, and becauſe it was ſent with other things, for a preſent out of the Eaſt, to an inquiſitive nobleman, who had been lately ambaſſador for his Britiſh Majeſty, at the Ottoman court, and who was pleaſed as a rarity to preſent it me.

THIS ſalt was affirmed to be the true Egyptian nitre, mentioned ſo much by antient writers: and indeed I found it to agree much better with the notion, that books had given me of it, than with that ſort, to which chymiſts generally give the name of nitre, and which is indeed the only nitre to be uſually met with in our European ſhops; where it is beſt known by the vulgarly received name of ſalt-petre. But to ſay ſomething of our Egyptian nitre, though it be not pertinent to mention here all that I obſerved about it, yet I muſt not here omit two things, that I made trial of, with that little, which eſcaped the misfortune that loſt me all the reſt, that ſeem conſiderable in the preſent occaſion.

THE firſt whereof is, that this Nilotic ſalt was very apt to imbibe the moiſt air, as calcined tartar and other fixt alkalies are wont to be; to which reſolution we do not find our ſalt petre, if it be unmixed, diſpoſed. But the other and more important thing I obſerved, was this, that having upon this Egyptian nitre, crude as it was, poured ſpirit of ſalt, this acid liquor did preſently, even in the cold, work briſkly upon it, as if it were a fixt alkali, or at leaſt abounded with ſuch a lixivial ſalt. And here, upon the by, give me leave to take notice of a text of the holy Scripture, that has ſometimes puzzled not only me, but far better critics in the Hebrew tongue than I. And it is a paſſage to be found in the 25th chap. of *Solomon*'s Proverbs, where, to illuſtrate things very incongruous to one another, the diſagreement of vinegar and nitre is mentioned; for ſuppoſing the words to be rightly tranſlated, as beſides the authority of I know not how many verſions, the very ſound of the Hebrew word *næter* or *næthar* argues it to be, it ſeems very hard to find, what ſhow of antipathy there is between vinegar and the ſalt-petre, that is commonly ſold in our ſhops for nitre. Wherefore ſtrongly preſuming, that *Solomon*, who reigned in *Judæa*, a country near to *Egypt*, and had much commerce with the Egyptians, whoſe King's daughter he had married, made uſe of Egyptian nitre as the beſt known, if not the only in his time and country; and might have found in this nitre ſome quality very differing Jer. ii. 22. from any we find in our ſalt-petre; as I remember, that in the Prophet *Jeremy* nitre is mentioned as a very abſterſive thing, and fit to cleanſe women's ſkins, which is a known virtue of our fixed alkalies, but not obſerved in pure ſalt-petre: wherefore when once I received the nitre, that I have mentioned, and ſaw in it ſigns of an alkalizate nature, I quickly poured upon it ſome ſtrong vinegar, and found, as I expected, that there preſently enſued a manifeſt conflict, with noiſe, and ſtore of bubbles; with which experiment I afterwards acquainted ſome critics, and other learned men, who were not ill pleaſed with it. But this theological uſe of the alkalizate nature of nitre not being that, which I chiefly mentioned it for, I ſhall now make the philoſophical uſe I intended, by taking notice that Egyptian nitre being acknowledged to be a native ſalt, and made only by the evaporation of the ſuperfluous water of the *Nile* (or ſome other ſuch liquor) is yet of a lixiviate nature, or at leaſt abounds with particles that are ſo: though, as I freſhly intimated, it was produced without any precedent incineration, and the matter of it ſuffered not, or at leaſt needed not ſuffer any violence of the fire to make it afford an alkali.

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* *Cinis proprio ſuo alkali fit ſal merum, complex. atque miſt.* No. 12. *Idem Helmontius in Blas humano,* No. 39. *videatur.*

I HAVE repreſented theſe things, not for that I pretend to be ſure, that alkalies may not be produced in multitudes of mixt bodies, eſpecially in a good number of vegetables, after the way propoſed by *Helmont*, or by ſome ſuch like; but partly, becauſe it ſeems not always neceſſary to the exiſtence of an alkali in nature, and to be the only way it can be produced by; and partly, becauſe I would give you and your chymical friends occaſion to clear (as far as they can) and confirm the doctrine I have queſtioned. It is true, that being a far greater friend to truth, than an enemy to the chymical hypotheſis, I would not ſtifle what may ſerve to advance that, in favour of *Helmont*'s doctrine, though this would never ſo well accommodate my preſent argument. But I have no great temptation to ſurmount in this caſe, for it concerns very little the main ſcope of this diſcourſe, whether *Helmont*'s way, or any other, of the production of alkalies be embraced; ſince it will ſuffice for my purpoſe, if ſome bodies belonging to this family, or kind of ſalts, may be produced: I ſay ſome, becauſe (as I have already intimated) I will not peremptorily aſſert, that all fixed alkalies are productions of the fire, or made by the help of it; though I do not readily remember, that I have met with any (except Egyptian nitre) that are not. But I ſhall wave that queſtion, becauſe my intended brevity calls upon me to proceed to the mention of ſome particular inſtances, fit to perſuade us, of the producibleneſs of ſome alkalizate ſalts.

IT is known, that chymiſts generally look upon ſpirit of nitre and aqua fortis, as liquors containing not alkalies, but acid ſalts; which they conclude not only from the taſte, but from the great ebullition, that is made, when thoſe liquors are poured on the ſalt of tartar, fixed nitre, pot-aſhes, or other ſuch unqueſtioned alkalies. That ſea-ſalt likewiſe does not contain alkalies, is generally taken notice of, the ſpirit of it being juſtly reckoned among the acid ones; and when I purpoſely examined that concrete by diſtillation, the remaining ſalt, though the fire had been violent, was very differing from alkalies: and yet my conjectures inclining me to ſuſpect what the event would prove, I ſeveral times made the following experiment upon ſea-ſalt, that yet retained all its acid ſpirit in it.

UPON well dried and powdered ſea-ſalt we put into a retort ſometimes an equal, and ſometimes (which I prefer) a double weight of good ſpirit of nitre, or aqua fortis, and leiſurely diſtilling all that would come over, we took out the dry ſalt remaining at the bottom, which we found much changed both as to colour (a good part of it being uſually very reddiſh) and as to taſte, which was differing enough from what it had been before, and might probably have been made much more ſo, if freſh ſpirit of nitre had been once or twice more abſtracted from it. This ſalt being again powdered (for it was in a lump when taken out) and put into a crucible, placed in a convenient fire, was by the repeated injection of fragments and well kindled charcoal made to flaſh divers times almoſt like melted nitre; and when it would flaſh no longer, the remaining matter being taken out, did, in great part, appear to be brought to an alcalizate nature, for it had a fiery taſte upon the tongue: if ſpirit of nitre or aqua fortis were poured on it, it would make an ebullition, it would turn ſyrup of violets green, and in ſhort, exhibit divers phænomena of alkalizate ſalts.

ANOTHER way there is like that mentioned of making an alkali out of nitre, which is thus done; pour upon it an equal weight, or half the weight of ſtrong oil of vitriol; and having diluted the mixture with a convenient proportion of fair water, diſtil it by degrees, till there remain a ſubſtance very dry. Powder this, and mix it well with about an eighth part of beaten charcoal; keep them in fuſion in a ſtrong and covered crucible, till the maſs grow very black, and a little of it being taken out with a wire, taſte fiery upon the tongue (which may happen in about half an hour or an hour, according to the quantity of matter, and degree of fire:) then take out the blackeſt or deeply red mixture, which will very eaſily imbibe the moiſture of the air, and you may find it, at leaſt while it is hot and dry, of a more fiery lixivial taſte than ſalt of tartar itſelf. It will make an ebullition with acid ſpirits, and precipitate divers ſolutions made with them; it will turn ſyrup of violets green, and in ſhort diſcover itſelf many ways to be of an alkalizate nature, though it be aſſociated with a ſulphur, that may, by divers methods, be made appear to be contained plentifully in it.

IT is alſo conſiderable on this occaſion, how the ſame body, without the addition of any other ſalt, may by the various manner of the fire's application to it be made to afford, either little elſe than acid ſalts, or a leſſer or greater quantity of alkali: for if fine ſalt-petre be dexterouſly diſtilled with about thrice its weight of ſome fit earth (but not, as is uſual, with powdered bricks) it will ſometimes afford very near as much ſpirit of nitre as the ſalt weighed; and though this, like other liquors, be not without phlegmatic parts, yet beſides that it may be doubted, whether moſt or many of them were not produced by the tranſmuting operation of the fire, we may ſuppoſe, that five parts of ſix, or ſix of ſeven have been diſtilled into dephlegmed ſpirit.

But if, according to the way I have elſewhere * circumſtantially delivered (which is by frequent injecting into fluxed ſalt-petre ſmall pieces of kindled charcoal, till one can make it flaſh no more at all) you make fixed nitre, you may obtain from nitre thus handled half its weight, and perhaps better, of an alkalizate ſalt, that many would, by its taſte and operations, gueſs to be ſalt of tartar.

But to ſhew yet further, how much the production of this alkali depends upon the operation of the fire, which, as it is variouſly applied, may vary the texture of the ſalt-petre, my conjectures led me to try the following experiment, which I did with ſucceſs from the beginning. We took a pound of good ſalt-petre, which was but groſsly beaten (for it ſhould not be finely powdered) and having laid it on a conical heap upon a flat tile, that the air might on all ſides have acceſs to it, we cauſed the upper part of it to be kindled by a little fragment of burning coal (which may be afterward thrown away.) Then we cauſed the laborant with an iron rod dexterouſly to ſtir the kindled part of the nitre, that the ignition might be preſently communicated to as many parts of the ſalt, as was poſſible; and this nimble ſtirring of the maſs, that the fire might be more diffuſed, and more parts might be obverted to the air, we cauſed him to continue to the end of the operation: by which method, within few minutes, we obtained, more than once, out of ſixteen ounces of ſalt-petre, about ten ounces or better of fixed nitre, very lixivial in taſte and operation; and for the colour, it was of a pleaſant greeniſh blue, and deeper than ſalt of tartar will uſually be brought to, by being (in a crucible) kept twenty times as long, in a good fire.

The other ſcopes and uſes deſiged in this new and quick way of making the alkali of nitre belong to another diſcourſe, the experiment, which will ſcarce ſucceed without a dextrous management, being here mentioned to ſhew, what quantity of alkalizate ſalt may by a differing operation of the fire be obtained from nitre; which (crude nitre) in diſtillation ſkilfully made for the purpoſe may be in great part driven over, in the form of acid ſpirit, and phlegm, and leaves ſo little true alkali behind it, that I have wondered at it, being ſometimes ſcarce able to find any at all, though I purpoſely tried to ſeparate it from the tobacco-pipe clay, which the petre had been mixed with after a diſtillation, wherein not half of the ſalt had been driven into the receiver, in the form of ſpirit.

And to ſhew, that to make the fixed ſalt of nitre, the actual inflammation of it, in the open air, is not neceſſary, as very learned men have ſuppoſed; and that it is poſſible, whatever is preſumed to the contrary, to make an alkali nitre, though charcoal, or ſome other combuſtible body be not added to it, to kindle the corpuſcles of the nitre by its ſulphur, and by the aſſociation of ſome part of the ſame ſulphur with the ſaline parts of the nitre, to compoſe an alkali; to ſhew this, I ſay, I more than once made the following experiment: with a convenient quantity of good ſalt-petre we carefully mingled about an eighth part of tobacco-pipe clay; and putting the mixture into a crucible cloſely luted at the top, we kept it by a fitly graduated fire in fuſion for ſome hours, and found, as we expected, that the remaining ſalt (for part would get through the lute, or commiſſures in the form of fumes) was turned into an alkali, of a fair blue colour, like the better ſort of that fixed nitre, which is made with charcoal. This experiment, and that formerly made with tobacco-pipe clay, ſeem plainly to argue, that to the making of fixed nitre, which is confeſſed to be an alkali, a congruous change of texture may ſuffice; whether that change be

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* In the *Eſſay of the differing Parts and Redintegration of Nitre.*

attempted to be made in open veſſels, or in cloſe ones, with, or without the addition of the ſulphur of charcoal, or any other ſuch combuſtible body. Upon the ſame ground, that I had for trying the former experiment, I attempted, and not without ſucceſs, to make an alkali of ſalt petre, by colliquating moderate quantities of it, ſeveral ways (and keeping it in fuſion) with ſome metals: I ſay, moderate quantities of nitre, becauſe an ingenious gentleman, to whom I communicated this practice, could not make it ſucceed in any conſiderable quantities. And to obviate the ſuſpicion, though perhaps groundleſs, that ſome chymiſts might have of the material concurrence of a good portion of the combuſtible ſulphur, preſumed to be in the ignobler metals to the production of the nitrous alkali; I ſhall add, that our experiment ſucceeded, when we tried it more than once, with more than ordinarily fine ſilver, whoſe ſulphur, if it have any, is granted to be fixt or incombuſtible. And I remember the laſt trials afforded us a bluiſh alkali, though there were employed but a fifth part of ſilver, in reference to the nitre; and though the fire (which was continued for ſome hours) was ſo moderate, that the metal, though thinly laminated, was not melted; and of an ounce, that was put in, there wanted but four grains, which ſmall loſs might eaſily be imputed to divers accidents.

After what is ſaid of the production of lixiviate ſalts and alkalies, it will not be impertinent to add, that as they may, by the operation of the fire, be produced; ſo by the operation of the fire they may be deſtroyed or deſpoiled of their alkalizate form, and turned into a ſubſtance of a nobler nature. This I am induced to think very probable by ſome experiments, amongſt which, that which ſeemed the moſt conſiderable, was this: we took a pretty quantity of good ſalt of tartar, that had been purified by ſolution and coagulation, and having put it into a clean crucible, we kept it in a ſtrong fire (that made the crucible red-hot) for a good while; then giving it at length a ſtronger fire, we poured it or (afterwards) took it out of the pot, and diſſolved again as much of it as we could in cold water, which being ſet to run through cap-paper, there appeared, as I foreſaw, in the filtre a pretty deal of matter, that would not (as the whole ſalt had done at firſt) be diſſolved in the water, but was turned into a kind of earthy ſubſtance. Then coagulating again the ſolution, that had paſſed through the filtre, into dry ſalt; we expoſed it again in the crucible to a ſtrong fire, and putting it again into water, we perceived it would not totally diſſolve, but left in the filtre a ſlime or mud. And in this manner we proceed to ignite, diſſolve, filtre, and coagulate the ſame ſalt of tartar many times; for, if I much miſtake not, it was ſixteen times, and ſtill found ſuch an earthy ſubſtance as has been ſpoken of, remaining in the filtre; and the reſt of the ſalt of tartar ſo little altered, that being ſomewhat tired, and other ways diverted, I deſiſted from proſecuting the operation to the uttermoſt; concluding it highly probable, that the remaining ſalt might, by the ſame way of management, be brought to yield more and more of that ſame ſubſtance, which either was earth, or of kin to it, being at leaſt ſomewhat, that was of a nature very differing from ſalt of tartar, ſince it was not like it, fiery on the tongue, and was indiſſoluble in water, as earth, but not ſalt of tartar, is known to be.

SECT. IV.

It may bring ſome illuſtration, and add ſome probability to what has been diſcourſed about the producibleneſs of ſalts, if we conſider what happens in the compoſitions and decompoſitions of ſaline particles, and their operations on other bodies, and on one another. For if it appear, that, by theſe manifeſt and mechanical ways, ſuch alterations may be made, and ſuch qualities produced, as are either altogether, or very near as conſiderable, as thoſe, which diſcriminate the ſeveral families of ſalts formerly ſpoken of, from one another, and from this or that chymical principle; it will, I preſume, be judged the more credible, that theſe families of ſalts may be either tranſmuted into one another, or otherwiſe produced; and ſo may not be primordial and immutable beings, in the ſenſe, wherein the chymiſts would have them to be ſuch. I have elſewhere taken notice of the production of vitriols, ſal armoniac, borax*, and divers other factitious ſalts; for which reaſon; I ſhall not inſiſt on them here the rather, becauſe it may ſuffice for my preſent purpoſe to take notice of two ſalts, whereof the one is meerly factitious, and the other ſuch in great part; and yet each of theſe, by a very ſlight and eaſy way of ordering it, afforded me differing ſaline concretions, ſome of which reſembled a ſalt, which many judge the moſt ſimple and natural, that we yet know of.

To ſhew then, that common ſalt itſelf, that ſeems the moſt primitive and ſimple amongſt groſs and viſible ſalts, may be produced by a change of texture made in bodies very differing from common ſalt, I ſhall recite an experiment, which though it have ſometimes failed me, yet it has divers times anſwered my deſire, though I ſhall now relate but that ſingle one of my laſt trials, that ſucceed beſt.

THAT, which our Engliſh glaſſmen call *ſandever*, and the French, of whom probably the name was borrowed, *ſuindever*, and is, you know, that recrement, that is made, when the materials of glaſs, namely ſand and a fixt lixiviate alkali, having been firſt baked together, aud then kept long in fuſion, the mixture caſts up the ſuperfluous ſalt, which the workmen afterwards take off with ladles, and lay by as little worth; this ſalt ſeldom uſed by mineraliſts, and ſcarce wont to be mentioned by the writers of courſes of chymiſtry, I have thought fit to employ about ſeveral purpoſes, invited thereto by conſidering the uſual way, wherein it is produced. For in ſandever we have a ſalt, which was once altogether lixiviate, but which having been kept long melted in a ſtrong fire with ſand (or flints or pebbles) muſt have had its ſaline corpuſcles variouſly and forcibly ground or rubbed againſt one another, and againſt the particles of the ſand, ſome of which it may alſo have diſſolved and retained with it; by which rude juſtlings and mutual attritions, I thought it very probable, that the alkali muſt not only have been conſiderably altered, but variouſly too, ſome parts being changed more and after a differing manner than others, by which means ſandever may conſiſt of portions differingly qualified both in reference to the lixiviate ſalt, that was at firſt employed, and to one another.

WE took a pretty quantity of good ſandever, and having diſſolved it in fair water, and filtred it, we ſet it to evaporate in a digeſtive furnace, till a ſaline cruſt, as if it were a thin plate of ice, ſpread itſelf upon the top of the liquor; then ſuffering it to cool and cryſtallize, we broke the mentioned cover, to come at the cryſtals, and ſet the liquor we had poured off from them to evaporate further and ſhoot again: and in this method we proceeded, whilſt we judged worth while to do ſo. By this means we obtained good ſtore of cryſtals, whoſe figures were not the ſame, but many of them differing enough, though moſt of them tranſparent and prettily ſhaped, as if nature had at once affected variety in their figuration, and yet confined herſelf to geometrize. But the chief thing, for which I mention this experiment, is, by this way of proceeding, I more than once obtained (not on the very ſurface of the water, as is uſual in the concretion of ſea-ſalt) but in other parts, and chiefly beneath the ſurface of the ſaline plate formerly mentioned, a conſiderable number of grains of ſalt, that better anſwered to the deſcription of common ſalt, than diſſolved and filtered ſea-ſalt itſelf is wont to do: for theſe grains, that were of no deſpicable bigneſs, were as like little cubes or dice, as if they had been made by a ſkilful jeweller; and their ſurfaces had a ſmoothneſs and gloſſineſs much ſurpaſſing whatever I had obſerved in marine or common ſalt.

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* In the *Eſſay of the Uſefulneſs of Chymiſtry to the Empire of Man.*

I MAY confirm the difference, I have mentioned to be between ſandever and common alkalies, if I here add, that ſome while ago having ſet a good quantity of filtered ſolution of ſandever to coagulate in a cool place, and thereby brought a great part of the ſalt to coagulate into cryſtals, almoſt like thoſe of nitre, but ſo very diaphanous, that divers of them were clear as rock cryſtal itſelf; I did not obſerve them to relent by the moiſture of the air in a long time, though the glaſs they were kept in were negligently enough covered with paper only; which argues their texture to have been remote enough from that, which is proper to alkalies: and to ſhew, that they were alſo ſalts of a peculiar nature, I ſhall further obſerve, that if they were expoſed, though but to a gentle heat, they would in no long time loſe their tranſparency, and be reduced to a white and fine calx; which being weighed and re-diſſolved in water, and made again to cryſtallize, would be diaphanous, and concoagulate with itſelf ſo much of the water, as ſufficed to give a very notable increaſe of the weight.

SANDEVER, which afforded me the firſt of the two inſtances I promiſed you of the production of ſalts, is one of thoſe bodies, that many would reckon amongſt thoſe, that are almoſt meerly artificial. I ſhall now mention a ſecond inſtance of a body, wherein art ſeems to have little to do, ſave the eaſy extrication of its particles from thoſe, wherewith nature had blended it in a human body. If then man's urine, after having been kept ſome weeks in a cloſed veſſel, be expoſed to a moderate fire, firſt it will yield a ſpirit and a volatile ſalt, and then a very copious phlegm, which being totally exhaled, there will remain a dry *caput mortuum*; and this being warily calcined, diſſolved in water, and coagulated, if the experiment ſucceed with you, as it did with me, you will find the ſalt very different from a common lixiviate alkali, rather you will find ſaline concretions of differing forms, if not kinds: for I obſerved ſome to be oblong, and to look like ſome cryſtals of nitre; others, to be of figures reſembling thoſe, that geometricians call *Rhombuses* or *Rhomboides*; and one of the faireſt of theſe lozenges, I remember, for trial-ſake, I kept for many days expoſed to the air, and that in winter, without finding it run *per deliquium*, as a piece of common alkali of that bigneſs would have done in a little part of that time. But beſides thoſe numerous ſaline concretions, that I could not eaſily reduce to any known figure, there was (which I chiefly expected, and would have you take notice of) a conſiderable number of fine grains, that looked like common ſalt, and were indeed more exactly cubical in their figure, than the grains of ſea-ſalt themſelves are wont to be. And I have the leſs cauſe to doubt, that the ſea ſalt abounding in our *caput mortuum* was not a common lixiviate alkali, but conſiſted of parts of other natures, eſpecially of ſuch as compoſed ſea-ſalt; becauſe I obſerved, that the *caput mortuum*, when expoſed to calcination, began early to melt in the fire, before it was near calcined, as not an alkali, but ſea-ſalt, would have done; alſo becauſe the taſte was much nearer to that of brine, than to that of lixivium; and becauſe, laſtly, it would make no conflict with the ſpirit of ſalt, as an alkali would have done, but did make of a ſolution of ſilver in aqua fortis a white precipitate, like that we make of that metal with ſea-ſalt, but not with alkalies.

AND becauſe a miſchance unſeaſonably deprived me of the *caput mortuum* of domeſtic urine, prepared in my own laboratory, I was fain to procure a ſupply of fixt ſalt of urine made by a diligent Spagyriſt of my acquaintance, who had wrought much upon that liquor, and having diſſolved and filtered a pretty quantity of this ſalt, and ſuffered the ſolution to evaporate ſlowly, till it began to have a ſkin, I found the cryſtals it afforded in a cool place to be ſome of them an inch or two long, and ſhaped almoſt like cryſtals of nitre, ſave that they were ſharper at both ends, and to many of them were faſtened ſtore of minute and oblong cryſtals prettily ſhaped, which were placed almoſt perpendicularly upon the greater proportions of ſalt, which by this means obtained a ſhape, not much unlike that, which the *French* engineers call *cheval de friſe:* theſe cryſtals, as they did not reſemble common alkalies in their figures, ſo they were unlike to them in divers other reſpects. For though ſome oil of tartar *per deliquium* being poured upon ſome of them, there enſued no manifeſt commotion, as is wont to be produced, when that liquor is mixt with a ſalt, where an acid is predominant, yet being beaten and mixt with an acid ſpirit, as that of common ſalt, they made not the leaſt ebullition or conflict, though they were ſtirred up and down to excite it; nor did aqua fortis, good enough to be worthy of that name, produce any hiſſing noiſe or froth, when it was put upon the ſalt of urine, though at length it diſſolved a good proportion of it. And though ſtrong oil of vitriol being put upon ſome of the forementioned cryſtals, did readily work upon them, and in corroding them excite a good number of bubbles, yet that did not ſurpriſe me, nor make me conclude the ſalt to be alkalizate, becauſe I have obſerved oil of vitriol (though not ſpirit of ſalt or aqua fortis) to work after the like manner upon common ſalt, of which that the fixt ſalt of urine did participate, I judge very probable, partly upon the account of the phænomenon newly recited, partly becauſe I found, that by impregnating good aqua fortis with a competent quantity of the fixt ſalt, inſtead of diſſolving in it ſea-ſalt, I could make it capable of corroding foliated gold, even in the cold; and partly alſo, becauſe that ſome part of the ſolution of our fixt ſalt, that was more ſlowly coagulable, being mixt with oil of tartar, preſently grew thick and muddy, and ſoon after let fall a precipitate copious enough: and another part of the ſame ſolution did readily precipitate ſilver diſſolved in aqua fortis, but would not ſo much as diſcolour a ſtrong ſolution of ſublimate (made in fair water) from which a common lixiviate alkali would have immediately ſtruck down an orange-coloured powder.

A LIGHT ſuſpicion I once had, that the common ſalt, that moſt men eat to ſeaſon their aliments, may in ſome degree impregnate men's urine, gave me the curioſity to examine that of horſes, which I found to require rather a ſhorter than a longer putrefaction than human urine, to make it fit for diſtillation; but the *caput mortuum* of this alſo, I was by an accident hindered to examine ſufficiently: but by the ſpirit and volatile ſalt the liquor after putrefaction eaſily afforded, it ſeemed probable enough, that the fixt ſalt would have been not unlike that of men's urine. Of which laſt named olid and deſpicable liquor I choſe to make an inſtance in this place, becauſe chymiſts are not wont to care for extracting the fixt ſalt of it (which is therefore commonly preſumed to be like other alkalies) but as ſoon as they have diſtilled the ſaline ſpirits, throw away all the reſt as nothing worth. Which practice, as general as it is, I cannot commend; for though I am not altogether of *Helmont*'s mind, where he ſays, that *Wiſdom deſpiſes thoſe, that deſpiſe the indagation of urine, and refuſe by the fire to ſearch out its contents*; yet I think, that thoſe, who underſtand the myſtical writings of ſome of the beſt chymical philoſophers of former times, will look upon it as a more tolerable hyperbole, than other men or even vulgar chymiſts imagine it to be.

OF THE

PRODUCIBLENESS OF SPIRITS.

PART II.

SECT. I.

Of the production of vinous ſpirits.

AS for what the chymiſts call ſpirit, they apply the name to ſo many differing things, that this various and ambiguous uſe of the word ſeems to me no mean proof, that they have no clear and ſettled notion of the thing. Moſt of them are indeed wont, in the general, to give the name of ſpirit to any diſtilled volatile liquor, that is not inſipid, as is phlegm, or inflammable, as oil. But under this general term they comprehend liquors, that are not only of a differing, but muſt be, according to their principles, of a quite contrary nature; ſome of them being acid, as ſpirit of nitre, of ſalt, and of vinegar itſelf; and ſome of them urinous, or, as ſome would have it, volatile alkalies, which are ſuch enemies to the former, that, as ſoon as they are put together in due proportions, they tumultuate and grow hot, and uſually continue to fight, till they have diſarmed or mortified each other. Beſides theſe two hoſtile families of ſpirits, there is a third ſort, which they call vinous or inflammable, which though very ſubtile and penetrant, is not manifeſtly either acid, or alkalizate; I ſay, manifeſtly, becauſe the taſte and ſmell of this ſort of ſpirit is differing from both the ſorts laſt named; and yet is referred to one, or the other of them, by ſome learned Spagyriſts; with whom I neither need, nor deſire to diſpute about this matter; ſince it may ſuffice for my purpoſe, if it can be made out, that all the three ſorts of ſpirits above mentioned, the vinous or inflammable, the urinous or alkalizate, and the acid, may be produced, and conſequently may be other than primeval bodies.

AND to begin with the firſt named, theſe vinous ſpirits are ſo producible by art, that we ſeldom find them produced by nature alone, which does indeed make the juice of grapes, but does not make wine, nor the ſpirit of wine, unleſs, by the help of man, that juice be preſſed out, and fermented. And the caſe is yet more plain in the ardent ſpirits made of ale, beer, and in the like vinous ſpirits made by the decocting and fermenting the ſeeds and other parts of vegetables. And it is obſervable to our purpoſe, that muſt (or the juice of grapes newly preſſed out) does not in diſtillation yield a vinous and inflammable ſpirit. And I remember, I had once the pleaſure to laugh at a man, otherwiſe very ingenious, who, to catch the ſubtile vinous ſpirit, that he would have me think was loſt in the common way of handling wort, made it work in a huge copper limbec, to catch the ſpirituous parts, that he thought would otherwiſe fly away; by which means, inſtead of the ardent ſpirit he expected, he got nothing in his receiver but a nauſeous phlegm. I have alſo found by trial, that raiſins (which we know are but dried grapes) being diſtilled alone, afforded an acid and empyreumatical, but not a vinous ſpirit; whereas, when I carefully

fully fermented them with a due proportion of water, they would afford me in diſtillation an ardent ſpirit like that of wine.

If it be objected, as I preſume it will, that the vinous and inflammable ſpirit, that is by fermentation obtained from bodies, was actually in them before, and is only extricated by fermentation; I anſwer, that this is *gratis dictum*, and is therefore not to be admitted till it be proved; ſince raiſins, and ſuch other fermentable bodies do not, upon the ſuppoſed analyſis made by diſtillation, afford a vinous ſpirit, but one that is very differing from it. And I ſee not, why the change of texture may not turn ſome part of the juice of grapes into a vinous and inflammable liquor, ſince a little further change is able to turn the ſame juice into a liquor, that is acid, and neither vinous nor inflammable, as it was before. And I have found by trial, that even this vinegar, crude as it is, being ſatiated with calx of lead made *per ſe*, would afford a ſpirit not acid, but of a very differing taſte, and inflammable, like the ſpirituous parts of wine. And if it ſhould be further objected, that theſe inflammable ſpirits were not produced by theſe operations, but pre-exiſting in the newly expreſſed juice of grapes, were only extricated by fermentation, and being afterwards covered or diſguiſed by the acid particles of the vinegar, were again extricated by diſtillation, the acid ſalts having fixt themſelves upon the lead they corroded, and thereby given the ſpirits leave to forſake them: if I ſay, this be objected, I might refer you to a more full anſwer, that I have elſewhere given. And at preſent it may ſerve the turn, that I put you in mind again, that the objection alleges no phænomena to make appear the actual pre-exiſtence of vinous ſpirits, either in the juice of grapes, or in the ſolution of lead.

And though I need but deny what is barely affirmed, not proved; yet to examine this matter further than I had found others to have done, I did (as I elſewhere mention) make a *ſaccharum Saturni* with an acid liquor made without fermentation, or the addition of any liquor from wood itſelf; and then, diſtilling it alſo without any additament, I had (as I expected) a ſpirit, that readily took fire and burnt away in a blue flame, like that of ſpirit of wine. I know another objection may be framed from the doctrine of ſome chymiſts, who would have ſpirit of wine to conſiſt of the oily parts of the juice of grapes rarefied and ſubtilized. But with theſe learned men (for ſuch I know ſome of them to be) I have not here any need to enter into a diſpute; ſince, without examining, whether their opinion be true or no, if it be admitted, it will be conſiſtent enough with mine. For to ſay, that by ſubtiliation, rarefaction, a peculiar kind of commixture with the phlegm, or the like means, the oil contained in the juice of grapes (and ſeparable from it, in the form of oil, if it be diſtilled before it be fermented) becomes ſpirit of wine, is but to aſſign the modus, whereby vinous ſpirits are produced, but not to deny their production. And all that my purpoſe requires, is, that it be proved or granted, that inflammable ſpirits are really produced, by what way ſoever they come to be ſo. I ſhall add, that though experience witneſſes, that honey being ſkilfully fermented with a due proportion of water, will yield a greater plenty of inflammable ſpirit than the wine itſelf, that is made in divers countries; yet when we have carefully diſtilled honey before fermentation, it afforded us a great proportion of conſiderably acid ſpirit, that would diſſolve ſome metals, but ſo little oil, that the paucity ſeemed ſtrange, and made it appear unlikely enough, that ſo inconſiderable a proportion of that liquor ſhould be rarefiable into ſo much ardent ſpirit, as may be obtained from well fermented honey.

SECT. II.

Of the production of urinous ſpirits.

I proceed now to the other ſort of ſpirits, as thoſe of hartſhorn, blood, ſal-armoniac, ſoot of wood, *&c.* that we have formerly called urinous, becauſe of their great affinity in many qualities to the more familiarly known liquor, ſpirit of urine. But as for theſe, I know not, whether it will be neceſſary to treat of their origin apart; ſince, for aught experience has yet aſſured me, theſe ſpirits are not ſimple, but compounded bodies, conſiſting of the volatile ſalt of the concrete, that afforded them, diſſolved in the phlegm, and for the moſt part accompanied with ſome little oil, at firſt undiſcerned by the eye, though afterwards it grow viſible. The preſence of this oil in moſt ſpirits, belonging to this family, may be probably argued from the deep tincture, that in proceſs of time, ſpirit of hartſhorn, of blood, and divers other ſubjects, will acquire by ſtanding, though preſently after their diſtillation, and firſt or ſecond rectification, they were clear, and colourleſs as water; the oily portion, which, whilſt it was in very minute particles, lay concealed in the mixture, becoming diſcoverable in proceſs of time by their extricating themſelves a little, and aſſociating, though not ſo far as to emerge and float, yet far enough to diſcloſe themſelves by the colour they give the liquor. But in ſpirit drawn by the help of an alkali from ſal armoniac, a concrete not abounding in oily parts, like hartſhorn, blood, *&c.* kept not only for many months, but divers years, I obſerved no ſuch diſcolouration; which was one inducement to make me, in ſpeaking of the oleaginouſneſs of urinous ſpirits, to employ the word *moſt* rather than the word *all*.

Having therefore hitherto by rectifications and digeſtions obſerved nothing in theſe urinous ſpirits but a cryſtalline volatile ſalt, moſt commonly ſeparable in a dry form, and the phlegm it was diſſolved in, beſides ſome oleaginous particles, that had (though at firſt unperceivedly) aſſociated themſelves to it; I ſee no great need to trouble you with particular inſtances about this ſort of ſpirituous liquors; what has been ſaid making it allowable for me to refer you to what I deliver about the production of ſalts, where that of volatile ones is mentioned.

SECT. III.

Of the production of acid ſpirits.

As for acid ſpirits, that ſome of them may be generated or produced *de novo*, ſeems probably deducible, partly from what has been already delivered (in the firſt part of theſe notes) concerning the production of acid ſalts; and partly from what will be ere long recited of acid, as well as of urinous and of vinous ſpirits, obtained by diſtillation from one and the ſame body. And if we take the word acid, as I uſually do in theſe notes, in a familiar ſenſe, without cryptically diſtinguiſhing it, from thoſe ſapors, that are a-kin to it, perhaps the ſpirit of ſea-ſalt and that of nitre may be fitly enough propoſed, as inſtances of the production of acid ſpirits. For though ſea-ſalt and its diſtilled liquor have upon ſome bodies the like operations, as either of them will precipitate ſilver out of aqua fortis, yet not only the taſte of the ſpirit of ſalt (eſpecially that which riſes laſt in diſtillation) is exceeding different from that of crude ſalt, not only in ſtrength and penetrancy, but in this, that the ſpirit is highly acid. Whereas the crude ſalt has a taſte not properly acid, but that, which by a diſtinct name is in Latin commonly called *ſalſus*, ſuch as that, which predominates in brine; and it does not appear, that this acid ſpirit did as ſuch pre-exiſt in the ſalt, whence it was obtained, ſo that we may ſuppoſe it to have been made rather by tranſmutation than extrication. And the like, I think, may (and that with greater probability) be ſaid of the ſpirit of nitre; for though this be highly acid, yet the nitre, that afforded it, is not at all ſenſibly acid: and this new vehement taſte of the ſpirituous parts, as well as their great efficacy in diſſolving metals, and divers other bodies, ſeems to have been produced by the violent action of the fire (agreeably to what I formerly noted) which by cleaving the nitrous corpuſcles, or by rubbing them one againſt another, or by both theſe ways, and perhaps by ſome others, makes a comminution of them into fragments or particles, which both becauſe of their ſmallneſs and lightneſs may be elevated by the action of the fire, and becauſe of the ſame minuteneſs and their ſharp and pointed figures may get into the pores of many other bodies, and divide their parts. I know, that chymiſts may object, that all the acid ſpirit, that can be diſtilled from nitre, was really pre-exiſtent in it, and only clogged and diſguiſed by the alkalizate ingredient, wherewith it was aſſociated, as may appear by what I myſelf relate of the ſpeedy way of making ſalt-petre, by putting a due proportion of the ſpirit of nitre to the alkali, or fixt part of nitre, that remains after the ſalt-petre has been fulminated or burned. But to this I anſwer, that this proves indeed (what I readily grant) that ſalt-petre may be artificially compounded of a nitrous ſpirit and a fixt alkali, but does not prove, that nature does always, or ſo much as ordinarily produce nitre by the ſame ways, that is, by compounding it of the ſame ingredients. For it does not appear, that wherever ſalt-petre is generated in the earth, nature has before-hand laid in a proviſion of lixiviate ſalt, which (at leaſt, in theſe countries) is not wont to be made without the violence of an incinerating fire and of corroſive ſpirits, to obtain which, or either of them, artiſts are fain to employ vehement fires; whereas it ſeems, that ſalt-petre is ſlowly generated in the earth by gradual or ſucceſſive alterations of ſome idoneous matter, wherein, for aught I have obſerved, not an acid, but an urinous ſalt is predominant; as may be made probable by what I have formerly related about earth, that had long lain covered with pigeons-dung in a dove-houſe, which I found in diſtillation to yield a volatile ſpirit and ſalt, much like thoſe of urine. Therefore I will not affirm, that nature does never employ fixt alkalies and acid ſpirits to make ſalt-petre; yet I ſee not, that chymiſts have hitherto given us, or perhaps offered us any cogent proof, that ſhe muſt neceſſarily do ſo. I further obſerve this more conſiderable argument, that, according to what I formerly noted, ſalt-petre diſtilled in cloſe veſſels afforded us but an inconſiderable quantity of fixt ſalt, and that too was but a very imperfect alkali; though the quantity of nitrous ſpirit was great enough to perſuade us, that not any thing near ſo much as was wanting of the entire weight of the ſalt-petre had paſſed into the receiver. And elſewhere I relate, that a friend of mine, with the help of a peculiar clay, obtained near a pound of ſpirit of nitre from a pound of ſalt-petre; whereas, on the other ſide, by a differing way of managing it, and without additament, I obtained, as I there relate, about ten ounces of fixt nitre from a pound of ſalt-petre. Whence it ſeems probable, that the ſame ſubſtance, that in crude nitre is almoſt inſipid, may by an operation of the fire be diſtilled into a highly acid ſpirit, as well as, by another operation and way of management, be brought into the nature of a fixt and cauſtic alkali. It may alſo be worth conſidering, whether, the ſpirits of nitre themſelves, when after being made *ſui juris* they compoſe a

Part I.

diſtinct liquor, and are ſpecificated, may not be deprived of their acid nature, and may become, or at leaſt materially concur to make up, a fixt alkali. For if ſea-ſalt, which chymiſts do not pretend to contain any ſuch alkali, be thoroughly diſſolved in a ſufficient quantity of ſpirit of nitre, and impregnated, this compounded ſalt will, as I have formerly noted, yield a conſiderable proportion of fixt alkali like that of ſalt-petre, which is as likely to proceed from the nitrous, as from the marine part of the reſulting ſalt; and if it do, it will make it the more probable, that it is not neceſſary, that the ſaline corpuſcles of ſpirit of nitre ſhould be primordial bodies, ſince they may be deſtroyed or turned into other ſalts; which is not leſs repugnant to the nature of a principle, than it is to be *de novo* producible from a body, that was not acid before.

SECT. IV.

IT may add much probability to what has been above diſcourſed, concerning the producibleneſs of the differing ſorts of ſpirits, if it ſhall appear, that the ſame body, meerly by different ways of ordering it, may be eaſily enough brought to afford, either acid, or inflammable, or volatile, commonly called urinous ſpirits, as the ſkilful artiſt pleaſes.

AN inſtance of this may be afforded us by ſome legumens, as peas, or beans; which if they be newly gathered and diſtilled in a retort, it will, I preſume, be eaſily granted, that they will, like many other green vegetables, afford, beſides a great deal of phlegm, an acid ſpirit; and if I much miſ-remember not, I had ſuch a ſpirit from either peas, or beans, or both, after they had been kept long enough to loſe their verdure. But if theſe ſeeds be at a fit time duly ſermented with common water, I think it will not be doubted, but that they, as well as other mealy ſeeds, will yield an ardent ſpirit; but it will not ſo eaſily be ſuſpected, much leſs believed, that without adding any thing to them, or meddling with them, barely by keeping them in a dry place, for a certain number of months, they will yield a ſpirit, that by one, that did not know whence it proceeded, would be judged near of kin to the ſpirit of urine, or of hartſhorn, and to other ſaline liquors drawn from animal ſubſtances. For having diſtilled theſe legumens by themſelves, without any additament, and without ſo much as breaking them, they afford me ſpirits, not only far more like in ſmell to thoſe, I have compared them to, than they were either to acid, or vinous ſpirits, but very like them, in more intimate qualities; ſince they would, as the ſpirit of urine and hartſhorn, make a conflict with acid ſpirits, turn ſyrup of violets green, diſſolve copper blue, precipitate a ſolution of ſublimate into a white ſubſtance; and in a word, perform thoſe things, which I many years ago delivered in the tract about colours, and ſeveral ingenious writers have ſince embraced, as the diſtinguiſhing marks of volatile and urinous ſalts, or ſpirits. I ſay ſalts or ſpirits, becauſe I found that theſe drawn from vegetables, as well as thoſe afforded by animals, may eaſily by a dexterous ſublimation be brought to exhibit many of their nobler parts in the form of a dry ſalt, as well as in that of a ſpirituous liquor.

ANOTHER inſtance I ſhall take from the juice of grapes, though chymiſts will perhaps think it ſtrange, that I ſhould undertake to accommodate it to my preſent purpoſe; but there is no great myſtery in the buſineſs: for the freſh juice of grapes or muſt, though ſweet in taſte, will, if it be timely diſtilled to the conſiſtence almoſt of a ſyrup, yield a copious phlegm, but not an ardent ſpirit: if the ſuperfluous moiſture be ſkilfully evaporated, there will remain a kind of rob or ſapa of a pleaſant tartneſs,

neſs, which I have known uſed in ſome places (as eſpecially in, or near *Switzerland*) for an excellent ingredient of ſauces, and alſo, to be ſpread upon bread to be eaten inſtead of butter.

BUT if this raiſinee or ſapa were preſently diſtilled, I ſuppoſe it would yield no vinous, but an acid ſpirit: I ſaid, I ſuppoſe, becauſe for want of vineyards in *England*, I could not examine any liquor taken out of great veſſels of muſt, and therefore cannot ſay preciſely and experimentally, what diſtilled liquors it would afford; ſince I know not certainly, whether the great quantity of the ſweet liquor, and its continuance for ſome time (though not a long one) in the ſtate of what they call muſt, may not ſomewhat alter its productions, but if, as it is probable, that diverſity be not conſiderable, I may ſafely ſuppoſe, that the vinous ſpirit afforded by the juice of grapes, after fermentation has turned it into wine, is a produced thing, and was not in that form pre-exiſtent in the juice; for having purpoſely cauſed ripe grapes to be moderately preſſed, that their juice may, without much dreggy matter, be ſqueezed out, we put this liquor into a glaſs head and body, and diſtilling it with a gentle fire, we obtained a ſcarce credible proportion of phlegm: and then transferring the ſomewhat inſpiſſated remainder into a retort, after having kept ſome of it (which had a grateful mixture of ſweetneſs and acidity) for raiſinee, we proſecuted the diſtillation with a ſtronger fire, and obtained not a vinous, but an acid ſpirit, as we found, not only by its ſmell and taſte, but by its corroding fragments of coral, even in the cold, by its growing ſweet upon minium, *&c.* Agreeably to which experiment, I found by trial, that raiſins, which conſiſt chiefly of the juice of grapes, inſpiſſated in the ſkins or huſks by the avolition of the ſuperfluous moiſture through their pores, being diſtilled in a retort, did not afford any vinous, but rather an acetous ſpirit, that, as an acid liquor, would work upon divers bodies, as ſpirit of vinegar would have done, and yet, as it was formerly noted, it is known, that raiſins being in a due proportion fermented with common water, will, after a certain time, afford a vinous and inebriating liquor; and though this time in the better ſort of the known ways, of making artificial wines, is wont to amount to many months, yet I have practiſed a way (which conſiſts chiefly in a determinate proportion of the water to the raiſins) by the help of which, the liquor in very few weeks becomes fit to drink, and conſequently to afford by diſtillation a vinous ſpirit; but this only upon the by.

I SHALL now add, which probably you will think ſomewhat ſtrange, that from the juice of grapes even after it has been duly fermented, there may be obtained a diſtilled liquor, which having not found mentioned in authors, I thought, that I might take the liberty to name, and upon the account of its taſte, and ſome other qualities, to call it the acid ſpirit of wine: to ſatisfy you therefore, that there is ſuch a liquor, I will not make uſe of Rheniſh wine or other wines, that are thought to reliſh of acidity, but I will acquaint you with an experiment, that I choſe to make upon ſack, as a wine fully ripe, and more remote from an acid and tartarous nature, than thoſe are wont to be, that are made in leſs hot climates. We took then ſome good ſack, and having a digeſtive furnace, and in a glaſs-head and body, ſlowly drawn off the ardent ſpirit firſt, and then the phlegm (which even in this generous wine was copious) till there remained a liquor of the conſiſtence of a ſomewhat thin ſyrup, we removed it into a retort, and diſtilled it by degrees of fire, whereby we obtained, beſides a ſouriſh phlegm, that came firſt over, a true acid ſpirit, as appeared not only by the taſte, but by the hiſſing noiſe and numerous bubbles, that were produced, when we poured it upon a lixiviate ſalt; as alſo by this, that having put it upon powdered corals, it began briſkly to diſſolve them, even in the cold: we likewiſe made it corrode ſome metalline, and mineral bodies, of which it is not here neceſſary to give you an account, no more than of the black ſubſtance, that remained after the diſtillation: only two things I will here intimate about them: the one, that as this acid ſpirit of wine has its origination differing from that of other known acid ſpirits, and even from ſpirit of vinegar, ſo I thought it not irrational to conjecture, that it might have ſome peculiar qualities, whoſe diſcovery I leave you (if you think it worth the while) to proſecute: only by way of encouragement, as well as hint, I ſhall tell you, that having put ſome of it, for a certain reaſon, upon filings of copper, in ſuch manner, that ſome of them, after being wetted with the ſpirit, ſhould remain expoſed to the air, and others lie beneath the liquor, I found, though the trials were made in the cold, that in a day or two the expoſed filings had gained a fine bluiſh-green colour, but the ſpirit, that ſwam upon the other filings, did in few hours acquire a fine redneſs, which afterwards in two or three days degenerated into a colour like that of the expoſed filings. The other thing I will note, relates to the *caput mortuum* of the diſtilled wine, which I found a more fixt body than one would have expected, and it is, that though probably the finer part belonging as to other vegetable mixts, ſo to the juice of grapes, being attenuated and ſubtilized, was changed into an ardent ſpirit, and therefore appeared not in the diſtillation, in the form of oil, yet it is not unlikely, that the coarſer part of the oleaginous ſubſtance remained ſtill in the *caput mortuum*: for holding it in the flame of a candle, I obſerved, that it would partly exhale in thick ſmoke, partly melt, and as it were fry, and partly burn with an actual flame, which was not only continued, while the flame of the candle cheriſhed it, but would after it was removed from the candle, continue a pretty while to flame upon its own account, and a parcel of it being caſt upon quick (but not upon flaming) coals, burned with a blaze, almoſt as if it had been amber, or bitumen. I could here tell you of fine cryſtals of wine, that I many years ſince made by a peculiar way, of the above mentioned extract of ſack; but this may be elſewhere more fitly mentioned.

To return therefore to our juice of grapes, we ſee, that meerly by a ſeemingly ſlight difference in the management of it, it may be made to afford either a vinous, or acid ſpirit; and I ſhall now add, that it may alſo be brought to yield a volatile or urinous one; for it is known, that in proceſs of time, wine affords tartar; and though chymiſts ſuppoſe the ſpirit of tartar to be of a quite differing nature from that of urine and of ſoot, and though I have elſewhere ſhewn, that tartar diſtilled the common way affords a double ſpirit, namely, an acid, and another, that I thought fit to call anonymous; yet I elſewhere ſhow, that by a peculiar and ſlow way of operating, I have been able to obtain (though perhaps not conſtantly) from crude tartar without any additament, a ſpirituous ſubſtance, that in taſte, ſmell, and divers manifeſt operations, much more reſembled the volatile ſpirit of urine, or rather that of ſoot, obtained as mine of tartar was, by meer diſtillation, than an acid ſpirit: with which (ſo little did they agree) it was diſpoſed to make a conflict as ſoon as they were put together. But ſuch a kind of volatile ſubſtance may be far more eaſily obtained from the lees of wine, than from tartar; for having been accidentally informed, that an expert chymiſt in *Germany* had found the way to get ſtore of volatile ſalt from lees of wine, I reſolved to try, whether it might not be done without any addition; and having procured ſome of the beſt lees (I could get) of Rheniſh wine, I cauſed them to be expoſed in broad veſſels to the ſun, and the free air (which circumſtance yet I am not ſure is neceſſary) that they might leiſurely be dried, if not alſo be impregnated, in order to the volatility of their ſaline parts. Then theſe dried fæces being carefully diſtilled in a retort by degrees of fire, the liquor was ſlowly rectified, by which means there aſcended before the phelgm a ſpirituous part, which would turn ſyrup of violets green, precipitate diſſolved ſublimate into a white powder, ſoon colour itſelf upon copper with a deep blue, and in ſhort do ſeveral things, by the performance of which we have elſewhere diſtinguiſhed volatile ſalts and ſpirits from acid and from vinous ones. By all ſuch trials upon the juice of grapes, we may infer the truth, we intended to prove by them, namely, that the ſame matter, as it is differingly managed, may be made to afford an acid (beſides one, that is truly acetous) a vinous, and a volatile ſpirit: beſides that, *ex abundanti*, it may alſo be made to yield, as I have noted in mentioning the diſtillation of tartar, another ſort of ſpirituous ſubſtance, as yet anonymous.

New obſervations about the adiaphorous ſpirits of Woods and divers other bodies.

AND now having fallen upon the mentioning of this ſort of ſpirits, that I have called anonymous, ſince I remember not that the notice I gave the public of them * has engaged any writer to examine them; I am content on this occaſion to touch upon ſome of the more quick and eaſy trials, that I have made about this kind of liquors, that I may both excite and ſomewhat aſſiſt the curioſity of thoſe enquirers, that ſhall attempt to make a farther diſcovery of the nature of theſe ſpirits; which when I firſt ſeparated from the acid ſpirits, wherewith chymiſts had before confounded them, as ſtyling them, and taking them to be meerly the acid ſpirits of tartar, wood, *&c.* their properties were ſo little known to me, that I contented myſelf to ſtyle them anonymous ſpirits: but ſince having found them to differ in divers qualities, both from vinous, from acid, and from urinous ones, and having not ſufficiently diſcovered their poſitive properties, I was wont to give them a negative appellation, and call each of them the neutral or adiaphorous ſpirit of the body, that affords it, (whether it be tartar, wood, or any other like concrete.)

BUT before I deſcend to particulars, it will not be improper to premiſe in general three or four things not unfit to make way for the obſervations, that are to follow them.

1. I KNOW not, whether it will be requiſite to repeat in the firſt place, that our adiaphorous ſpirit may be obtained by diſtilling the liquor, that is afforded by woods and divers other bodies; by diſtilling this liquor, I ſay, from corals, or calcined lead; for by this means the acid corpuſcles of the menſtruum will work upon the coral or the lead, and ſo faſten themſelves to what they corrode, that they will eaſily enough part with the adiaphorous ſpirits, which by this means are permitted to aſcend by themſelves and fall into the receiver, in the form of a liquor. This, as I was ſaying, I know not, whether it be neceſſary to inſiſt on in this place, becauſe I have already mentioned it in another paper: but I think it may be very pertinent to relate here, that I endeavoured to try, whether there was not a difference in gravity or fixedneſs between the acid and neutral ſpirit of wood, without mortifying the firſt; and whether by the help of this gravity and fixedneſs, I might not be able to ſeparate, at leaſt in great part, the acid from the other, and ſo preſerve it in its diſtinct nature.

In order to this, I cauſed a pretty quantity of the rectified ſpirit of box to be ſlowly diſtilled in a glaſs body and head, placed in a ſand cupel with the flame of a

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* This was done in the *Sceptical Chymiſt.*

lamp, as that, which would give a more gentle and regular heat than charcoal; as indeed in the firſt twenty-four hours or thereabouts, this furnace afforded but about two ſpoonfuls of liquor; and though the menſtruum firſt put in ſcarce exceeded, by our gueſs, one pint or pound (if it were ſo much) yet it was divers days and nights in drawing over. And in this operation the moſt obſervable circumſtances were theſe two: Firſt, that the liquor, that firſt aſcended, was not phlegm, but had a very penetrant taſte, yet without any manifeſt acidity, diſcoverable by the tongue, though by putting it upon fine powder of coral (whether crude or calcined I remember not) it had ſome operation, that made me think it not altogether devoid of acid particles. Secondly, having often ſhifted the receiver, the better to judge, whether the portions of the aſcending ſpirit were conſiderably different in quantity, I found that, towards the latter end, the liquor, that came over, was ſharper than before; and having, at length, diſtilled all I could make to ariſe, we found the laſt parcel of liquor (which was copious enough) to be of a good yellow colour (though thoſe that preceded it were limpid enough) and both to ſmell ſtrong of vinegar, and to taſte more acid upon the tongue than ſpirit of common vinegar itſelf: ſo that if I had not known, how it was obtained, I had ſuſpected it to be what the chymiſts call *acetum radicatum*; and accordingly I found it to be a very active menſtruum in the diſſolution of ſome bodies, that, for trial-ſake, were put into it. All which ſeems to argue, that the acid portion of ſuch diſtilled liquors, as I have been ſpeaking of, is more ponderous, or more fixt, than the adiaphorous ſpirit, which, upon this account, may be in great part ſeparated from it by bare diſtillation, if it be warily enough made.

My ſecond general remark ſhall be, that I have obſerved theſe neutral ſpirits to be not all of them, in all things, of the ſame nature; ſince though they agree in ſome general attributes, which ſuffice to entitle them to the ſame ſpecies or denomination, yet they ſometimes differ from one another in particular qualities: which advertiſement I thought it neceſſary to premiſe, that it may not ſeem ſtrange, and that I may not be blamed, though ſome of the trials I ſhall ſet down do not punctually ſucceed in their hands, that ſhall not make uſe of the anonymous ſpirit of box, which I employed; not becauſe I think it better than any other, but becauſe amongſt divers, that I have made uſe of, I had then a greater quantity of it at hand. But though for this reaſon, when I ſhall ſpeak what I have obſerved in an adiaphorous ſpirit, without naming it, I would be underſtood of the ſpirit of box, which I had freed from its acid mixture by diſtilling it from calcined coral; yet I ſhall not ſo confine myſelf to this, as not to mention, now and then, ſome other ſpirit of the ſame family.

The third general obſervation, that I ſhall make about our adiaphorous ſpirits, is, that though the new chymiſts, that have taken any notice of the diſtilled liquors, for example, of woods, were wont, by reaſon of their acid taſtes to look upon them, as of a merely acetous nature, and having accordingly called them the vinegars or acetous ſpirits of wood, yet really the acid portion of theſe diſtilled liquors is far from being the greateſt: for beſides what other trials I have purpoſely made, I remember I took eight ounces of the rectified ſpirit of box, wherein the acetous and neutral ſpirit remained confounded, as they had been in the firſt diſtillation; and having poured this upon a quantity of calcined coral, ſufficient to ſatiate the acid corpuſcles (which quickly fell to corrode it with noiſe and bubbles) we gently diſtilled it to a dryneſs in a glaſs head and body, by which means we obtained of adiaphorous ſpirit but eight grains leſs than ſeven ounces and an half, and ſome of the menſtruum having been waſted in the operation, the acid corpuſcles remaining in the bottom, with the coral they had corroded, weighed but between two and three drachms; which ſhews, that notwithſtanding the not contemptible quantity of ſtrong ſpirit of vinegar, that by our lately recited obſervation the diſtilled liquors of box does contain, the corpuſcles, that make it ſo acid, being concentred, take up but a little room. And ſince it was rational to ſuſpect, that the acetous corpuſcles being made without fermentation, might have ſomething peculiar in their nature, I cauſed them to be gradually diſtilled with a ſtrong fire from the coral, and thereby obtained a very red ſpirit, of which, though many chymiſts would take it for a volatile tincture of coral, I ſhall only obſerve, that its ſmell was very ſtrong, and its taſte exceeding penetrant, but very differing from that of acid liquors.

WHETHER our adiaphorous ſpirit may (as I ſometimes ſuſpected it may) be generated, by a commixture of the finer parts of the oil of the wood reduced to an extraordinary ſmallneſs, and thereby capable of being exquiſitely mixt with the phlegm, and ſtrictly aſſociated with its particles, I ſhall ſtay, till I be better furniſhed with experiments, before I venture to determine.

HAVING premiſed the foregoing general obſervations, I ſhall proceed to particular ones, as ſoon as I ſhall have advertiſed you, that for the better diſcerning the phænomena to be produced, I choſe to make almoſt all the following trials in cylindrical glaſſes of about an inch in diameter.

To the phænomenon I am about to take notice of, I therefore give the firſt place, amongſt thoſe produced by the help of our adiaphorous ſpirit, becauſe it is uncommon, and not unpleaſant: for though we have many experiments of the ſudden tranſmutation of colours, whereby we change one into another; yet we have very few of the production of colours *de novo*, in bodies, that were colourleſs before. And I remember not, that the writers I have ſince met with, have added any experiments of this kind to thoſe three or four, that I have mentioned in the hiſtory of colours.

1. I SHALL begin then with obſerving, that having into our adiaphorous ſpirit of box dropt a convenient quantity of ſtrong and tranſparent oil of vitriol, and ſhaking the liquor together, there preſently emerged a rich and lovely red colour; which at firſt was diaphanous, but afterwards grew ſo deep, that it was opacous, though by ſhaking the glaſs, the thin liquor, that would ſlowly glide down the inſide of the glaſs, being held againſt the light, manifeſted, that the colour was ſtill red, though much more dark and muſcadine-like than before.

2. SOME common aqua fortis being put to our neutral ſpirit, and ſhaken a little with it, preſently gave it a rich amber or high yellow colour, but not a true red: but if the liquors were not mingled by agitation, the ſpirit did but ſlowly and gradually obtain the above mentioned colour, which was ſomewhat deeper than that of ſack: after this change the liquor continued tranſparent, and which is a circumſtance not to be omitted) the change at firſt was wrought without any manifeſt precipitation, though afterwards, when the mixture had reſted a good while, there appeared ſome little and light fæculency at the bottom of the glaſs, and the inſide of it, as far as the liquor reached, was ſullied with a cloudineſs not eaſy to be waſhed off. One circumſtance more of this trial I muſt not omit, which is, that notwithſtanding the ſtrong and offenſive ſmell, that is wont to be juſtly complained of in aqua fortis, in our mixture it was either none, or but very faint, being concealed (if I may ſo ſpeak) or ſuppreſſed, and partly perhaps diſguiſed by the predominant odour of the adiaphorous ſpirit.

3. SOME ſpirit of ſalt being mingled with our ſpirit of box, the mixture became much leſs diaphanous than the liquors had been before their conjunction, and for a

day or two was only whitiſh; but when we removed it into a digeſtive furnace, and kept it there for many hours, it acquired a colour high enough, partaking of brown and yellow, and appeared to have let fall ſome little ſediment to the bottom of the glaſs.

4. Having put ſome of our adiaphorous liquor on ſalt of tartar, it had not any ſenſible operation on it that we took notice of, ſave that it diſſolved the ſalt; and, after ſome digeſtion, appeared of a yellow colour tending to brown, and faſtened to the inſide of the phial in many little grains of ſalt, that ſeemed to have been firſt diſſolved, and then coagulated again in newly-emergent figures.

5. Our adiaphorous liquor being confounded with high rectified ſpirit of wine, neither of them appeared to change colour much (for ſome change there was towards yellowneſs) or be opacated by their conjunction, even after ſome days digeſting; but the vinous ſpirit did not hinder the other from being turned red by the action of ſome potent acid, when it was poured on the mixture.

6. Rectified ſpirit of urine being put to our adiaphorous liquor did not make any conflict with it, but joined with it quietly, as the above mentioned ſpirits had done, and did not manifeſtly change the colour of either of the liquors, whilſt they were kept many hours in the cold; but being transferred into a digeſtive furnace, and kept there a night or two, the liquor acquired a high colour, which was almoſt orange-brown, and there appeared ſome little fæces at the bottom. Having made theſe trials upon our ſpirit with ſimple liquors, I thought fit to make ſome with ſuch compounded liquors, as the ſolution of metals are, to ſee if our ſpirit, though neither manifeſtly of an acid, or an urinous, or a lixiviate nature, would procure precipitations of any part of the diſſolved metals.

7. In proſecuting this enquiry, I dropt into ſome of our ſpirit a little ſolution of refined gold, which at firſt imparted thereto its own colour (perhaps ſomewhat heightened) but the mixture quickly loſt its tranſparency and grew muddy, and, after a while, let fall a conſiderable quantity of ſediment or precipitate, the ſupernatant liquor having acquired a browniſh colour.

8. Having mixt our ſpirit with a good ſolution of crude lead, made with an appropriated menſtruum, that diſſolves it readily and clear, almoſt as aqua fortis does common ſilver, the mixture preſently grew muddy, and at length, after ſome days, let fall a copious ſediment, over which ſwam a liquor between brown and red.

9. We put to our ſpirit of box ſome fine ceruleous tincture or ſolution of copper, made with an urinous ſpirit (as of putrified urine, or ſal armoniac) and ſoon perceived the mixture to grow troubled, which afforded us, though but very ſlowly, a copious reſidence.

10. We mingled with our ſpirit a convenient quantity of ſtrong infuſion of ſublimate made in fair water, but found not any manifeſt re-action between thoſe liquors, no more than we did between dry and undiſſolved ſublimate and the ſame ſpirit, when we kept them together in this ſame phial.

11. Mixing our ſpirit with oil of tartar *per deliquium*, there did not ſuddenly appear any manifeſt change; but having digeſted the mixture for ſeveral days, there precipitated a light fæculency; and the ſupernatant liquor, which was tranſparent, appeared of a colour inclinable to red.

12. We alſo mingled with ſome of our ſpirit a convenient quantity of vitriol of copper diſſolved in fair water, till the liquor ſeemed ſatiated with the vitriol; but I remember not that in ſome days, but the ſolution grew manifeſtly opacous or diſcoloured.

13. We put to our ſpirit a ſolution of tin, made in a menſtruum, that diſſolves it clear, and found very little alteration to enſue, though we left the liquors many hours together.

14. But when I put to our ſpirit a convenient quantity of the ſolution of mercury, made in aqua fortis, the colour of the mixture became firſt deeply yellow, and in a minute or two intenſely red; and being digeſted for ſome days, I found at the bottom of the phial a white precipitate, much more copious than I expected, and the tranſparent liquor, that ſwam above it, was of a rich golden colour. Whether phyſicians or ſurgeons ſhould think fit to employ this precipitate, or this tinged liquor for medicinal purpoſes, I ſhall leave them to conſider.

15. Several of the foregoing experiments were tried with the ſpirits of other woods than box, and in particular with thoſe of oak and guaiacum, the phænomena of which experiments were not always the ſame with thoſe above recited, which may probably argue ſome difference in the nature of ſuch ſpirits, as well as there is in the conſtitution of the woods, that afforded them; nor for certain reaſons have I thought fit to recount here all the trials I have made with the adiaphorous ſpirit of box itſelf, of which ſort I ſhall for example-ſake name only two, which I remember as having been the lateſt I made; whereof the firſt was, that having put ſome of our neutral ſpirit upon ſome pieces of fine red coral, and kept them there many days, the liquor did not appear to have extracted any tincture from them, though the upper part of the higheſt fragments ſeemed to be turned white. And the other was, that having taken a parcel of ſpirit, that came over by rectification in a lamp furnace, long before the more fixt acetous ſpirit came to aſcend, and having purpoſely expoſed a phial ſcarce half full of it, in a very ſharp froſty night, in a garden covered with ſnow and ice, it was taken up the next morning, not at all frozen, but leſs limpid than before, and this little opacity did (ſomewhat to our wonder) remain more or leſs for ſome weeks after.

ABOUT THE PRODUCIBLENESS OF SULPHURS.

PART III.

THOSE ſubſtances, that chymiſts are wont to call the ſulphurs of the mixt bodies, that by the help of the fire, are brought to afford them, are not of ſo uniform a nature, as might be expected in the portions of the ſame principle. For as, on the one ſide, chymiſts make inflammability to be the conſtituent character of ſulphur, ſo on the other ſide, it is obvious enough to thoſe, that are any thing verſed in ſpagyrical operations, that there are at leaſt three ſubſtances manifeſtly differing in conſiſtence, texture, or both, that, according to the notion lately aſſigned, ought to be referred to ſulphur. For ſometimes the inflammable ſubſtance, that is obtained from a mixt body, by the intervention of the fire, appears in the form of an oil, that will not mingle with water; ſometimes in the form of an ardent ſpirit, that will readily unite with that liquor; and ſometimes alſo in the form of a conſiſtent body, almoſt like common ſulphur.

NOTWITHSTANDING theſe various forms, in which it appears, it is not impoſſible, but that in many mixt bodies, not to ſay in all, what is called ſulphur may be no primordial ingredient, but rather a generated or reſulting thing. For that, which is common to theſe differing bodies, that paſs under the name of ſulphurs, and which is the conſtituent quality (if I may ſo call it) that diſcriminates them from the other material principles of mixt bodies, muſt be confeſſed, if we will ſpeak intelligibly, to be inflammability, or, if you pleaſe a diſpoſition to be turned into fire, and uſually alſo into flame. Which being premiſed I conſider here, that ſulphur itſelf is made of the ſame univerſal matter, whereof other bodies conſiſt, and is but a coalition of certain particles thereof, whoſe aggregate, by having ſuch a contexture, motion, *&c.* acquires thoſe properties, for which a body is called ſulphur. And therefore if the like contexture happen to be found in other portions of matter, or (to expreſs myſelf more fully) if art, or chance can frame and bring together particles of matter, and give them ſuch a contexture as is apt and ſufficient to diſpoſe them to be kindled and flame or burn away; theſe qualifications of ſuch an aggregate of corpuſcles will ſuffice to confer on it the nature of a ſulphur, whether this portion of matter do, or do not conſiſt, or copiouſly participate, of the chymiſts primeval ſulphur. For it is not by virtue of the long preceding duration of a thing, but by that of the eſſential qualities belonging to it, that a body deſerves this or that denomination. As the ſnow, that fell yeſterday, and was generated in a trice, is as true ſnow, as that, which has lain, perhaps for many years, on thoſe *Alps*, that are always covered with ſnow, or on the higheſt mountains of the frigid zone. And in the judgment of the chymiſts themſelves, a pound of quickſilver recently tranſmuted by a grain or two of their elixir into gold becomes as true gold, as that, which was coeval with the mountains, where nature has formed the antienteſt mines of that metal.

SECT. I.

Of the production of oils.

THAT it is not neceſſary, the oils, or ſulphurs obtained by the fire from mixt bodies, ſhould be a primeval element or principle, may be probably argued from the experiment mentioned in the *Sceptical Chymiſt* about the growth of plants nouriſhed by meer water, which nevertheleſs, by diſtillation, afforded an oil. And we ſee, that in almond trees, walnut trees, and divers others, the rain water, that inſinuates itſelf into their roots, is, by ſucceſſive changes of texture, reduced into the oil, which the fruit by expreſſion ſo plentifully affords. And to confirm our experiment from the growth of plants by tranſmuted or aſſimilated water, to obviate the ſuſpicion of common water's being impregnated with the groſſer juices of the earth, I employed diſtilled water. About which experiment I find this ſhort memorial among my *adverſaria.*

[A SPRIG of mint put into rain-water, diſtilled, and fed almoſt wholly with rediſtilled rain-water, weighed *July* 15. gr. 3. and was taken out *Auguſt* the 14th, and being well dried with paper and a cloth, weighed 10 grains and about a quarter: ſo that within leſs than a month it grew to be three times as heavy, as when it was firſt put in. Another put in, and taken out at the ſame time with the former, had attained within leſs than a month to near four times its firſt weight, and had ſhot out a ſecond ſprig much higher than the firſt, and ſtore of roots, ſome of them near as long again as the whole plant, when it was firſt put in.]

If we conſider, what a great quantity of oil is afforded by an olive-yard, whoſe trees are probably, as well as thoſe that bear apples, cherries, and other kinds of aqueous fruits, nouriſhed chiefly by rain-water, that being imbibed by the roots is by various digeſtions, or preparatory changes, turned into oil in the olive, it will not appear unlikely, that oil may be produced of other ſubſtances: ſince in our inſtance it ſeems to have been made by tranſmutation of water, though this be generally reputed to be of all liquors the moſt contrary to it, and is evidently of a nature exceeding diſtant from it.

And here I ſhall relate an experiment, by which I attempted to produce it, out of only two diſtilled liquors, that according to the common eſtimation of chymiſts are uncompounded bodies; and, whether they be really ſo or not, are each of them readily diſſolvable in water, and in one another. Take then of oil of vitriol, and of ſuch ſpirit of wine, as is totally inflammable, an equal weight; mix them together by degrees, leſt the heat they will produce ſhould breed ſome inconvenience, and having digeſted them a good while (which yet is not abſolutely neceſſary) with a very wary management of the fire (for elſe the experiment will eaſily miſcarry) draw off what will come over: and if you go to work, as I have ſeveral times done, you ſhall obtain, beſides a ſubtile and odoriferous ſpirit of wine and an acid ſulphureous liquor, a conſiderable quantity of chymical oil, which I have had ſometimes deeply coloured, ſometimes clear like fair water: and this oil you will perchance look on as an odd liquor, when I tell you, that I have had it, ſometimes exceeding fragrant, and (though the oil of vitriol be ſo highly corroſive) without any acidity at all, the taſte of it being very ſubtile and penetrant, but no way like that of any ſaline liquor, that we know. This hath ſometimes inticed me to doubt, whether it hath been made of the ſpirit of wine, or of the oil of vitriol. The circumſtances laſt mentioned ſeem to perſuade the former; eſpecially if I add to them, that I found by trial purpoſely made, that this oil would readily mix with good ſpirit of wine, that had never had to do with oil of vitriol; but on the other ſide it ſeemed conſiderable, that the oil of vitriol by this operation was much weakened and changed, and it appeared not, whence the ſpirit of wine ſhould have ſo great a fragrancy; which conſiderations were backed by this more weighty argument, that this oil was ſo ponderous, as to ſink not only in common water, which is yet a far more heavy liquor than pure ſpirit of wine, but in the acid ſpirit itſelf, which ſeemed to be the remains of the altered oil of vitriol, which, by reaſon of its abounding in ſalt, you will eaſily grant to be far heavier than common water. But I need not much trouble myſelf, to determine, which it is of the two liquors, that affords this ſtrong oil; for it may well be (though not equally) compoſed of both, by their mutual action, and the operation of the fire, united in the form of oil. And if it be objected, as probably it will, that this inflammable ſubſtance is made but by extrication of the parts, that lay concealed in the liquors before they were brought together, it may be anſwered, that this ſhould not be ſuppoſed, but proved; which till it be, our cauſe will be favoured by our experiment, wherein there appears nothing ſo likely as a change of texture; to which may be aſcribed the production of our anomalous chymical oil, ſince this plainly ſeems to reſult from two bodies, whereof neither was a true oil before. For each of them would readily mingle with water, whereas this produced oil of ours, being ſhaken with water, would break, like common oils, into numerous little globules; which would preſently after ſink to the bottom, and re-unite there into a liquor, which for trial-ſake I have kept divers weeks in the water, and found it at laſt undiſſolved by it. Some odd properties of this oil make it ſeem likely to participate of ſome of the nobler parts of vitriol, and the ſulphur of that metal having extraordinary virtues aſcribed to it, by ſome of the famouſeſt and intelligenteſt Spagyriſts (as *Baſilius Valentius*, *Helmont*, &c.) I kept ſome quantity of this oil by me for ſeveral years, to obſerve, as I did with pleaſure, the alterations, that time would produce in it, and afterwards I imparted either ſome of the medicine itſelf, (whereof the firſt trial proved very ſucceſsful) or the ways of preparing it, or both, to ſome ingenious men, who (I am told) did not all of them remember me in the free mention they made of it.

But this concerns not our argument, upon occaſion whereof I ſhall obſerve upon the by, that though chymiſts ſhould be able to prove, that our oil was but ſeparated from the ſpirit of wine, or the oil of vitriol, in which it was latent before, yet ſtill the experiment would afford me a conſiderable reaſon for queſtioning a main point in the doctrine of the vulgar chymiſts, who confidently pretend to prove from the number of ſimilar ſubſtances (as they ſuppoſe them) obtained from a mixt body, that it was actually compounded of juſt ſo many diſtinct and true material principles, and ſuch a quantity of each. For if from a diſtilled liquor, as the oil or rather ponderous and acid ſpirit of vitriol, or from alcohol of wine, which is commonly reputed to be uncompounded, a liquor of quite another kind may be (not made but) ſeparated, how little reaſon have we, to take it for granted with the chymiſts, that every diſtilled liquor, that they look upon as one of the component principles of the body, that afforded it, is a homogeneous ſubſtance not further diviſible into differing parts?

SECT. II.

Of the production of inflammable ſpirits.

AFTER what has been hitherto delivered concerning the production of oils, I ſhould now proceed to that of another ſort of liquors, referred by the chymiſts of the principle they call ſulphur; though better known to others by the name of inflammable ſpirits. But of theſe I ſhall purpoſely forbear to diſcourſe in this place, and rather refer to what I have ſaid to them in another, where I thought it more proper to conſider them; namely, in one of the ſections of that part of theſe notes, that treat of the producibleneſs of vinous ſpirits.

SECT. III.

Of the production of conſiſtent ſulphur.

IF you ſhould here tell me, as perhaps you will do, that what I have been hitherto ſaying relates but to inflammable liquors, whereas ſulphur, in its moſt proper and primary acception, ſignifies a mineral body; I ſhall anſwer, that, as I formerly intimated, the chymiſts uſe the term ſulphur ſo ambiguouſly, and ſo uncertainly, that they have made it difficult for other men's diſcourſes to avoid all appearance of participating of the confuſedneſs, they ſeem to have affected in theirs. But becauſe the moſt intelligible, and leaſt indefinite notion their writings ſuggeſt of ſulphur, is, that it is a combuſtible and inflammable principle; I have hitherto treated of it as ſuch. And as for that ſulphur, that is commonly known by that name, and bought in ſhops, though I know there are ſome chymiſts, that have affirmed, that from vegetables and animals they can ſeparate ſuch a ſulphur; yet ſince they are not wont to teach us the way of doing it, nor give us any proofs, beſides their own word, of their having ever done it themſelves, the thing has ſeemed ſo improbable, that I find few or none of the more judicious of their own party, that look upon it, as other than a brag: only a follower of *Glauber* I find to have undertaken, by his maſter's directions, to produce a real ſulphur, like the mineral, out of vegetable charcoal, by a way, which, becauſe it has deceived more than him, and is ſpecious enough to impoſe upon thoſe, that either are not chymiſts, or, if they be chymiſts, are not cautious men, I ſhall here ſet down, and examine, as after the author I made it.

WE took then equal quantities (ſuppoſe a pound of each) of good oil of vitriol and of common ſea-ſalt, diſſolved in as much water as was requiſite: this mixture was ſlowly diſtilled, till the bottom was throughly dry (which it will not be ſo ſoon as it begins to look white, or appear coagulated) then ſetting aſide the liquor (whereof the firſt part that came over was phlegm, and the other part ſpirit of ſalt) we took out the *caput mortuum* (which, if one pleaſes, may be purified by being diſſolved and philtered) and having beat it to powder with about one fourth or one eighth part of its weight of charcoal, we put it in a ſtrong crucible, and kept the mixture melted in a vehement fire, till it grew of a dark reddiſh colour, for by that time ſuch a change was made in the maſs, that it both ſmelt and taſted rankly enough of ſulphur; and if ſpirit of ſal armoniac were ſeaſonably diſtilled from it, with a competent, but not over-haſty fire, the aſcending ſpirit would be manifeſtly impregnated with ſulphur not difficultly ſeparable, which may alſo be divers other ways obtained from the ſame fixt *caput mortuum*.

BUT for all this ſpecious operation, I do not take the ſulphur, thus produced, to have been the vegetable ſulphur of charcoal, but a mineral ſulphur, that lay concealed in a liquid form among the ſaline parts of the oil of vitriol.

FOR, firſt, it is not likely, that ſo ſmall a quantity of charcoal, as was employed in this experiment, and much leſs that ſo ſmall a quantity, as may ſuffice to make it, could contain ſo much ſulphur, as may this way be obtained.

NEXT, that common vitriol is not deſtitute of mineral ſulphur, may be eaſily conjectured by the ſulphureouſneſs of the marchaſites, whereof it is wont to be made. In ſo much that in divers countries, as about *Liege*, and in ſome parts of *Italy*, from the ſame ſubſtance, that affords them vitriol, they obtain by ſublimation great quantities of common ſulphur, which is ſold for ſuch into divers other countries. And I have found by trial, and do not at all think myſelf in that ſingular, that one may obtain from vitriol an oil, and a *caput mortuum*, which being put together, afforded a ſmell of common ſulphur ſo ſtrong, that I was ſcarce able to indure it.

AND to come yet more cloſe to our experiment, I have (as I have elſewhere mentioned) purpoſely tried, more than once or twice, that by diſtilling together common oil of turpentine and common oil of vitriol, the former of thoſe liquors would make a ſeparation of ſome of the ſulphur, that lay concealed in the latter, and as it were extricate and extract it: ſo that beſides an exceedingly ſulphureous liquor, which ſometimes was made white by the copiouſly diſſolved (and partly precipitated) ſulphur, that paſſed into the receiver, we had in the neck of the retort a yellowiſh conſiſtent body, which being put upon a quick coal, would, after a little yellow flame (probably proceeding from ſome adhering parts of turpentine) afford good ſtore of bluiſh flame, like that of common ſulphur, which it alſo emulated in its ſmell. And ſuch a kind of ſulphur I have alſo ſeen, in tract of time, ſettle itſelf, in no inconſiderable quantity, at the bottom of the liquor, diſtilled from the mixture of the two above mentioned oils. Nor are theſe the only ways, by which I have obtained from oil of vitriol manifeſt proofs of its containing a mineral ſulphur very like to common ſulphur.

And in particular it now comes into my mind, that I once put into a retort, together with one part of running mercury, four parts of oil of vitriol, and having diſtilled off the menſtruum, by degrees of fire, there remained at the bottom of the glaſs a very white powder. This calx, being afterwards gradually preſt with a ſtronger fire, afforded in the upper part of the retort a great many ſmall bodies, that looked like half beads of amber, and ſeemed to be of very fine ſulphur, (but were afterwards confounded with many other aſcending corpuſcles.) This amber-like body (which was ſomewhat copious, and, as to ſome portions of it, whitiſh) by its readineſs to be melted, by its ſmell, and by the bluiſh flame it afforded, when it burned, appeared to be a kind of ſulphur, which you will eaſily grant to be far more unlikely to have proceeded from ſo homogeneous a body, as the quickſilver, than from the oil of vitriol, which we have already ſhewn to conſiſt of divers ſulphureous as well as many acid corpuſcles. And on this occaſion I remember, that, whereas upon mingling the oils of turpentine and of vitriol in a due proportion, I have conſtantly obſerved, that they incorporated into a mixture, that was deeply red (and this may eaſily be tried by letting fall two or three drops of oil of vitriol upon ſome drops of that of turpentine, and mixing them in a concave veſſel, or even in a hollowed piece of paper) whereas, I ſay, I obſerved this, I was thereby induced to ſuſpect the chymical (for I ſay not, the optical) cauſe of that phænomenon might be, that the terebinthinate oil had made a ſolution of divers ſulphureous particles it met with in the oil of vitriol, and by that means acquired ſuch a redneſs, as we ſee that common flower of ſulphur gives to the oil of turpentine, when it is diſſolved in it. And to examine this conjecture, I found, that divers other chymical oils, and oil of aniſeeds itſelf, as remote as it is from redneſs, would preſently acquire that colour, being carefully incorporated with a due quantity of oil of vitriol. But this conjecture is propoſed only upon the by.

As for the ſulphur of minerals and metals, I confeſs, I have not yet found enough, either in chymiſts books, or in my own experience, to make me willing to ſpeak dogmatically about them. And this the rather, becauſe firſt, as to the ſulphurs, that are ſometimes obtainable from ſome of the minerals, I think it may be doubted, whether they belonged to thoſe minerals as eſſential ingredients, or were only corpuſcles of common ſulphur, perhaps a little altered, that were mingled in the bowels of the earth, with other parts, that are eſſential to the nature of the mineral. As we ſee, that in native cinnabar the mercury, which, according to chymiſts, is a complete metal by itſelf, is ſo mixt with another body, as not to be diſtinctly diſcernible, till it be ſeparated by the fire. And this cinnabar affords me an inſtance, the more fit for my preſent purpoſe, becauſe I have ſometimes by an eaſy way obtained a ſulphur alſo from it: and ſince we have lately noted, that the vitriolate marchaſites afford great ſtore of common ſulphur, by a groſs way of ſeparation, it ſhould not ſeem irrational to ſuſpect, that ſome common ſulphur may remain more cloſely mixt with the ſaline and metalline parts of the vitriol afforded by the ſame marchaſites; from which latent corpuſcles of ſulphur may in part proceed the ſulphureous ſmell, and other like things, that we have lately taken notice of in vitriol, and its oil. And perhaps by the ſame conſideration one may account for the ſulphureous qualities, that are ſometimes to be met with in the liquors, that paſs for the vinegars of mineral bodies, and particularly antimony; to which may now and then be added ſome metalline ores: ſince, I remember, I have had ſuch a ſulphureous liquor from good lead-ore, that I had ordered to be purpoſely digged out of the mine at *Minedeep*, and being put in cloſe veſſels ſpeedily conveyed to me. And that nature herſelf may blend an imperfect mineral with lead, I have had occaſion to obſerve by an ore, whereof the owner found a mine, but not being able to diſcover what it was, deſired me to inform him. For this gave me occaſion to conſider the ore (whereof I have yet a lump by me) and to obſerve, that it was ſo differing from the other ores of that country, that I did but diffidently gueſs, that it was a mixture, that nature had made of lead and antimony, till particular trials had juſtified my ſuſpicions.

BUT this is not all I had to ſay about the ſulphurs of foſſils: for though I know, that chymiſts pretend to teach us ways of extracting the true ſulphurs of minerals and metals; and experience aſſures me, that a real combuſtible ſulphur may be in a pretty quantity obtained from antimony; yet there are two ſcruples, that ſuffer me not fully to acquieſce in what they teach. The firſt is this, that chymiſts oftentimes deceive others and themſelves too, by miſtaking thoſe things for the true ſulphurs of minerals and metals, that really are not ſo: of which I ſhall give a plain inſtance in the preparation, that many Spagyriſts deliver of the ſulphur of antimony.

FOR when they have boiled that mineral in a ſtrong lixivium of pot-aſhes, they ſuppoſe, that, as by the ſame operation, common ſulphur is diſſolved, ſo the menſtruum ſeeks out, and takes up, only the ſulphureous parts of the antimony: and as common ſulphur is precipitated out of the lixivium, wherein it is diſſolved, by ſprinkling on it vinegar, or ſome other acid; ſo they preſume, that what is ſtruck down the ſame way from the ſolution of antimony, made in the ſame menſtruum, muſt be the true ſulphur of that mineral; in which they are confirmed by the colour: and yet in reality, not only the ſulphur (ſuppoſing that there is one) but the other parts of the antimony will be diſſolved by a ſtrong lixivium, and the yellow powder, that is precipitated, is but a kind of crocus, which will ſometimes after a while (at leaſt in part) ſubſide of itſelf, without the help of an acid. Nor does it convince me, that ſuch a body obtained from a mineral, or metal, is its true ſulphur, that it may be made to burn; for unleſs the colour and ſmell of the flame concur, I ſhall be prone to ſuſpect, that the inflammability may be apt to riſe, partly from the great comminution made of the prepared body, and partly from the additament employed in preparing it. For theſe two, and perhaps even one of them, may contribute ſo much to the inflammable diſpoſition of a body, that little, or no true ſulphur, will be neceſſary to make it burn. Of this I elſewhere give an inſtance in plates of copper; from which an equal weight of ſublimate has been diſtilled: for the remaining maſs will melt and burn at the flame of a candle, almoſt as readily as ſealing-wax. And of theſe inſtances I mention more in another paper, where I endeavour to ſhew, that combuſtible and inflammable bodies may be made up of parts or ingredients, that ſingly had not ſuch qualities. And yet the contrary of this is ſuppoſed in the chymical argument, that infers from theſe qualities the preſence of ſulphur in all thoſe mineral preparations, wherein they are found. Yet by this diſcourſe I would not be thought to derogate from the medical virtues, or other utilities of ſuch ſuppoſed ſulphurs. For they may be very uſeful concretes, though they be not true principles; the finer parts of the mineral being in ſome of theſe preparations extracted,

Of the Producibleneſs of Inflammability.

and further divided, and perhaps very luckily aſſociated with the finer parts of the body, employed to act on them. By which means there may emerge new concretes of great virtue and uſe. And therefore I intend not to derogate from thoſe metalline ſulphurs, which ſome few maſters of chymical arcana reſerve with great care among the chiefeſt they are proud of. And that you may the better examine theſe fine crocuſes, as I am apt to think moſt of them, and try both what they are, and what they do; if I can light on the proceſs (for I dare not truſt my memory) I will at the end of this Appendix impart to you a way of preparing ſome of thoſe, that are made of metals, thoſe being accounted the moſt difficult as well as noble. And thus much I now remember of the trials I made according to this way; that I employed not any acid menſtruum, or liquor made of any particular ſalt, but a neutral or compounded ſalt; which, whilſt it was in actual fuſion, would diſſolve or corrode the very thinly laminated metal.

I do not look upon theſe ſubſtances as the true ſulphurs of the metals, that afford them, but rather (as I lately intimated) ſuſpect them to be a ſort of fine crocuſes, and perhaps magiſteries; which by reaſon of the ſubtilty, and ſometimes fixtneſs of their parts, may prove uſeful to conſiderable purpoſes both in alchymy and phyſic.

But there is another ſort of bodies obtained from ſome minerals, and perhaps ſome metals too, that has a greater reſemblance to mineral ſulphur, than the newly mentioned crocuſes have.

To this purpoſe I remember, that by putting aqua fortis, in a certain proportion, upon crude antimony, and, after it was almoſt totally diſtilled off, increaſing the fire till a dry ſubſtance began to ſublime, we had in the upper part of the retort a yellow and brittle ſubſtance, which being carefully ſeparated from the dark-coloured antimonial powder, that was alſo elevated by the force of the fire, appeared not only by its own colour, but that of its flame, and ſome other ſigns, to be much of the nature of common ſulphur. Nor is this the only way, whereby we have obtained ſuch a ſubſtance from crude antimony, from which (if I much miſ-remember not) I have had a yellow and combuſtible ſulphur, even without the help of a menſtruum.

Paracelsus pretends to have a way of drawing ſulphurs from all metals; of which proceſs becauſe I have found little or no notice taken by chymiſts, I ſhall, for the oddneſs of it, and the reputation of the author (whom I look upon as a man of great experience in metalline affairs) ſubjoin it, as I find it among ſome of his looſe papers or fragments.

Sulphur Metallorum Theophraſti.

Sulphur *metallorum eſt oleitas ex ipſis extracta, prædita virtutibus pro hominis ſalute. Sulphur aliud ex metallis, antequam ignem ſunt paſſa, elicitur, ut ex marchaſitis aureis, aut argenteis, &c. ſecundum nobilitatem mineræ, etiam nobile & præſtans:* ‘ & paucis ‘ interjectis (extractio ſulphuris ex mineris metallicis.)’ *Etiam fieri poteſt per lixivium acre & depuratiſſimum : ſed* (vel potius illa) *alia ſulphura pro intrinſeco corporis uſu minus ſunt commoda propter alkali cinerum, ex quo clavellatum conficimus erodens, & propter calcem, ex quibus talia fiunt lixivia. Sulphur ſic extractum poteſt ablui aqua dulci, & præcipitari. Digeſtio duplum requirit temporis. Debet & rectificari lixivium per ipſius ſublimationem ab omni reſidentia terreſtri, ne cum ipſo incorporentur talia ſulphura, & fiant corroſiva, ad perniciem ægrotorum : quod ne fiat dicta, debet fieri ſeparatio. (Tantum de crudis.)*

SED *jam fuſis & depuratis elicias ipſorum ſulphur : certa nobilior meliorque via non dabitur, quàm per aquam ſalis ſeu oleum ipſius præparatum, eo modo, quem in alchymia luculentur deſcripſi. Talis quippe aqua fundaliter & radicitus extrahit omnibus corporibus metallicis liquorem ipſorum naturalem, ſeu ſulphur & crocum præſtantiſſimum, tam pro operibus medicis, quàm pro chymicis. Reſolvit & frangit unumquodlibet metallum, ex natura ipſius metallica deducens in aliam pro varia intentione & induſtria laborantis.*

THUS far *Paracelſus*'s proceſs; but as I know not whether it be true, becauſe I am not able to reduce it to practice; ſo, becauſe I do not clearly underſtand his meaning, and what is the true nature of the inſtruments he would have us employ, I will not take upon me to determine, whether or no the ſulphurs, he teaches us to be obtainable by this method, be genuine ones, and fit to decide the queſtion we are now conſidering.

BUT whatever become of this obſcure Paracelſian proceſs, what I was ſaying about a ſort of bodies leſs remote than the formerly deſcribed crocuſes from the true ſulphurs of metals (if they have any ſuch) may well ſubſiſt. For I remember we have ſometimes (though the experiment did not always ſucceed) by cementing very thin plates of a certain metal with burnt allum, and afterwards dexterouſly elevating the more diſpoſed parts with ſal armoniac, obtained a ſublimate, from whence we ſeparated, by ablution with fair water, that diſſolved the ſalt, a ſubſtance, which, by its inflammability, appeared a kind of metalline ſulphur.

AND this may ſuffice touching the firſt ſcruple I thought fit to propoſe, concerning the factitious ſulphurs of metals and minerals. To proceed therefore now to my ſecond ſcruple, it may, I think, be ſuſpected, that even this ſort of bodies, which I have mentioned to have been drawn from a metal and from antimony, may not be the effects of a bare ſeparation of pre-exiſtent ſulphur, from the other ingredients of the bodies, that yielded them, but new concretes produced by the operation of the fire on thoſe bodies, and by the combination of ſome of their parts with thoſe of the additament, employed to obtain the ſulphurs. For, as far as I have yet ſeen, either ſalt-petre, crude or diſtilled, or menſtruums made of it, or of other ſalts, or elſe oleaginous liquors, are wont to be made uſe of on theſe occaſions. And it is very poſſible, that ſome of the more diſpoſed parts of theſe additaments may aſſociate themſelves with thoſe of the mineral or metal to be wrought upon; and ſo, from this combination of the ingredients, there may reſult a body of a new texture, which texture may diſpoſe it to be combuſtible, or inflammable, whether the ingredients in their ſeparate condition were ſo or not. As I remember I have elſewhere ſhown, that though aqua fortis be not inflammable, nor a piece of crude copper inflammable or combuſtible in a common moderate fire, yet the metal being diſſolved in aqua fortis, and the ſuperfluous moiſture warily exhaled, there will remain a fuſible concrete, wherein the copper being much comminuted, and its ſmall parts fitly aſſociated with the ſaline ones of the menſtruum, compoſe a kind of vitriol, that being held to the flame of a candle, or even of a piece of paper, will readily burn away in a flame finely coloured; and which may, if one pleaſe, be kept in a flame diſtinct from the other.

To conclude what I have to ſay about my ſecond ſcruple; it ſeems not improbable, that if any of the metals be, by a fit menſtruum, or ſome other congruous additaments, reduced to parts minute enough, and that theſe parts be fitly aſſociated with ſome of thoſe of the menſtruum; the metal may thereby be brought to burn or flame, as I have ſucceſsfully tried, by a way elſewhere declared, upon gold itſelf, whoſe ſulphur the chymiſts would have us look upon, as (what ſeems not very agreeable to the nature of ſulphur) incombuſtible; ſo that, for aught yet appears, it is allowable to ſuſpect, that the ſulphur obtained from this or that metal is not ſo much an elementary or hypoſtatical principle barely extracted, as it is a magiſtery, or ſome other new compound, made by the combination of the metalline particles with all, or ſome of the body, that works on them. But if a chymiſt will have metalline preparations of this kind to be ſulphurs; I may be allowed to make them ſerve for inſtances of the producibleneſs of ſulphurs.

Yet theſe doubts concerning the ſulphurs of metals I propoſe but as ſuſpicions, to draw on further and more accurate trials; by which perchance they may be happily removed. And ſpeaking of the ſulphureous principle of mixt bodies, in that general notion of it, wherein the chymiſts often uſe it, and indeed muſt employ it; we may be thought to have ſaid enough to our preſent purpoſe, though we had left mineral ſulphurs untouched; ſince we have ſhewn, that inflammable parts of mixt bodies may be produced, and therefore cannot be ſafely affirmed to have all been pre-exiſtent in them.

OF THE

PRODUCTION OF MERCURY.

PART IV.

THE complaint I have divers times had occaſion to make, of the darkneſs and ambiguity, that chymiſts have allowed themſelves, if not affected, in treating of their three principles, is applicable to nothing more juſtly, than what they have written about that, which they call mercury. For when they would ſeem to tell us, what they mean by that principle, they are wont to do it in terms ſo looſe and ſo ambiguous, that the repreſentations they make of it, are more like to panegyrics, and ſometimes to riddles, than to clear definitions, or ſo much as good deſcriptions. Since then they have given us no ſettled notion of what they call mercury, but have left us to gueſs what they mean by it; I hope a miſtake about it (if I ſhould run into any) would appear pardonable. That which is agreed on by the moſt of chymiſts, when they ſpeak ſomewhat intelligibly of the principle they call mercury, is, that it is a crude ſubſtance, and that it is a volatile liquor; which, by being ſo, may be diſtinguiſhed from the ſaline principles, eſpecially from the alkalizate or fixed ſalt; as it may alſo be from the oil or ſulphur, by its not being inflammable. But theſe marks will not diſcriminate it from phlegm, which is alſo a fugitive and uninflammable liquor; and therefore to make difference, they muſt add ſome other quality, ſuch as ſapor (which yet agrees not to quickſilver itſelf) that is wanting to phlegm. So that, according to this doctrine, the nature of a chymical mercury or ſpirit will conſiſt in its being a liquor volatile, not inflammable like oil or ſulphur, nor yet inſipid like phlegm. How odd a principle this muſt be, that compriſes ſuch differing bodies, as are acid ſpirits, thoſe of nitre and vitriol; urinous, as thoſe of blood, hartſhorn, *&c.* and anonymous ones, as thoſe of guaiacum, honey, raiſins, *&c.* forced from their acidity; and the running mercuries of minerals and metals, as cinnabar, antimony, and lead; under one principle, which, to deſerve that name, ought to have all the portions of matter belonging to it homogeneous; I may ſafely leave any conſidering naturaliſt to judge. And therefore inſtead of taking further notice of this, it may ſuffice for my preſent purpoſe to mind you, that as for the mercuries or uninflammable ſpirits of vegetables and animals, I have endeavoured to ſhow their production, where I diſcourſe of that of ſpirits and volatile ſalts. And therefore I need but ſay ſomething of the production of mercury more properly ſo called, that is, running mercury: about which perhaps it will not be leſs acceptable to you, and I am ſure it will be leſs troubleſome to me, if I leave you to gather my opinion out of three papers, that were written for differing virtuoſi, at ſeveral times, and on diſtinct occaſions; upon which account, beſides thoſe particulars that relate to our preſent argument, you will perchance find ſome things, that you have not elſewhere met with.

Whether Mercury may be obtained from metals and minerals, or (to ſpeak chymically) An dentur mercurii corporum?

THAT there may be extracted or obtained from metals and minerals a fluid ſubſtance, in the form of running mercury, is the common opinion of chymiſts; in whoſe bodies we may meet with many proceſſes, to make theſe mercuries: which becauſe they are ſaid to be afforded by mineral, and eſpecially metalline bodies, theſe writers affect to call (how aptly I now examine not) *mercurios corporum*.

BUT notwithſtanding all this, divers of the more learned of the Spagyriſts themſelves have looked upon the pretenſion of other chymiſts to the art of making theſe mercuries as but a chymical brag; and ſome judicious modern writers, applauded therein by moſt of the mechanical philoſophers, have proceeded ſo far, as to explode all theſe mercuries of bodies as mere *non-entia chymica*; nay, ſome of them have not ſcrupled to cenſure all thoſe, who pretend to have ſeen or made any of them, as credulous or impoſtors.

IN the management of this controverſy, I confeſs I am not ſatisfied with either of the contending parties; and therefore I ſhall not refuſe to comply with your curioſity to receive in a few lines my thoughts, whether there are or may be any ſuch mercuries, as are diſputed of; yet I deſire leave to premiſe ſuch a ſtate of the controverſy, as I think will avoid ſome verbal janglings, and at leaſt acquaint you clearly with the ſenſe, wherein I deſire to have my opinion underſtood.

WAVING then, in the preſent inquiry, the queſtion, that may occur, 'whether 'or no the mercury ſaid to be obtained from metals and minerals are primitive in'gredients, or hypoſtatical principles only extracted or ſeparated from the bodies, 'that afforded them?' I ſhall propoſe the queſtion in theſe terms: Whether or no 'from a metal or mineral body there may, without the addition of any body, that 'we may be ſure has any common quickſilver in it, be obtained, by the help of art, 'a ſubſtance reſembling common quickſilver, by being ponderous, fluid when actually cold, amalgamable with gold and ſome other metals, and indiſpoſed to wet or ſtick 'to one's hand, or to bodies not of a metalline nature.'

To give you now my preſent thoughts about this queſtion, I ſhall offer them to your conſideration, in the following propoſitions.

Prop. I. *There are divers proceſſes of making the mercuries of bodies, that are ſo darkly delivered, that the generality of chymiſts cannot ſufficiently underſtand them, to be able to try them.* For ſome of theſe proceſſes are ſet down in terms of art, which, for their great darkneſs or ambiguity, are not to be underſtood but by the authors themſelves, or thoſe, who are verſed in the more myſterious parts of hermetic philoſophy. And others there are of theſe proceſſes, that require ſome menſtruum, ſalts, or other inſtruments, that it is not in the power of ordinary chymiſts to procure. Inſtances of this kind may be frequently enough met with by thoſe, that have the curioſity to peruſe heedfully the writings of thoſe, that paſs for the adept philoſophers. And for a ſpecimen of ſuch proceſſes, I am content to annex to the cloſe of this paper the way delivered by *Lullius* of making mercury of ſilver, *Helmont*'s way of preparing mercury of lead, and *Paracelſus*'s way of extracting the mercuries of all metals.

Prop. II. *There are divers proceſſes to make mercuries of bodies, that are either falſe, or accompanied with circumſtances, that make them unfit to be truſted.* For there are of theſe proceſſes, that having been curiouſly tried by thoſe, that had a great deſire to find them true, have not been found to ſucceed at all in practice. Hence we have ſo many complaints of chymiſts, that have loſt their labour in endeavouring to make, according to *Beguinus*'s directions in his *Tyrocinium Chymicum*, the mercury of ſilver, though I do not take that to be one of the difficulteſt to be prepared; and he, that converſes much among thoſe, that have made attempts to make the mercuries of other bodies, as gold, antimony, *&c.* according to the vulgar proceſſes extant in chymical books, will (if I miſtake not) find by their confeſſions, how little the events of their endeavours anſwer their labours and expectations. Nor do all the manuſcript proceſſes, that are communicated to private friends, as great arcana, much excel thoſe I have been ſpeaking of; as ſeveral of my acquaintance have complained to me, that they have found to their coſt. And here not to mention my own experience (which by the help of good principles made me timely deſiſt from unlikely attempts) amongſt many chymiſts I have known, I remember not to have found above three or four credible perſons, that would affirm to me, that they made, or ſaw made, the mercury of any metal or mineral (except of native cinnabar, which is the natural ore of quickſilver) in a conſtant way, by any proceſs he had found in printed books: ſo that ſo many of theſe proceſſes having been upon trial found falſe, wary men may be excuſed, if they do not think fit to believe other proceſſes of mercurification; which, though not yet tried, ſeemed not more probable, than thoſe, that have been already found ſo unſucceſsful, that not only many learned modern naturaliſts, but *Angelus Sala*, and divers others, eminent chymiſts themſelves, have publiſhed to the world, that theſe mercuries are to be found no where, but in the bragging chymiſts books and promiſes; and ſome have, as has been already intimated, gone ſo far as to brand all thoſe for cheats, that pretend they can make ſuch mercuries, and thoſe for credulous, that believe they can be made. But what I think of this ſevere opinion, I ſhall quickly have occaſion to declare.

Prop. III. THERE *are ſome proceſſes, wherein it is thought, that the mercury of a metal or mineral is obtained; when indeed the obtained ſubſtance is miſnamed, or the true mercury, that is ſaid to be extracted, was put in, though in a diſguiſed form, by the operator.*

I WILL not here give inſtances of the ſubtle cheats, that may be put upon the ignorant and unwary, and ſometimes too upon the ſkilful, if they be not alſo cautious;

but ſhall content myſelf to illuſtrate the propoſition by a few known, and therefore innocent, inſtances. And firſt there are ſome, who finding themſelves unable to make the true mercuries of metals or minerals, make bold to aſcribe the name of mercuries to productions, whoſe qualities are very remote from thoſe, that are agreed to be eſſential to quickſilver. Thus *Glauberus* ſpeaks much of his mercury of Luna, which yet is far from being running mercury, or having the ponderoſity and other properties of true quickſilver. So *Angelus Sala* himſelf, in his anatomy of antimony, would have us to look upon the reguline parts of that mineral, as its mercury: becauſe he takes it for granted, it muſt contain mercury, and is pleaſed to fancy no other can be obtained from it. But the difference of the reguline part of antimony, and running mercury in point of conſiſtence, gravity, and other qualities, will, I preſume, indiſpoſe men to confound them. And therefore, I will proceed to confirm the ſecond part of our propoſition, by ſhewing, that the mercury obtained by ſome proceſſes, that may ſucceed, made part of the additament employed by the artiſt, in the operations, and ſo was not properly extracted from the metal, but only recovered from the body, compounded of the metal and the additament. Of this, I remember, I have elſewhere given an eaſy inſtance, in a deluding experiment, that I long ſince ſhewed ſome virtuoſi, in whoſe preſence having mingled the filings of copper with a certain ſalt, and put them in a conveniently ſhaped veſſel of glaſs, I warily held it over a competent fire of well kindled charcoal, till the ſalt was thoroughly melted, and in part ſublimed: by which operation the copper ſeemed to be quite changed, eſpecially in colour, and was really become inflammable: and there remained in the lower part of the glaſs a pretty deal of running mercury, ſo that they would have gone away perſuaded, that they did ſee me make the mercury of Venus, if I had not been careful to undeceive them; which I did by telling them, that this quickſilver was only the common mercury, that lay diſguiſed in the compounded ſublimate I had employed, together with the copper, which ſet the mercury at liberty from the corroſive ſalts it lay concealed in before, by preſenting them a metal more diſpoſed to be wrought on by them, than quickſilver is.

Prop. IV. IT *is poſſible to obtain, at leaſt from ſome metals and minerals, true running mercury, that cannot be juſtly thought to come merely from the additament.* This propoſition a chymiſt might more compendiouſly expreſs by turning it into this ſhort aſſertion, *Dantur mercurii corporum*; but I thought the words I have employed would expreſs my ſenſe more warily and clearly; and yet, *ex abundanti*, I ſhall add this further explication, that though the propoſition ſpeaks affirmatively but of ſome metals and minerals; yet it does not deny, either that more minerals, or that all metals, may afford true running mercury: by which I underſtand (according to what I formerly noted) a mineral body fluid, opacous, exceeding ponderous, amalgamable with gold, and not apt to wet or ſtick to one's fingers, or any other bodies, beſides ſome metalline and mineral ones.

THAT ſuch a mercury may be obtained without the help of additaments, whereof quickſilver is an ingredient, I have been perſuaded to believe by the following obſervations.

I REMEMBER, that many years ago, having had an occaſion to diſtil copper with certain ſaline ſubſtances, I was not a little ſurpriſed to find in the veſſels (that had been luted together) ſome globules of running mercury, which I could not reaſonably ſuſpect to come from the additament, which was not ſublimate, nor any thing I could judge to contain quickſilver. And though the indiſpoſition I had to admit the mercuries of bodies, that ſo many learned men looked upon as non-entities, made me ſomewhat diffident of the genuineneſs of the mercury I had obtained (whereof I had not quantity enough to make juſt trials) yet afterwards, when I found that accidents of the like nature had happened to ſeveral of my friends, I began to think, that what I had kept only for a few days as a queſtionable rarity, might really have been venereal mercury.

A LABORIOUS chymiſt of my acquaintance coming to viſit me once, when I was not well, was very earneſt with me to communicate to him the way of making the mercury of antimony and of Saturn; and when I told him, that I had no ſuch proceſſes of my own, and that I was far from believing thoſe, that I had met with in printed books, to be true ones, he would not acquieſce in this anſwer; but declaring, that he reſolved to make attempts to gain ſuch mercuries, and had rather do it by methods of my propoſing, than of his own deviſing, he preſſed me ſo much to let him know, which way I would go to work, in caſe I had the ſame deſign, that he then had, that to be rid of his importunity, I told him what on a ſudden came into my thoughts. And as ſometimes the mind being put to ſuch plunges, happens upon a lucky hit, and ſuch as much premeditation would not have led it to; ſo it happened at that time to me: for when I, becauſe of my diſtemper, had forgot this affair, the chymiſt, who was a plain honeſt man, came to me with great joy to give me thanks for the inſtructions I had given him, bringing along with him ſome mercury of antimony, and a little mercury of lead, that he had already made by the help of thoſe inſtructions; by purſuing which, he expected to obtain much more mercury from the minerals when they ſhould be longer digeſted with the concourſe of the air, in thoſe ſalts, that I had adviſed him to grind with them. This pleaſing ſucceſs of directions, which I had as to divers particulars forgotten, made me deſire them of the chymiſt; who, beginning to be proud of his attainment, when he perceived I remembered not ſo much as he thought I did, ungratefully delayed to bring me the account he promiſed me at firſt, till the plague reaching to the place, where he lived, and diſpatching him, deprived me of the hopes of ſatisfying my curioſity.

Two gentlemen of my acquaintance, but unacquainted with each other, working almoſt at the ſame time upon ſilver, did each of them, to his wonder, find parts of his ſilver turned into running mercury; with which odd accident each of them came to acquaint me, bringing along with them a little portion of the unexpected mercury. One of theſe portions a ſervant of mine loſt by miſtake, before I could try any thing with it; the other I found, by a trial unknown to the maker of it, not to be common, but metalline mercury; of which the chymiſt complained to me, that he had ſometimes had conſiderable quantities to his great loſs, becauſe much of the ſilver he employed in an operation, that he expected would prove lucriferous, being turned into quickſilver, had ſwallowed up all his gain; and this was that, which invited him to apply himſelf to me, hoping to be able to prevent or remedy this inconvenience by my advice; which I willingly gave him, but, becauſe of his departure out of *England*, could not know with what ſucceſs.

A FELLOW-TRAVELLER of mine, having occaſion to employ a ſaline body about lead, after he had finiſhed his operation, left the lead and ſalt together for ſome months, in a veſſel, which he laid by in a garret, where the air had acceſs to it: afterwards wanting ſuch a veſſel, and not being able to ſupply himſelf readily in the country (in which his experiment was made) he remembered this long neglected veſſel, and coming to ſee, whether it was fit for his turn, he found to his wonder, that though he had employed no mercurial body to work upon the lead, yet part of it was already turned into quickſilver, ſeparable by ſtraining, and more ſeemed in a near diſpoſition to admit the like change: whereupon he brought me, as a rarity, a part of the metal, and a little of the mercury, which I found by experience on gold to be of a nobler kind than common mercury. And I the leſs wondered at this mercurification, becauſe examining the gentleman that chanced to make it, I found the main thing he had employed in the operation was common or ſea-ſalt.

AN expert metalliſt of my acquaintance, being deſirous to try, what gold and ſilver he could get out of a fine Engliſh marchaſite I had preſented him at his deſire, he examined it according to his method, without any mercurial preparation, and found to his ſurpriſe, that it yielded him, beſides other things, ſome running mercury, which he brought and gave me, becauſe it was afforded by the marchaſite I had preſented him.

THE mercury of antimony more than one of my friends have made, by unſuſpected additaments, ſuch as ſalts, that have nothing to do with ſublimate, or other compoſitions, whereof common mercury is an ingredient. One of theſe antimonial mercuries looked ſo oddly, that, though it were made by diſtillation, I had that curioſity to try, whether it would not operate on gold, in a peculiar manner; and having accordingly put a little fine calx of that metal (as about half a drachm or a drachm) into the palm of my hand, I added to it an equal or double weight of the above mentioned mercury; they immediately incorporated with a very manifeſt heat. And this was the quick way I uſed to examine other mercuries of bodies; for though this alone be not a certain ſign of a mercury's being of that ſort, becauſe I can obtain a mercury ſo qualified by another way than any I have hitherto mentioned; yet as their aſſertions and relations gave me ſufficient ground to conclude, that they had obtained thoſe mercuries from the bodies, that they affirm to have yielded them; ſo the readineſs of theſe mercuries to mix with gold, without the help of fire, and even to grow hot with it, which vulgar mercury will not do, confirmed, that they were metalline mercuries, rather than of the ſame kind with common quickſilver. And my way of obtaining incaleſcent mercury is ſo quite differing from any of thoſe, that there was not the leaſt cauſe to ſuſpect, that the mercuries of bodies we have been mentioning were ſo obtained, eſpecially ſince I knew, that my way was unknown to moſt of the perſons I have mentioned, and was practiſed by none of them.

As for the mercury of gold, though I think I have brought a great many parts of crude gold to aſſume a mercurial form, and to come over in that form by diſtillation (whatever divers learned men think of the inſuperable fixity of gold;) yet I confeſs I have not ſeen any mercury, that I was ſatisfied did deſerve the name of the mercury of that metal. But happening to be once in a place, where a foreigner, that was a ſtranger to me, was ſhowing a friend of his, with whom I had ſome little acquaintance, a metalline experiment, that I confeſs, I could not but admire; (for this foreigner was ſo civil, becauſe I came ſo lucky in, as to let me be preſent at the experiment, though not to diſcover any thing of the drug he employed about it;) I made bold to aſk this civil traveller, who ſeemed a candid man, and I perceived had ſeen uncommon things, whether he had met with any way of making mercury of gold? To which he anſwered, that he knew no ſuch way himſelf, but that he met (ſomewhile before) with a very learned man, in compariſon of whom he confeſſed himſelf but a novice, that put ſome gold into a little phial, full of a certain menſtruum, which my relator owned he knew not how to prepare, and intimated to him, that this menſtruum would have a peculiar operation as well upon gold as ſilver.

Afterward this relator having put the phial well ſtopt into his pocket, and carried it about with him, was, when he came home, and took it out to ſet it aſide, much ſurpriſed to find, inſtead of the gold he had ſeen put in, a pretty quantity of running mercury. Which the artiſt, who only lent him the menſtruum, did not ſeem to think ſtrange, when he was made acquainted with it.

IF I would relate what I have heard from men, that I judge to be either eaſily deceivable themſelves, or concerned in point of intereſt to deceive others, or at leaſt of a vain-glorious bragging humour, I might eaſily ſwell this diſcourſe to a greater bulk; but I have been careful to mention only thoſe relations, to which myſelf, in ſpite of my long backwardneſs to believe ſuch things, ſaw cauſe to give aſſent. And if it be objected, theſe inſtances were but caſual experiments, notwithſtanding which there may be no ſettled way for the obtaining the mercuries of bodies; I might anſwer, that ſome paſſages of what has been lately delivered make it probable enough, that even ſettled ways of making the mercuries of bodies, or at leaſt of ſome of them, are not unknown to ſome artiſts; though for certain reaſons, and particularly for the ingratitude of many men, they do not think fit to divulge them. But to anſwer more home to the objection, I ſhall need only to ſay, that though moſt of the above-recited experiments may be ſaid to have been made by chance, in this reſpect, that thoſe, that made them, did not principally deſign the obtaining of metalline or mineral mercuries; yet the effects produced were as naturally and neceſſarily conſequent to operations ſo managed as they were, as if the artiſt had directly deſigned them, as in ſome of the above mentioned relations they did. And it is not material for us to enquire, whether the quickſilver made by thoſe experiments be to be aſcribed to chance or ſkill; ſince whatever becomes of that queſtion, it is plain, that if metals and minerals could by either way be brought actually to afford running mercury, there needs no more to prove, that ſuch mercuries are really obtainable from them.

Doubts about the pre-exiſtence of running Mercury in metals.

THE propoſed queſtion, Whether or no the mercuries of metals and minerals be principles pre-exiſtent in them, and only extracted from them, may to many ſeem, though it do not to you, a ſuperfluous enquiry; ſince the generality of chymiſts, of differing ages and countries, have reſolutely determined it in the affirmative: which is not at all to be wondered at, ſince according to their hypotheſis of the *tria prima* (or three hypoſtatical principles) whereof they preſume all perfectly mixed bodies to be compoſed, metals, being of this ſort, muſt conſiſt of mercury, as well as of ſalt and ſulphur; and conſequently muſt afford it upon the analyſis of the body into its three primordial ingredients. But notwithſtanding all this, the problem ſeems to me difficult enough to be reſolved, partly becauſe ſuppoſing, that there be true metalline mercuries preparable by chymiſts, they very ſtudiouſly conceal the ways of preparation; and partly, becauſe as it is very difficult to obtain any of the factitious mercuries, wherewith to make ſuch lucriferous trials, as a naturaliſt would deſign; ſo thoſe few authors, that affirm themſelves to have poſſeſſed ſuch mercuries, have given us but an exceeding lame and defective account of them, not mentioning thoſe particulars, which are moſt proper and deſirable, in order to the paſſing a right judgment about them. I pretend not therefore to anſwer your queſtion otherwiſe than conjecturally, till I ſhall be better furniſhed with matters of fact. But in the mean while, that I may comply with your curioſity, as much as I ſafely can, I ſhall confeſs to you, that for the preſent I am, by as much information as yet I have had, inclined to think, that the mercuries obtained from metals do not clearly appear to have been pre-exiſtent in them, and only ſeparated from them by the artiſt, but that I think, that at leaſt ſome of them may be further fluid magiſteries of metals, than their extracted principles. One of the moſt obvious things, that ſuggeſted this ſuſpicion to me, was, that whilſt ſome metals, as tin and lead, are in fuſion, they would, to one, that ſhould not know of their being melted, appear to be many parcels of mercury; ſince like it they are fluid and ponderous, and ſtick not to crucibles, or ſtones, bricks, or almoſt any other bodies, except ſome metalline ones, divers of which they will eaſily pierce into, as quickſilver does into ſilver or gold; ſo that if the fluidity of theſe metals were permanent, they might paſs for mercuries. And if in the moon and ſome of the other ſtars, as there are mountains, ſo there are metalline mines, in caſe the heat of the climate or of the ſoil ſhould keep them conſtantly in ſuch a degree of heat, as we here find ſufficient to melt lead (which we know needs not be very intenſe) theſe metals would there emulate the nature of mercuries, as I have learned from travellers, that in divers parts of the torrid zone, what would here be butter, is fluid as well as unctuous like oil, and is ſold like other liquors, by meaſure, not by weight. And an inquiſitive man, who is a ſcholar as well as a traveller, aſſured me, that whilſt he was in ſome parts of the *Indies*, he furniſhed himſelf with ſome liquid ſubſtances afforded by wounded plants, that as ſoon as he came near *Europe*, and not before, turned into conſiſtent and pulverable bodies; it did not therefore ſeem to me impoſſible, that the piercing ſalts, and other ſubtile bodies, employed by artiſts about the mercurification (as ſome ſtile it) of metals, may either by the agility of their own nature, or by ſo altering the ſhapes, and looſing the wonted coheſion of the metalline corpuſcles, bring them to have ſuch a texture and ſuch pores, as may enable the ethereal ſubſtance, whereto ſo many other bodies owe their fluidity, to agitate them. Theſe cauſes, I ſay, or ſome other, that may be propoſed, may poſſibly keep the prepared metal fluid; as we ſee, that though camphire be a conſiſtent and tough body, yet ſome nitrous ſpirits of aqua fortis will eaſily penetrate it, and may be brought to ſtay ſo long with it, that I have, for curioſity-ſake, kept the camphire ſeveral years without loſs of its fluidity, which I found that this kind of liquor would retain, though for trial-ſake I expoſed it to intenſe degrees of cold, ſuch as would freeze divers other liquors. Nor did it to me ſeem impoſſible, that a ſmall quantity of appropriated additament might ſuffice to put a metal into a ſtate of fluidity; for ſince we ſee, that the vapour of lead can arreſt quickſilver, and make it a conſiſtent body; and ſince *Helmont* aſſures us *, that the liquor alkaheſt being once abſtracted from running mercury, deprives it, and that almoſt irrecoverably, of its fluidity, ſo as to make it pulverable; it appears not, why nature or art may not be able to ſupply ſome corpuſcles, that may expel or diſable thoſe, that keep a metal in the form of a fluid body; and eſpecially ſince, as I have elſewhere ſhewn, the matter of metals themſelves may (at leaſt ſometimes) have been a liquor, or ſome other fluid body.

ANOTHER reaſon, that induced me to ſuſpect, that the mercuries of metals and minerals are not, as it is preſumed, merely extracted principles or ingredients, was, that I have obſerved a greater diſſimilitude between mercuries all of them quick, and furniſhed with all, that is requiſite to make them paſs for true mercuries, than will comport with the ſuppoſition, that they are ſimple and primordial bodies, barely extricated from the others, with which they were at firſt commixed. But this argument being the ſubject of an entire, though ſhort, diſcourſe (of the diſſimilitude of running mercury) I need not inlarge on it in this place.

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* *Eſt ſcilicet (Corallatus Paracelſi) Mercurius, a quo liquor Alkaheſt ſemel diſtillatus eſt, reſidetque in fundo coagulatus & pulverabilis, nequicquam in pondere auctus aut diminutus.* Helmontius in Scripto de Arcanis Paracelſi.

IT did alſo ſtrengthen my ſuſpicion to conſider, that the chymiſts, that talk of the mercuries they have drawn from metals, are not wont to tell us, what other ingredients they obtained by their ſuppoſed analyſis, which left me dubious, whether they obtained any ſalt and ſulphur, or not; and of what nature thoſe ſubſtances were, that they did obtain. For if theſe were not true ſalt and ſulphur, the genuineneſs of the analyſis might be queſtioned; becauſe it may be alleged, that the chymical operation and the additament turning ſome parts of the metal into decompounded bodies, which muſt be acknowledged not to have been (in ſuch) forms preexiſtent in them, may alſo have by change of texture turned ſome other parts of the metal into the form of mercury.

To the foregoing conſiderations, afforded me chiefly by the nature of the thing, I ſhall, for the ſake of thoſe, that are moved by the authority of adept philoſophers, as they call them, add that, which among them ought to paſs for a proof from experience. For *Raymund Lully*, whom I take to be one of the greateſt chymical philoſophers, whoſe writings are come to our hands, though in many of his other books he ſpeaks of mercury in a dark and allegorical ſenſe; yet in that excellent little tract, which he calls his *Clavicula*, he delivers a proceſs (which is not to be wrought with vulgar menſtruums, though they bear the ſame names with thoſe he preſcribes and names) from the cloſe whereof it ſeems manifeſt, that his deſign was not to extract a pre-exiſtent quickſilver out of the metal propoſed, but to turn the metal into quickſilver; ſince he orders and directs us to proſecute the mercurification, till the obtained quickſilver be equal in weight to the ſilver, that was to be tranſmuted. And partly upon this I have ventured to ground the foregoing paradox; that the mercuries of bodies are rather magiſteries than extracts. For in this Lullian proceſs, it appears not, that the mercurial principle was extracted from the ſalt and ſulphur, but rather that the body of the metal (without being analyzed) was turned into mercury. And though magiſtery be a term variouſly enough employed by chymiſts, and particularly uſed by *Paracelſus* to ſignify very different things; yet the beſt notion I know of it, and that, which I find authorized even by *Paracelſus* in ſome paſſages, where he expreſſes himſelf more diſtinctly, is, that it is a preparation, whereby there is not an analyſis made of the body aſſigned, nor an extraction of this or that principle, but the whole, or very near the whole body, by the help of ſome additament greater or leſs, is turned into a body of another kind. As when iron or copper by an acid menſtruum, that corrodes and aſſociates itſelf with it, is turned into vitriol of Mars or of Venus; and quickſilver having a ſufficient quantity of aqua fortis ſtrongly abſtracted from it, is changed into a red precipitate; or by being ſublimed up with common ſulphur, is turned into cinnabar; or, to give yet a more appoſite example, when quickſilver (which is the body we treat of) is by the laſting operation of the fire, without external additaments, at leaſt diſtinct from the igneous particles, turned into a red powder, that chymiſts call *Præcipitate per ſe.*

Vide L[illegible]*lium in Clavicula*, cap. 2.

I HAVE received credible information, and ſome proof too, that there is a place in *Tranſylvania*, where portions of running mercury, which, when they fall out of the earth, and lie a while in the air, do of themſelves coagulate into permanently hard bodies: ſo little a diſtance hath nature herſelf there put between the mercurial fluidity,

dity, and the ſolid conſiſtence of the ſame portion of matter. So that if ſo ſmall a thing (and perhaps unponderable as well as inviſible) as the contact of the air can expel, is able by its preſence to retain a mineral body in the form of a true running mercury, as well as by its receſs to leave a ſolid body; I ſee not why it ſhould be impoſſible for art, to intercloſe ſome very minute and reſtleſs particles, which, by their various and inceſſant motions, may keep a metalline body in the ſtate of fluidity, much after ſome ſuch way, as I lately noted, that the ſpirits of nitre did, for whole years together, keep camphire in the form of a liquid oil.

HAVING now propoſed ſome of the conſiderations, that inclined me to think, that the mercuries obtained from metals and minerals may not have been pre-exiſtent in them; the impartiality, that, I think, becomes a naturaliſt, obliges me to take notice alſo of thoſe things, that occurred to me, in favour of the received opinion of the chymiſts, in behalf of which I objected to myſelf divers ſpecious arguments.

OF theſe, the firſt was the general conſent of chymiſts, who take it for granted, that all metals are compoſed of mercury as a material principle, and commonly more copious, than any other conſtituent part of thoſe bodies; but this being an argument drawn only from authority was of ſmall weight with me, in a controverſy properly determinable by reaſon and experience.

2. A SECOND objection was afforded me, by many proceſſes I had met with in chymiſts books, to extract the mercuries, as well as the ſulphurs and ſalts of metals. But neither did this argument appear to me of any great moment, for moſt of theſe proceſſes I looked upon as fictitious things; which, if the authors of them had taken the pains to try themſelves, they would have found not to ſucceed in practice; and ſcarce any of them was ſo ſkilfully framed, as to ſatisfy a conſidering naturaliſt, in caſe it had ſucceeded, that the obtained mercury was a pure principle only ſeparated or extracted from the other ingredients of the metals, and not a reſult of ſome metalline parts conjoined with ſome parts of the additament; as it ſeemed manifeſt enough to me, that the ſuppoſed ſalts of metals, that are pretended to be made by ſuch preparations, are not the principles of ſuch metals, but new concretions, and indeed not ſimple, but decompounded bodies; as is evident in the ſalt or ſugar of lead made with the ſpirit of vinegar, and in the ſalt of ſteel made with that, or other acids.

3. A THIRD objection, and of greater weight, ſeemed derivable from this conſideration, that quickſilver eaſily amalgams with metals, becauſe of its cognation with the mercurial part theſe bodies abound with.

4. AND this argument appeared capable of being ſtrengthened by a more conſiderable one; which is, that the gravity of the metals is ſuch, as cannot reaſonably be deduced from any other cauſe, than an abundance of the mercurial principle, there being no other bodies (known to us) beſides quickſilver, that are near ſo ponderous as metals.

THESE two objections I thought fit to couch together, to be able, in fewer words, to anſwer them both: I conſidered then, that amalgamation being, in effect, but a kind of diſſolution of metals in a menſtruum or fluid body (for ſuch mercury is, in reference to them) there is no neceſſity, that the ſolvent ſhould find in the metal a copious ingredient juſt of its own nature; for diſſolution depends not ſo much upon the pretended cognation between the ſolvent and the body it is to work on, as upon the congruity, as to ſize and figure, between the pores of the latter and the corpuſcles of the former; as may appear by the ſolution of ivory and hartſhorn (which belong to the animal kingdom) that may be made with aqua fortis; and by that, which I have elſewhere ſhewn may be made of zink, and even of copper, by the ſpirit of vinegar, the urinous ſpirit of ſal armoniac, and ſpirit of vitriol ſeparately employed; though the firſt of them be a menſtruum drawn from a vegetable, the ſecond from an animal, the third from a mineral ſubſtance. And as for amalgamations themſelves, I obſerve, that the facility mercury finds in joining with a metal, does not barely depend upon the plenty of the mercurial ingredient, contained in the metal; at leaſt if the greater ponderoſity, or ſpecific gravity of the metal depend upon the copiouſneſs of the ſame mercurial principle, or ingredient, as the fourth objection ſuppoſeth: for we find by experience, that mercury will far more eaſily amalgam with tin than with copper, which, yet, is much more heavy than it; nay, than with ſilver, which is a good deal heavier, (in ſpecie) than copper; and is by chymiſts preſumed to be much nearer of kin to mercury than is tin. To which I ſhall add, that although Mars be ſpecifically heavier than tin, yet it is far from being more eaſily amalgamable with mercury; that though tin will readily admit this mineral liquor, without the help of heat, there is no way vulgarly known to chymiſts to make an immediate amalgam between mercury and Mars. So that one of the two objections, I lately joined together, muſt be declined: ſince by the trials I have purpoſely made, it appears, that either the diſpoſition of metals to amalgamate with mercury does not barely depend upon the ſuppoſed plenty of mercury contained in the metal; or elſe that the greatneſs of the ſpecific gravity does not depend upon the more plentiful participation of that mercurial ingredient. Although the fourth objection be built upon a ſuppoſition, that the great ponderouſneſs of metals, in compariſon of other bodies, can proceed from no other cauſe than the great quantity of mercury they contain; I conſidered too, that it might be juſtly demanded, whence mercury itſelf, as well as whence metals, derive their great ponderoſity; and I ſee not, why it may not be ſaid, that both the one and the other owe it to the ſolidity, and cloſe order, of the corpuſcles they conſiſt of; to which qualification it is not eſſential, that the portion of matter endued with them be in a ſtate of fluidity, rather than in one of conſiſtence: as on the contrary we ſee, that gold and lead are exceeding ponderous bodies, as well when they are in fuſion, as when they are cold and hard; and ſo is quickſilver, as well in its wonted and liquid form, as when it is coagulated, as chymiſts ſuppoſe, by the vapour of lead.

But this will be ſomewhat further cleared in what I ſhall ſay to the fifth and laſt objection, that my thoughts ſuggeſted to me, and which philoſophical candour forbids me to conceal; though I find it eaſier to be propoſed than anſwered. It may be then alleged in the fifth place; that the mercuries of metals muſt needs be but partial principles of them, ſince quickſilver being confeſſedly heavier than either the ſulphureous or ſaline principle, and being ſpecifically heavier, than almoſt any metal itſelf; the gravity of a metal cannot reaſonably be ſuppoſed to proceed from the whole body of the metal, but only from ſome one ingredient heavier in ſpecie than the reſt, and, than the metal itſelf. And this ingredient or principle can be no other, than the moſt ponderous body, mercury.

This difficulty, I confeſs, does keep me yet in ſome ſuſpence, till I have further opportunity, to make ſuch trials, as I think proper to clear it. Yet in the mean time, I ſhall offer ſome few things, which perhaps may leſſen it, if not quite remove it.

I consider then, that there is no neceſſity to ſuppoſe, that metals, of what denomination ſoever, as tin, iron, ſilver or gold, are bodies perfectly homogeneous, though they ſeem ſuch to our eyes. This ſuppoſition I elſewhere purpoſely diſcourſe of, but in this place I need not ſpend time about it; ſince the chymiſts (who are thoſe I now reaſon with) do not only allow, but teach it, ſince they will have metals as well as other mixt bodies to conſiſt of three hypoſtatical principles, whereof mercury is one, although it muſt be much heavier in ſpecie, than either the ſalt or the ſulphur it is blended with; becauſe it is from the participation of that ingredient, that they derive the great ponderouſneſs, which metals have, in compariſon of other bodies.

AND to this granted ſuppoſition, I ſee not why it ſhould be abſurd, to add this other, that the more ſolid and heavy particles or corpuſcles of metal may lie in it, not in the form of fluid or mercurial, but conſiſtent parts, and that theſe may be more diſpoſed than the reſt, to be brought by chymical additaments and the operation of the fire into the form of a running mercury. Nor ought it to be judged incredible, that the fore-mentioned ſolid portion of the metal ſhould be more ponderous than quickſilver, ſince, as I have often tried, gold, though a conſiſtent body, is far heavier than quickſilver, to the bare participation whereof gold cannot owe its ſpecific gravity.

IF this hypotheſis be admitted, it will be eaſy to give an account, how the mercury of a metal may be heavier in ſpecie (that is, bulk for bulk) than the metal that afforded it; for the difficulty is eaſily reſolved, by ſaying, that the ſolid parts, which by the chymical operation are reduced into the form of quickſilver, were far more ponderous in kind, than the other parts of the metal, which being alſo aſſociated with them, did, by their comparative lightneſs, make the entire metal leſs heavy (if the bulks be equalled) than an aggregate or convention of all the ſolid parts alone would have been. Which may be illuſtrated by what I have heedfully obſerved, of the decrement of ſpecific gravity, ſuſtained by quickſilver, when it is united by ſublimation either with ſulphur into cinnabar, or with ſalts in corroſive ſublimate.

BUT I muſt not diſſemble, that againſt the foregoing diſcourſe there occurred to me a couple of arguments (that I have not met with amongſt chymiſts) whereof the latter is very conſiderable For I foreſaw it might be alleged, firſt, that the mercuries of metals being in a liquid form, could not well be ſuppoſed to be ſo cloſe and ponderous bodies, as our hypotheſis requires: and next, that we ourſelves admit an experiment of *Raymund Lully*, whereby he pretends to reduce the whole body of ſilver into mercury, which is a heavier ſubſtance than ſilver; and in this caſe we cannot have recourſe to this anſwer, that the corpuſcles, which aſſume the form of mercury, were far more ponderous than the others, that concurred with them to compoſe the metal.

THIS twofold objection I do not pretend to anſwer at once, but may perhaps enervate it by degrees.

AND firſt, though it be very poſſible, that a pretty quantity of additament may be employed about the mercurification (to ſpeak in the chymiſts language) of a metal, yet there ſhall really and finally adhere to the metalline parts, but a very ſmall proportion of additament, that will continue with them, and keep them in a mercurial flux. And it may appear the more credible, that a very ſmall quantity of additional matter may have a very great ſtroke in altering the conſiſtence of that, which is obtained from a metal, as its moſt ponderous portion, if you conſider with me, that the bare acceſſion of igneous particles, is able in time, to turn running mercury into that red powder, which chymiſts call *Præcipitate per ſe:* and I have found by trials purpoſely made, and elſewhere related, that this powder without any further additament may be reduced into running mercury. Nor muſt I pretermit, on this occaſion, a notable paſſage I remember to have met with in *Helmont*, who relates, that by the abſtraction of the liquor alkaheſt (which is wont to come all over in diſtillation from common quickſilver) he did quite deprive it of its fluidity, and turned it into a conſiſtent body, and even into a fixed one; whereby you may ſee how little a quantity of matter will ſerve to change the conſiſtence of a body of a mercurial nature.

BESIDES that, a fluid form does not always argue the lightneſs of the body, that it is found in, ſince it may conſiſt of particles ſo ſolid and ſo numerous, that, notwithſtanding their inteſtine motion, the body they compoſe may be very ponderous: as may appear by red-hot iron, melted lead, and, which is an inſtance appoſite to our purpoſe, in common quickſilver, which though fluid is heavier than any known body in the world, gold excepted.

BUT I conſider farther, that though the ſolid portion of a metal retain more of the additaments employed to bring it into the form of mercury, than it can be proved to contain; yet this diſadvantage may be compenſated by the new diſpoſition of parts, that the mercurified portion acquires by the operation, that turns it into a liquor, and may be ſuppoſed to bring the parts to a cloſer, or otherwiſe a more expedient order than they were in before: as ice when thawed takes up leſs room in the form of water, than it did before it became a liquor. I ſee no impoſſibility, that the ſpecific gravity of metalline bodies may be increaſed or diminiſhed by ſuch ſmall proportions of additaments, as do not at all conſiderably add to their abſolute gravity. This the chymiſts ought not to deny, if they conſider what themſelves grant of the efficacy of what they call the philoſophers ſtone, whereof they tell us, that one grain, if it be of a nobler order or degree, may tranſmute a whole pound of quickſilver into perfect gold and conſequently the ſpecific gravity of a metal is notably changed by an additament; which (according to the differing pounds uſed in ſeveral countries) amounts not, perhaps to the ſixth or ſeventh thouſandth part of its weight. Beſides, the tranſmuting powder being a compounded body, whereof but part is gold, may probably be ſuppoſed to be more light in ſpecie than the metal, that by addition of it is produced; which being pure gold, is the ponderouſeſt body yet known to us. And to confirm the argument, I ſhall add, that there is a way, though I pretend not to know it, of making a metal far lighter in ſpecie, than it naturally is, by the addition of leſs than a hundredth part of its weight, as experience has convinced me.

WHEREFORE to come now to the grand objection furniſhed by *Lully*'s lately-mentioned experiment, it will not preſently follow, that if the whole body of a metal be brought into a mercurial form, this mercury will ſwallow up and deſtroy our hypotheſis: for though I grant, that, in this caſe, it cannot be ſaid, as in the former caſes (wherein a part only of the metal is mercurified) it may be, that the obtained quickſilver conſiſts of the more ſolid and ponderous parts of the metal; yet it may be ſtill ſaid, that, for aught we know, the mercury, produced by the reduction of the whole metal into a fluid form, may be ſpecifically lighter than common mercury, and ſo cannot be neceſſarily concluded to be ſpecifically heavier than the metal, that afforded it. I lately employed the words, *for aught we know*, becauſe we are now upon the caſe, wherein philoſophical candour invited me to acknowledge, that I wanted further trials to give myſelf full ſatisfaction: for although I have had portions of the mercuries of more than one or two metals, yet it was but in ſmall quantities; ſo that the other trials, I had the curioſity to make with them, kept me from examining their ſpecific gravity, and from finding by an hydroſtatical way, that I have elſewhere declared, whether they were not lighter in ſpecie than inferior metals, and conſequently than common mercury. For that quickſilver may be ſpecifically lighter than the metal that affords it, I think the chymiſts cannot reaſonably refuſe to grant; ſince they allow, that running mercury may be obtained of gold, and tell us great matters of it, becauſe of its proceeding from ſo noble a body. Now if this golden mercury be ſaid, becauſe of the ſuppoſed reſemblance of all mercuries, to be of the ſame ſpecific weight with common quickſilver, then I have a notable inſtance of a mercury, that is conſiderably lighter in ſpecie, than the metal, that afforded it. And therefore, till experience have manifeſted the contrary, it will not be abſurd to preſume, that the mercuries of other metals may likewiſe be lighter in ſpecie, than the reſpective bodies, from which they were obtained: but if it be ſaid, that this golden mercury may perhaps be as ponderous as gold itſelf or even more, then it is plain, that it is poſſible for a metalline body, notwithſtanding its being reduced into the form of a fluid, to be equiponderant to the metal, that afforded it. And that I may not ſeem to argue altogether from the conceſſions the chymiſts ought to make, I will add, by way of confirmation, a couple of things, that perhaps you will think ſomewhat ſtrange. Whereof the former is, that it is poſſible for a metalline body to reſemble another in all the manifeſt qualities, whereby artiſts are wont to examine them, and yet they differ much from it in ſpecific gravity: as I had once opportunity to obſerve in a metal, that was not only white (within and without) like ſilver, and very malleable, but did, when I purpoſely examined it, endure cupellation, and paſſed for, and was reputed by a very eminent artiſt, that ſent it me to examine, to be good ſilver in all proofs; and yet this metal I found by hydroſtatical trials to be much lighter in ſpecie, than common ſilver. And if the famous perſon, that ſent it me, was not miſtaken (for ſo I muſt not think he would knowingly miſinform me) this odd metal may yield me a notable inſtance to my preſent purpoſe, ſince he affirmed this metal to be made without the addition of any metalline body of quickſilver; which, if this be ſo, muſt, by a change of texture, have made a conſiderable loſs of its ſpecific gravity. But to proceed to my ſecond inſtance, which will be yet more oppoſite; I ſhall add, that once I had a mercury, which amongſt other remarkable properties, that belong not to this argument, had a very ſtrange one; namely, that it was conſiderably heavier in ſpecie than common mercury (as I found and ſhewed to a great virtuoſo by hydroſtatical trial) though it was made of a body no heavier than common mercury, and by the help of additaments, which were much lighter than common mercury. And this was ſo far from being a more groſs and ſluggiſh kind of quickſilver than the ordinary, that it looked very fine, and was very agile, and had, before I examined it, been more than once diſtilled. By this it may appear, that from hence, *that a body is in a mercurial form*, we cannot ſafely determine what degree of ſpecific gravity it has. For ſince by this laſt example it appears, that a ſort of quickſilver may be far more ponderous than common quickſilver; it ſeems not unreaſonable, that a ſort of quickſilver may be far lighter than common mercury, and ſo perhaps lighter than the metals, that were reduced into that form: it being far leſs likely, that the former ſhould be produced than the latter, in regard there is but one mineral body in the world, that we know of, at all heavier than common quickſilver; whereas there are many of thoſe, that are capable of being aſſociated with it, that are far lighter than it.

BUT, as I intimated above, I am unwilling to ſpeak ſo poſitively about this matter as I might do, if I had opportunity to make the trials I would with the mercuries of bodies: only thus much I ſhall venture to ſay, that, for aught yet appears, the argument, I have been anſwering all this while, is not cogent, ſince it is built upon a ſuppoſition, that the mercuries afforded by metals and minerals muſt be of the ſame weight with common mercury; which is not only a prooſleſs aſſertion, but repugnant to the experiment lately mentioned of the diſtilled mercury, that was heavier than common mercury, and to the preſumption derived thence, that there may be bodies, in a mercurial form, more light in ſpecie than common mercury. And whatever becomes of the opinion I incline to; the argument I have been examining of the chymiſts, may be invalidated by what I have ſaid, where I took notice of the notable exceſs of ponderoſity, that pure gold has in regard of common quickſilver: for by that inſtance it plainly appears, that it is not to the participation of common mercury, that metals muſt neceſſarily owe their great ponderoſity, but that nature (and art too) may contrive the parts of a body into ſo cloſe an order, as to make that body (whether ſolid or fluid) more ponderous, bulk for bulk, than common quickſilver itſelf.

HAVING now diſpatched what I intended to ſay in the foregoing diſcourſe, it remains, that I perform the promiſe I made, of adding the ways of mercurification (as chymiſts ſpeak) above referred to, as delivered by *Paracelſus*, *Helmont*, and *Lully*: about which I muſt give you this advertiſement, that beſides the obſcurities, and imperfections, that a moderate degree of attention may enable you to diſcover in theſe proceſſes, underſtood in the literal ſenſe, there are, if I much miſtake not, ſome affected equivocations in terms, that ſeem very plain, and free from ſuſpicion of ambiguity. As for inſtance, though the word *ſal armoniacum* ſeem to be of this ſort, yet amongſt Hermetic philoſophers it often ſignifies, not common ſal armoniac, which is far from being able to perform the effects they aſcribe to theirs, but a very differing and much more noble and operative thing; which, becauſe it may be ſublimed like common ſal armoniac, they are pleaſed to call by that name. And though ſometimes they give it the title of *ſal armoniacum philoſophorum*, yet oftentimes they omit the diſcriminating epithet, eſpecially in philoſophical proceſſes (that is, ſuch as thoſe, wherein they deliver their higher arcana) of which ſort are many of *Paracelſus*'s proceſſes, and more (not to ſay moſt) of *Lully*'s. What is meant by *ſal armoniacum philoſophorum*, I think it needleſs to tell you here (but may perchance do it on another occaſion;) ſince that compoſition requires an ingredient, that neither of us is furniſhed with, and that you cannot procure. There may be other ambiguities in the following proceſſes, that will not be eaſily diſcovered, but by ſuch as are verſed in the myſterious language, which ſome would call canting, of the Hermetic philoſophers. But, I think, I have ſaid enough already to ſhew, that the annexed proceſſes are fit to confirm, what is delivered upon the firſt propoſition of the foregoing diſcourſe; and therefore without offering to explain them, I ſhall ſubjoin them in the proper terms of the reſpective authors.

Ratio extrahendi ex omnibus metallis mercurium Paracelſica.

Hæc extractio (ſcilicet mercurii ex metallis) fit per aquam mercurialem, quæ nec *Johanni de Rupeſciſſa*, nec aliis, quicquid etiam jactitent, cognita fuit. Ideoque diligenter eſt cognoſcenda, & indefeſſo labore tractanda. Hoc ergo pacto paretur dicta aqua mercurialis.

℞. ℔ iii. *Mercurii ſublimati ſepties per vitriolum, ſal nitri, & alumen.*

SALIS armoniaci, ter à ſale ſublimati & clari & albi iſs. *Trita ſimul & alcolizita ſublima in ſublimatorio per arenam, horis* 9. *Ubi refrixerit, ſublimatum cum penna detrahito, & cum reliquo ſublima, ut prius. Hanc operationem quater repete, donec amplius*

non ſublimetur, & in fundo maſſa nigra maneat, inſtar ceræ, fluens. Refrigeratum exime, & tritam rurſus in patina vitrea ſæpius cum ſalis armoniaci aquâ, ſ. autem præparata, imbibe, & ſua ſponte coagulatam rurſus imbibe & ſicca, ad 9. *ſeu* 10. *uſque vices, donec ferè non ulterius coaguletur. Tritum ſubtiliter ſupra marmor in loco humido ſolve in oleum pulchrum, quod rectificabis per diſtillationem in cineribus, ab omnibus fæcibus & reſidentia. Hanc aquam omnium facile principem diligenter aſſervabis, cujus* ℞ *unc.* viii *& impone laminas opt. ſolis ac lunæ, optimè mundatas, pondere unc.* iß. *vitro clauſo repone ad digeſtionem in cineres calidos horis* 8. *Corpus tuum videbis in fundo vaſis tranſmutatum in ſubtilem vaporem ſeu mercurium. Factâ ſolutione totius aquæ mercurialis per alembicum lento igne à prima materia ſublimando ſepara, & in vitreo vaſculo diligenter aſſervato. Habebis hoc pacto veriſſimum mercurium corporis.* Paracelſus in Man. de lapide Philoſophorum.

SENSI (ſays he) *cruditatem Saturni, pinguedine fixorum ſalium ſolubilem, ſolo quandoque igne carptim delebilem, ſicque dividi compoſiti partes, crudumque argentum vivum currere permitti. Sulphur fugitivum ſuperans in Saturno trahere ad volatum, fixum, inſeparabiliter junctum. Quodque expediet imprimis Saturni ſublimatio. Cujus expreſſione nulla eſt elevati ad reſidens coloris aut ſubſtantiæ differentia. Unde etiam caloris, fuſionis, & mollitiei cauſis, poſt calcinationes & redactiones, reſiduis medullitus, ſine igne fuſionem, ſolitamque mollitiem minimè refutat.* Helmontius, in Poteſt. medicaminum. num. 40.

Extractio mercurii à corpore perfecto.

R*EC. unc.* i. *calcis lunæ appellatæ, calcinetur modo quo dicitur in fine noſtri magiſterii operis, quæ quidem calx teratur ſuper porfidum in pulverem ſubtilem, quem pulverem imbiberis bis, ter, quater in die, cum optimo oleo tartari, facto eo modo, quo dicetur in fine noſtri, deſiccando ad ſolem quouſque dicta calx abſorbuerit de dicto oleo* 4 *aut* 5 *partes, plus quàm fuit ipſa calx, terendo ſemper ſuper porfidum, ut dictum eſt, & in fine bene deſiccetur calx, ut benè poſſit in pulverem redigi. Et quando fuerit bene pulverizata, ponatur in metreto cum collo longo. Ponatis de noſtro menſtruali fœtenti, facto de duabus partibus vitrioli rubei, & una ſalis petræ, & prædictum menſtruum prius diſtilletur ſepties, & bene rectificetur, ſeparando fæces terreſtres in tantum, quod prædictum menſtruale ſit totum eſſentiale. Deinde lutetur benè metretum, & ponatur ad ignem cinerum, cum parvo igne carbonum, quouſque videris materiam bullire & diſſolvi. Deinde ſic ſupra cineres diſtilla, donec amiſerit menſtruum, & materia fuerit frigida totaliter: cum frigidum fuerit, vas aperiatur, & materia, quæ frigida eſt, ponatur in alio vaſe bene mundo, cum ſua cappa bene lutata ad furnum ſupra cineres, & luto bene diſiccato, fiat ignis paulatim in principio, quouſque totam recipias aquam ipſius. Poſtea augeatur ignis, quouſque materia bene fuerit deſiccato, & ſpiritus fœtentes ſint ad cappam, & in receptorio jam exaltati. Et dum tale ſignum videbis apparere, dimittatur vas infrigidari, ignem minuendo. Et poſt refrigerationem vaſis, extrahatur materia, & in pulverem ſubtiliſſimum redigatur ſuper porfidum; ita quòd pulvis ſit impalpabilis, qui ponatur in vaſe terreo bene cocto & bene vitreato. Et poſt ponatur ſuper dictum pulverem de aqua communi bulliente, movendo ſemper cum baculo mundo materiam, uſque materia fuerit ſpiſſa ſicut ſinapi. Et move dictam ſalſam cum baculo, quouſque videris apparere grana mercurii è corpore, & quod vobis appareat quantitas magna prædicti mercurii vivi, ſecundum quod poſueris de corpore perfecto, id eſt, de luna, & dum habueris magnam quantitatem, interim projice deſuper aquam bullientem; & tandem movendo, quoad tota materia reſolvatur in materiam ſimilem argento vivo vulgari, tollantur terreſtreitates cum aqua frigida, & deſiccetur per pannum: poſtea tranſeat per corium, & videbis mirabilia.* Lullius in Clavicula, Cap. 2.

Of

Of the diſſimilitude of running mercury.

BEFORE I undertake to give you my opinion about your queſtion, I muſt crave leave to ſtate it ſomewhat more clearly, by propounding it thus: whether all the bodies, that in the ſhops, and among chymiſts, paſs for true running mercuries, are homogeneous; or ſo much of one and the ſame nature, that the ſeveral portions of ſuch mercuries are but numerically different? Now to the queſtion thus ſtated, the fear of ſeeming to maintain a paradox, by diſſenting from the generality of chymiſts, as well as naturaliſts (who are wont to employ indiſcriminately all running mercuries not manifeſtly adulterated) will not keep me from returning a negative anſwer.

AND though it were not over difficult for me to give you the reaſons of my opinion, drawn up into method, and refer ſeveral inſtances I ſhall produce, ſome to the depuration of quickſilver, ſome to the impregnation of it, ſome to the coction, and others to two, or all theſe ways of altering it; yet I ſhall rather preſent you with them, by way of looſe obſervations, becauſe I preſume that freedom will not be unacceptable to you, as it will allow me, to give you ſome few, but uncommon notices and hints about ſuch noble ſubjects as prepared mercuries.

1. IN the firſt place, then, it may be obſerved, that a running mercury may be brought to differ from common quickſilver by depuration: for there are in moſt mercuries either recrementitious particles, or at leaſt ſome looſe adherencies, that are ſeparable from the reſt of the body; and which being ſeparated, the mercury becomes more homogeneous or clean, than it was before this external depuration (for ſo I call it, to diſtinguiſh it from another, that is internal) that is uſually made, by grinding and waſhing mercury very well with ſalt and vinegar (for which purpoſe, I alſo ſometimes uſe ſpirit of wine) which one may not unfrequently ſee ſomewhat fouled, by what it carries off from the mercury, which is alſo ſometimes attempted to be purified by the more laborious way of diſtillation; which, though in ſome caſes inſufficient (as I ſhall ſhew anon) is in ſome others very convenient; whereto ſome artiſts add other probable means, tending to the ſame purpoſe. So that I do not wonder to find, that divers philoſophical Spagyriſts themſelves, before they proceed to more intimate preparations of mercury, order it to be ſeveral times previouſly incorporated and ſublimed with acid ſalts or ſulphurs, and then revived with alkalies. Since without examining their grounds it may be ſaid, according to mechanical principles, that by diligent commixtures the mercury is divided into exceedingly minute, if not inviſible globules, or ſuch like parts; and by this great comminution, it acquires far more of ſurface than it had before, by which means a great multitude of ſeparable parts come to be touched, almoſt of every ſide, by the ſalts or ſulphurs, to which by this means, when the quickſilver is driven from them in the revivification, it is probable, that very many of them ſtick, that were not ſuperficial, when all theſe globules made up but one mercurial maſs. And it is poſſible too, that the alkalies employed to revive the quickſilver, may help to tear off from it ſome of the feculent particles, which the chymiſts would deſire to have it freed from. And here, let me advertiſe you upon the by, that there is no neceſſity to have recourſe to ſalt of tartar or quicklime, or ſuch like alkalies for the reviving of quickſilver: and therefore when I would, with eaſe, obtain a clean and active mercury for ſome purpoſes, I do not employ acid and then alcalizate ſalts, but mix very well common cinnabar finely powdered with a double weight, or at leaſt, an equal weight of filings of iron, or ſteel. For theſe being diſtilled together in a low retort with a ſmart fire, the ſulphur of the cinnabar

will faſten upon the filings, and let the mercury come over fair and vivid, and perhaps ſomewhat impregnated with a martial virtue, upon whoſe ſcore it may be better than if it had been prepared by mere depuration.

2. AND this leads me to the mention of another way of diverſifying mercury, which is by impregnation, either corporeal, or ſpiritual (if for diſtinction ſake I may ſo call them.) But the impregnation being a comprehenſive way, divers particular methods may, after a manner, be referred to it: yet becauſe the true grounds of ſuch references are ſometimes hard enough to be aſſigned, at leaſt in few words, I ſhall allow myſelf, without ſcrupulouſly regarding them, to proceed in my free obſervations.

3. THE next thing then, upon whoſe account a running mercury may come to differ from common quickſilver, is a ſpiritual impregnation. By mercury ſpiritually impregnated I mean that, with which ſome ſubtile parts of another body are ſo intimately aſſociated and united, that not only the additament will paſs with the mercury, when it is ſtrained through leather (though that be the means, by which artiſts uſually ſeparate gold itſelf from the mercury, wherewith it has been amalgamed) but will alſo continue with it after diſtillation, without hindering the mercury from being vivid enough. I know there are many chymiſts, eſpecially among the more cautious, that look upon quickſilver as ſo heteroclite a mineral, that as no body can faſten enough upon it, to alter it intrinſecally, ſo it will not admit any other body to be aſſociated with it any thing intimately, or permanently. And indeed ſince we find, that when gold itſelf, with which of all bodies whatſoever mercury is believed to have the greateſt ſympathy, may yet be ſeparated from it by ſtraining an amalgam of thoſe two metals through leather, which will tranſmit the quickſilver, and retain the gold; and if ſuch an amalgam be diſtilled with a competent fire, the mercury will aſcend, and leave all the gold behind in the retort; ſince mercury I ſay, is ſo ſeparable from gold itſelf, it may ſeem improbable, that it ſhould be more intimately aſſociated with any other bodies. But theſe arguments, though ſpecious, do not, I confeſs, convince me, who muſt not deny, but that the corpuſcles of ſome mineral bodies may be ſo well commixt with quickſilver, as to paſs with it through the pores of leather , and who have found by trial purpoſely made (and elſewhere related) that quickſilver being in a convenient proportion amalgamed with tin, or with lead, and diſtilled with a competent fire, will manifeſtly bring over with it ſome of the aſſociated metal; inſomuch that not only I have found a notable increaſe in the weight of the diſtilled quickſilver, but it would both leave a tail, as they call it, behind it, when it moved upon a ſloping glaſs, and (which was more) when the ſluggiſh mercury had reſted a while, it would appear covered over with a kind of ſcum, made of the emerging corpuſcles of the tin, or lead, either of which, eſpecially the former, is a metal lighter in ſpecie than quickſilver. Hence it appears, that mercury may be ſo ſtrictly united to a not deſpicable proportion of a groſs and ponderous body, and of an ignobler kind, as to carry it along with itſelf in diſtillation; which by this appears not to be near ſo certain a way, as ſome learned chymiſts think it, to try, whether mercury be pure from all adulterating mixtures, and to free it from them, if it had any before. But the chief uſe I will make of this experiment is this, that ſince we ſee, that ſometimes mercury does not refuſe even corporal impregnations (as for diſtinction ſake I call thoſe lately recited) it ought not to appear incredible, that it may in ſome caſes admit ſpiritual impregnations, and ſo intimately aſſociate with itſelf ſome of the finer parts of certain metals and minerals, as not to part with them, though they be diſtilled, and afterwards perhaps ſeveral times waſhed. This brings

into my mind, that I had once a diſtilled mercury made by an impregnation of common mercury, a drop or globule of which, being evaporated from a thin piece of ſilver, not only ſeemed to have ſomewhat penetrated it, but (as I expected) left upon it a rugged ſubſtance apparently lighter than the ſurface of the piece, and of a colour very near that of gold, from whoſe nature perhaps it was not very remote: but that common mercury may indeed be ſpiritually impregnated, I have been perſuaded by divers effects, that I have tried of ſuch impregnations: and I acknowledge to you, that moſt of the uncommon mercuries, that I am now proceeding to tell you of, have been prepared after ſome ſuch manner.

4. ANOTHER thing, wherein a mercury may differ from common quickſilver, is a facility to amalgam with gold: for it is known to gilders, goldſmiths, and others, that are verſed in ſuch experiments, that to make amalgams with gold and mercury, it is uſual enough to take ſix parts of the latter to one of the former, and ſome take eight or more. Nor is ſo great a proportion of mercury wont to keep them from thinking it requiſite to make both it and the gold ſeparably, and conſiderably hot to facilitate their commixture, but I have divers times had ſpiritually impregnated mercuries with but two parts, of which I would preſently make an amalgam with one part of the calx, or leaves of gold, and that without any other external heat, than that of the palm of my hand. Nay, ſometimes, for trial ſake, I have employed but one part of quickſilver to make in the palm of my hand a mixture, wherein the gold was ſo far from appearing, that the colour of the quickſilver was not ſenſibly ſo much as impaired.

5. ANOTHER difference between ſome mercuries and thoſe that are vulgar, is, that theſe being put to calx of gold, though one do at length bring them to mix (for it is not ſo eaſily done, as men are wont to preſume) yet they will not diſcloſe any ſenſible heat, but the mixture, as each of the incorporated ingredients was, will to the touch be cold: but though I know there are many learned chymiſts, that look upon incaleſcent mercuries, that is, ſuch as will grow hot upon their mixing with gold, as chymical *non-entia*, or chimæras, yet they are not competent judges of the poſſibility of things. For I have more than once, or a few times, both alone, and in the preſence of ſome curious perſons, found and evinced, that a diſtilled mercury may be ſo animated, that a ſingle drachm of it, or perhaps a far leſs quantity, being mixed barely with my finger, with as much, or perchance half as much, calx of gold, would preſently conceive, not only a ſenſible, but a very conſiderable heat; inſomuch that ſometimes it would prove offenſive to the palm of my hand, wherein I made the mixture. Divers phænomena of this experiment may be ſeen in the author's little Tract *Of the Incaleſcence of Quickſilver with Gold*, now extant in the *Philoſophical Tranſactions, Numb.* 112. And I remember, that once being to convince a very eminent chymiſt, that there were ſuch mercuries, as I have been ſpeaking of, I took a remnant of a certain quickſilver, which I intend never to make again (and of which, for the ſake of mankind, I reſolve not to teach the preparation) of whoſe diſpoſition to incaleſcence I had ſuch an opinion, that though we had no calx, nor ſo much as filings of gold, but only ſuch pieces, as he could groſsly prepare with a hammer and a pair of ſciſſors, I ventured to put my mercury to them in a glaſs-mortar; and yet notwithſtanding the thickneſs and cloſeneſs of the beaten metal, and the coldneſs of the veſſel, the mercury, to the artiſt's wonder, penetrated the gold, and grew manifeſtly hot with it. And this faculty of our quickſilver was not a tranſient and eaſily vaniſhing one; for I had already kept the mercury by me for ſeveral years. The incaleſcent mercuries hitherto mentioned were animated by tedious and laborious

operations; but if I had deſired only to convince gain-ſayers, I could have done it by a very much ſhorter way: for though this ſort of impregnated mercuries be many degrees inferiour to the forementioned animated mercuries, yet as to incaleſcence with gold, I know by experience a way, which is indeed hard to hit, and requires a dexterous artiſt, but which, when it ſucceeds aright, will in an hour, and perhaps a leſs time, qualify mercury to grow preſently hot with gold.

6. WHEN an animated mercury is by due impregnation qualified to amalgam readily and intimately with gold, aud penetrated ſo as preſently to grow hot with it, it is not much to be admired, that it ſhould alſo differ from common mercury, in the being able to carry up with it part of the gold, wherewith it was ſo ſtrictly aſſociated. I know, that many learned men, and among them divers chymiſts themſelves, do not think it credible, that at leaſt corporal gold ſhould be volatilized by quickſilver. And indeed that, which is common, may be many times diſtilled from gold, without carrying up any of it; but this ought not to conclude againſt ſuch ſpiritually impregnated mercuries, as I lately mentioned: for with a very ſmall quantity of one of them I have ſometimes elevated ſo much calx of gold, that the inſide and neck of the retort were richly gilt by the adherent particles of that metal, which would ſometimes ſtick ſo cloſe, as not to be without difficulty ſeparated from the glaſs. And I remember too, that having with one of theſe noble mercuries amalgamed about half its weight at moſt (if I miſtake not) of calx of gold, though it did not gild the inſide of the glaſs, yet I found, as I expected, that the diſtilled mercury was manifeſtly encreaſed in weight, as well as ſomewhat changed in colour and conſiſtence: which experiment may be added to thoſe, that I formerly mentioned, to prove, that quickſilver (duly prepared) may be corporally impregnated.

7. IN the amalgams made of one of theſe ſpiritually impregnated mercuries with calx of gold, I have ſometimes obſerved a thing, that argues ſuch mercuries to be differing from common quickſilver: of whoſe amalgams with gold ſuch an effect has never (that I know) been taken notice of. The phænomenon I mean was this; that by diſtilling one of thoſe ſubtile amalgams in a retort, a good part of the bottom of the veſſel, which I have yet by me, was left adorned with a very lovely colour, almoſt like that of turquois ſtone, inclining towards the colour of gold, and ſomewhat changeable; and alſo ſo cloſely faſtened to the glaſs, that it ſeems to have penetrated into it, though this beautiful ſtain were left by but a very ſmall quantity of the amalgam, and though this mixture were diſtilled but in a moderate fire, ſince it was in a ſand-furnace. Nor is this the only experiment of this kind, that I would allege, ſince I elſewhere mention an amalgam of gold with an animated mercury, which being long decocted, when at length by an exceſſive fire unſkilfully adminiſtred the veſſel was unluckily broken, left the lower part of the glaſs permanently tinged a pure and tranſparent red, that ſeemed to me to emulate that of a not common ruby.

8. ANOTHER difference I found between ordinary quickſilver and ſpiritually impregnated mercuries, that will perhaps ſomewhat ſurprize you. And it is, that though one would expect, that amalgams made with mercuries ſo penetrant, and ſo diſpoſed to adhere cloſely to gold, ſhould make with it amalgams far more eaſy than thoſe made with ordinary quickſilver to be turned into red precipitate, yet I found the quite contrary upon trial. For whereas chymiſts are wont to mention about ſix weeks as the uſual time, wherein mercury may be precipitated even *per ſe*, that is, without additament, and allow but a ſhorter time to make this precipitation, when it is amalgamed with gold (whereby ſome of it is detained, and all more expoſed to the action

of the fire;) I have had the curioſity to keep an animated mercury amalgamed with about a third part of its weight of fine gold above twice ſix weeks, without having ſo much as a grain or two of precipitate (perhaps not half ſo much) that I could perceive, though the mercury grew hot with the gold at their being mingled, and though the mixture were purpoſely kept in a good heat capable to make quickſilver circulate. Nor did I content myſelf with one trial, nor with one ſort of animated mercury, but in above five or ſix months I obtained not one grain (that I could diſcern) of precipitate, though the heat was ſo ſtrong, as to carry up many parts of the quickſilver, and of the gold with it, to the top of the glaces; nay, in one of them (which was a ſomewhat odd caſe) the fire was ſo violent, that the hermetically ſealed glaſſes beginning to melt, the ſpirituous matter included in it was ſo forcibly expanded, as to ſtretch the weaker ſide of the glaſs, and give it as it were a bunch, yet without breaking it; as I can ſhew you in the glaſs itſelf, that I have yet entire by me. Nor do ſix months make the longeſt term, that the obſtinacy of my curioſity has made me keep gold in decoction with animated mercuries without obtaining a red powder or precipitate, though in the mean time there were produced very pretty vegetations, and ſometimes, which is far more conſiderable, odd changes of colours, about which it is not here neceſſary to entertain you; the main drift of this obſervation being to give you notice, that as far as I have yet tried, the more ſubtle and richly impregnated mercuries are far leſs apt to afford precipitates with gold, than common quickſilver is. As if that diſpoſition to be calcined (as the chymiſts are pleaſed to ſpeak) or turned into powder, required the preſence of the recrementitious or more ſeparable part of quickſilver, that a chymiſt would perhaps call it ſulphur, which was a diſcovery I could willingly enough have miſſed. For I confeſs I had ſome hope, as well as intention, to try, whether a precipitate, made with gold and ſome of theſe noble and richly impregnated mercuries, would not prove a nobler medicine than precipitates made with gold, and only common mercury; though even of ſome of theſe, when dexterouſly prepared and kept their due time in decoction, experience invited me to have no ſlight opinion; eſpecially, if they be exhibited in a juſt doſe, and accompanied with a proper additament, by which they may be kept from raiſing any ſalivation, and have their operation either altogether or almoſt totally determined downwards.

9. The laſt difference I ſhall obſerve between ſome diſtilled mercuries and common quickſilver, ſhall be their inequality in point of ſpecific gravity. I know you will think this a paradox; but I can tell you, that I had once the opportunity to examine hydroſtatically a noble mercury, for the impregnating whereof neither corporal gold nor ſilver was employed; and yet having carefully weighed this quickſilver in water, according to the method I elſewhere teach, in the preſence of a famous and very heedful virtuoſo, I found it, as I had foretold, not only manifeſtly, but very conſiderably heavier in ſpecie (that is, bulk for bulk) than common quickſilver, though this mercury had been ſeveral times diſtilled, and by other ways depurated: which to me ſeemed to argue, that even ſpiritual or volatile gold (for no viſible gold was employed, and no metal but gold is ſo heavy as quickſilver) is able to increaſe the ſpecific gravity of mercury itſelf, which, of all the bodies we know, is exceeded or equalled in that quality but by one alone. And the ponderouſneſs of our lately mentioned mercury ſeems to me the more wonderful, becauſe having by the ſame method hydroſtatically examined a mercury, made after a ſtrange way (without common mercury) I found it ſcarce at all to differ in gravity from common quickſilver, ſince it did not weigh full fourteen times as much common water of the ſame bulk.

BUT here I ſhall obſerve to you upon the by, that it is not a certain conſequence, to infer, that the heavier the mercury is, the more fixt it muſt be; for I remember, that having been once ſo unadviſed as to comply with the earneſt ſolicitations of an inquiſitive gentleman, that afterward behaved himſelf very ungratefully and unworthily to me, I gave him inſtructions, how to make an animated mercury, which I looked upon as very much of the like nature to the ponderous one, I have been ſpeaking of, but leſs tedious, and far leſs difficult to be prepared; and while he found he needed my renewed directions, according as new difficulties occurred to him, he gave me from time to time an account of his progreſs; and when he was advanced far in the proceſs, he informed me, amongſt other things, that following my direction in purifying and animating his quickſilver, he found it ſo altered and ſubtilized, that he would diſtil it in leſs than half the time he had formerly employed to drive it over, with the like fire and veſſels.

THIS is what I thought fit to ſay at preſent, about the differences between common quickſilver and prepared (but yet running) mercuries. And yet I am content to add two or three advertiſements, for which, and eſpecially for the firſt of them, you will perhaps thank me, if ever you ſhould vigorouſly proſecute, in a ſpagyrical way, the more noble ſort of mercurial experiments.

IN the firſt place then, I ſhall obſerve to you, that whatever ſome learned chymiſts, and others, teach to the contrary, it is matter of fact, that mercuries may be animated or ſpiritually impregnated by more ways than one (not to ſay, by more than a few) ſo as to penetrate gold very powerfully, and grow hot with it; and it ſeems to me very probable, upon grounds not meerly notional, that the differing ways, that are employed to prepare theſe animated mercuries, by impregnating them with this, or that mineral, or metal, may much diverſify their qualities and operations, according to the reſpective natures of the bodies they are impregnated with. Nay though there ſeem ſo great a diſtance between quickſilver and vegetable ſubſtances, yet I have ſeen a mercury, that was prepared by the help of vegetables without metals or minerals, which was very different from common quickſilver, by being more noble than it.

THE ſecond thing I am to acquaint you with, is, that as divers bodies and methods may be employed in the preparation of noble mercuries (as I have newly obſerved) ſo it ſeems very probable, that the common mercuries ſo prepared may have differing, as well as noble qualities and uſes, not only in reſpect of alchymy, but of medicine; as being fitted to have potent operations, as well upon human bodies, as the more ſtubborn ones of metals and minerals. I am not indeed at all forward to recommend the needleſs uſe of mercurial medicines, of which we may too often ſee bad effects, if they be not as well prudently and cautiouſly given, as faithfully and ſkilfully prepared: but ſince we ſee, that, in ſpight of *Helmont*, very many learned and experienced phyſicians allow themſelves to employ, frequently enough, even the vulgar preparations of common mercury, ſome of which prove indeed oftentimes in ſome ſtubborn diſeaſes far more efficacious than ordinary medicines; I ſee not, why we may not hope for greater and more innocent effects from a mercury well purified and impregnated with the ſulphur and finer parts of ſuch bodies, as volatile gold, or Venus, or Mars, or antimony, *&c.* And though, as I lately told you, I found ſuch animated mercuries far more indiſpoſed than common quickſilver, to make a precipitate with corporal gold; yet this need not hinder, but that divers other preparations may be made, as well with impregnated, as with vulgar mercury; ſuch as are turbith mineral, the white precipitate, that purges downwards, *mercurius dulcis*, pills of crude mercury made up with fit ingredients (as in thoſe, that are by ſome called the blue and the black pill) and eſpecially the cinnabar made by ſubliming quickſilver and ſulphur into a purely red ſubſtance, which, though wont to be employed chiefly by painters, ought not perhaps to be neglected by phyſicians; ſince even in ordinary cinnabar the vulgar mercury is ſo bridled by the common ſulphur, that unleſs too frequently given without pauſes, or in an indiſcreet doſe, it has not been uſually found to ſalivate, yet does often lie not idle nor uſeleſs in the body: ſo that it may be well worth trying, whether a noble cinnabar may not be obtained by ſubſtituting animated mercury for vulgar, eſpecially if inſtead of common ſulphur one ſhould employ that of antimony, or of antimony and vitriol, which I have elſewhere ſhewn to make.

THE third and laſt advertiſement I will give you, ſhall be, that you are not haſtily to conclude, that a mercury, that has been carefully depurated and impregnated, has not been well prepared, if you find it not readily to elevate corporal gold, as it may ſeem by the paſt diſcourſe, that moſt of the animated mercuries I have mentioned did. For though it be true, that I have had ſome mercuries fitted to penetrate gold ſo far, and mix with it ſo cloſely, that it would quickly upon diſtillation viſibly carry up ſome of that ponderous metal with it; yet ſo much is not to be expected from all mercuries, that may lay claim to the title of animated or noble. For I have found, that ſome even of theſe may require a ſtrong decoction to incorporate them intimately with gold; and I remember, that once, for trial-ſake, I made mercury, which upon bare diſtillation would not at all colour the glaſs: I made, I ſay, this mercury, by decocting or circulating it with the gold for ten days or a fortnight, unite ſo cloſely with the metal, that it would afterwards elevate enough of it to gild the inſide of the glaſs; and by a much longer decoction I have ſometimes had the gold lodged copiouſly in the upper part, and even in the neck of conſiderably tall glaſs-eggs, hermetically ſealed, one or two of which I can yet ſhew you.

OF THE

Producibleneſs of PHLEGM or WATER.

PART V.

OF the ſeveral ſubſtances, that chymiſts obtain by the fire from mixt bodies, that, which they call phlegm or water, and would have men look upon as mere water, ſeparated by a preceding analyſis, ſeems to the Helmontians, and divers other modern artiſts, to bid the faireſt for the title of elementary and primordial. Wherefore it will now be fit to conſider, whether, about that alſo, we may not juſtly retain ſome doubts, and rationally ſuſpect, that all, that they call the phlegm of bodies, was not in the form of elementary ſimple water, pre-exiſtent in the body, whence it is obtained; but that even ſuch portions of matter, as many of thoſe, that paſs among chymiſts for phlegms, may be produced either by the operation of the fire, or by other ways.

In order to this inquiry, it will be fit to premiſe ſomething againſt the preſumed ſimplicity and homogeneity of the liquors, whereto the Spagyriſts give, in common, the name of phlegm; that in caſe ſome of the produced liquors we ſpeak of ſhall be denied to be preciſely of an elementary nature, it may appear, that that ought not to hinder us from allowing them the name of phlegm, provided that they be not remoter from ſimplicity, than thoſe, to which chymiſts grant that appellation.

And firſt, I conſider, that beſides thoſe qualities, that are common with water to divers other liquors confeſſedly not ſimple, as tranſparency, want of colour, aptneſs to be imbibed by moſt ſorts of vegetable and animal ſubſtances; the two qualities upon whoſe account chymiſts are wont to call a body phlegm or water, are its appearing to them inſipid, and its being of a volatile and fugitive nature.

I further conſider, that not only divers of thoſe liquors, that paſs for phlegm, will yield a taſte ſenſible enough to him, that will hold them with attention of mind, for a competent time, in his mouth, but that the criterion of liquors by the taſte is nothing near ſo certain as many think. For, not to mention, that it is plain, that ſome kinds of dogs, as ſetters, ſpaniels, and blood-hounds, take notice of many things by their odours, that we men have no perception of by our ſmelling, which may argue, that our ſenſes may not be moved with objects, that would affect them, if they were of a more delicate contexture; not to mention this, I ſay, it is plain, that the ſubtlety of the ſenſe of taſting differs among men themſelves. And thoſe that drink nothing but water, will often tell us of a great diſparity betwixt common water, wherein other men find not any. And I remember, that when once I did, though but for ſome months, confine myſelf to drink water, I could diſtinguiſh the limpid waters of differing places, almoſt as manifeſtly as I now diſtinguiſh beers, which, after I fell again to drink wine and other liquors, I ceaſed to be able to do.

The conſideration of quickſilver may, methinks, let us ſee, that it is poſſible for a groſs and fluid body, that is far from elementary water, to be inſipid. For quickſilver is without queſtion a fluid, and at leaſt in reference to ſome bodies, gold, ſilver, and ſome others, a liquor; ſince it ſoaks into their pores, and ſoftens the bodies. The ſame quickſilver may alſo ſerve to ſhew, by its diſpoſition to fly away in the fire, that volatility, even in conjunction with inſipidneſs, is no certain mark of an elementary or ſimple, nor conſequently of a primordial body. And indeed, ſince volatility depends mainly upon the extraordinary minuteneſs of the particles, whereof a body conſiſts, and on their being incoherent, and of ſhapes fitted for motion; this quality may be acquired by ſo many differing ways, and be found in bodies otherwiſe of ſuch differing natures, that unleſs it be found aſſociated with the other qualities proper to phlegm, it will be but an unſure argument, to prove the body, that it belongs to, to be elementary, and not to have been by compoſition, diviſion, or tranſpoſition produced.

If it be true, as the Carteſians will have it, that water conſiſts of particles, that, like little eels, are long and extremely ſlender, and conſequently flexible; I ſee not any impoſſibility, that the various action of the fire, upon the minute parts of a body, and that, which it may cauſe the corpuſcles of one body to have upon thoſe of another, may produce water, that did not in the form of water pre-exiſt in the body, that affords it: for it ſeems to me poſſible enough, that the particles, whereof a corporeal maſs is made up, may have ſuch ſhapes, ſizes, and contextures, that by the various agitation, which the pervading corpuſcles of the fire may produce amongſt them, whatever edges and points they had before, may by mutual attrition of the corpuſcles be worn off, and by the ſame means ſo much of the ſubſtance may be worn away, that what remains, cannot but be very flexible, and by all theſe qualifications become fit to make a particle of water. As a bar of iron may, by divers ſtrokes of the wedge and hammer ſkilfully employed, be divided into long and ſlender parts, whoſe edges and points being blunted, they may be reduced into ſlender, and every way flexible wires. But not to build on ſpeculations, let us proceed to ſome experiments, which afford phænomena, that ſeem favourable to out hypotheſis.

Amongst the bodies, about which chymiſtry is converſant, thoſe that ſeem to be the moſt indiſpoſed to be turned into water, are the metalline, and mineral ones: ſo that if it can be made appear, that any of this ſort can be changed into an aqueous liquor, it will make it highly probable, that aqueous liquors may be by chymical operations produced, eſpecially in vegetable and animal bodies; which ſeem far more ſuſceptible of ſuch a change, than the ſtubborn ſubjects of the mineral kingdom. And ſince quickſilver is by many learned men, as well chymiſts as others, looked upon as one of the few moſt indeſtructible bodies in nature, and by its great ponderouſneſs, in which it exceeds all the known bodies of the world ſave one, is ſo much the more remote from ſuch a liquor as water, that has not the ſixteenth part of its ſpecific weight; if quickſilver itſelf can in great part be turned into an aqueous liquor, it will not a little favour the doctrine propoſed in theſe notes; for which reaſon I ſhall ſubjoin the enſuing ſtory.

Relating to a very ingenious and ſober phyſician of my acquaintance what had befallen me in diſtilling mercury, from whence I once obtained a water without additament, without being able to make the like experiment afterwards ſucceed; he aſſured me, that he had a friend of his, had ſome years paſt provided a very large Dutch retort of good earth, furniſhed with a pipe of about a foot long, to caſt in the mercury at; and that having by little and little conveyed through that pipe a pound of quickſilver into the cadent retort, they obtained four ounces of water, and loſt in ſpite of their care two ounces of matter (whatever it were) the remaining part of the pound having been elevated in the form of mercury. And when I ſuggeſted, that perhaps the water, that came over, was afforded by the aqueous particles of the earthen retort itſelf, he replied, that, to prevent their being impoſed on, they had been careful not to put on the receiver, till the retort had been made throughly glowing hot: and that this liquor was far from common water, he thought to be paſt doubt by that, which follows. For I having acquainted him with an odd trial or two, I had made with mercurial water, and aſked him, whether he found the like effects from his, he told me, that his friend and he poured both their diſtilled mercury and their water into a kind of China cup, and though it were in *June*, left it open in a garret for two or three days, upon a preſumption his friend had, that this mercurial water thus ordered would turn a good part of the quickſilver into its own nature, and and ſo multiply itſelf upon it. But when they came to viſit their cup again, they were much ſurpriſed to find their water all gone, and that the greateſt part thereof was turned again into mercury; which they concluded from this, that they miſſed upon the balance but about half an ounce of the whole matter; which (half ounce) they ſuppoſed to have been loſt by evaporation; the other three ounces and a half being found in the increaſed weight of the mercury.

The mention I have made of quickſilver, puts me in mind of an argument *ad hominem*, that may deſerve to be conſidered by the chief ſect of modern chymiſts, the Helmontians: for if it be true, which their maſter teaches, that by his liquor alkaheſt, not only quickſilver, but all other tangible bodies, may be reduced into inſipid water, juſt like rain water, I may be allowed to infer, that water may be produced, ſince ſalt and ſulphur themſelves may be turned into water. I know the Helmontians may anſwer, that this is not ſo much a production, as a reduction; ſince all things conſiſting originally of water, the alkaheſt does but deprive it of the diſguiſes, that ſeminal principles put it into, to make it appear, under the form of gold, quickſilver, plants, animals, *&c.* But this anſwer may be elſewhere further examined: for the preſent, it may perhaps be ſufficient to reply, that even by this anſwer it is granted to appear by experiment, that water has been copiouſly produced out of mineral bodies, but it has not yet been made appear, that thoſe bodies were produced out of water.

But this is not all, nor perhaps the principal thing I have to ſay in favour of the opinion pleaded for in theſe notes. For ſuppoſing bodies by being reduced, by the alkaheſt and the fire, into an inſipid liquor, were really deduced into water, yet the Helmontians would not fully ſatisfy me. For *Helmont* relates, that by abſtracting his immortal liquor from ſtones, or ſuch kinds of bodies, he turns them into ſalt equiponderant to the concrete; which ſalt by further operations he reduces, as he ſuppoſes, into elementary water. Since then he ſtops not at ſalt, but goes to a further tranſmutation, and concludes, that a ſtone doth not conſiſt of ſalt, becauſe that ſalt may, by further operations, be turned into inſipid water; he muſt give me leave, on the ſame ground, to argue, that inſipid water is not the firſt matter of bodies, ſince by a further operation of the fire, that liquor itſelf may be, at leaſt in great part, turned into earth. For I elſewhere relate ſome experiments of my own and a friend's, in which clear water, divers times very ſlowly diſtilled out of clear glaſs bodies, left every time a terreſtrial powder at the bottom: as if (in caſe water be ſo homogeneous a ſubſtance as is ſuppoſed) the whole body of the water might, by reiterated diſtillations, without violence of the fire, be reduced into earth; whereof I remember, in the laſt trial of mine, I had enough to cover the bottom of a large cucurbit, out of which the diſtillations had been made.

And on this occaſion, I ſhall add a trial, which ſeems to argue, that without the help of often repeated diſtillations in tall cucurbits, clear water itſelf may, by the operation of the fire, be changed into another body.

We took, then, very pure and limpid water, which had, by our pneumatic engine, been carefully freed from the aërial particles, that are wont to be harboured in the pores of that liquor: this in a new bolt-head of ſuch a ſize, that the matter might have room to play and circulate, we ſealed up hermetically; and placing the veſſel in a digeſtive furnace, we left it there above a year, and obſerved, as we expected, that after it had continued for a good while, there began to form themſelves, in the water, little concretions heavier than it, which in proceſs of time increaſed in magnitude, and, as we thought, in number; making a kind of *terra foliata*, that conſiſted of a multitude of little thin films (or ſcales like thoſe of the ſmaller ſort of fiſhes) which, when the glaſs was ſhaken in an enlightened place, were copiouſly diſperſed through the body of the liquor, and appeared variouſly and vividly coloured, being ſome of them almoſt as big as ſpangles, and more glittering: and when the agitation ceaſed, they preſently fell to the bottom, which they covered in the form of a *terra foliata*; by their ſubſidence manifeſting themſelves to be notably heavier, in ſpecie, than the water they had been formed of. And the longer the glaſs was kept in the digeſtive furnace, the more of this fine terreſtrial ſubſtance was produced: and leſt the effect ſhould be aſcribed to the abſtraction of the air from the water, handled as is before related, I ſhall add, that we produced the like ſubſtance, though, as it ſeemed, not ſo copiouſly, after the like manner, in water, that had not at all been freed from air.

OF THE

PRODUCIBLENESS OF EARTH.

PART VI.

OF all the ſubſtances obtainable from mixt bodies, that which, to perſons not prepoſſeſſed with Helmontian opinions, may ſeem the moſt ſimple, elementary, and unchangeable, is that, which they call earth or *terra damnata*; becauſe there is ſuppoſed to be no doubt, but that the calcining or incinerating violence of the fire, muſt not only have driven away the mercurial and other volatile parts, but muſt alſo have quite burnt out the ſulphurs, which oftentimes are more fixt than the reſt; as the water on the other ſide muſt have diſſolved away all the alkali or fixt ſalt.

THIS ratiocination, I confeſs, is very ſpecious and probable, but yet not ſo ſatisfactory, but that a ſceptic may retain not irrational doubts about the cogency of it, upon ſuch conſiderations, as I am now going to propoſe.

AND I will begin with conſidering, that, whereas the things, wherein this ſuppoſed ſimplicity, and unchangeableneſs of the earthy part of mixt bodies, is founded, are theſe three: its not diſſolving in water; its not affecting the taſte; and its not having flown away from the incinerated body, that afforded it; it may with probability be doubted, whether any of theſe, or all of them put together, do neceſſarily evince what chymiſts pretend they do.

AND in the firſt place, according to the different conſtitutions of certain ſorts of bodies, I think it fit to make a diſtinction between the dry and heavy parts, that remain after a body has been expoſed to the violence of the fire, and, if need be, freed from its ſalt, as much as it can be, by the affuſion of water. For it is evident, that in ſome bodies, eſpecially of a metalline nature, the fire, that makes that which they call calcination, does not operate as it does in the burning of vegetables. For, beſides that ſometimes almoſt (and ſometimes more than almoſt, the whole weight of a mineral is to be found in that which they call its calx, and is manifeſt in the calcination of lead and tin *per ſe* (if ſkilfully performed) the calx is in great part reducible ſometimes into a body of the ſame nature with that, which afforded it, and ſometimes into ſome other body, very far from being elementary: as is manifeſt partly in the reduction of minium (which is calx of lead made *per ſe*) which, as to the greateſt part of it, we have more than once, by the bare way of ordering the fire, reduced in a very ſhort time, and without additaments, into malleable lead; and partly in the calces or (as they alſo ſpeak) aſhes of antimony, which by bare fuſion are eaſily reduced into glaſs, whence we have ſometimes obtained an antimonial regulus. So that it is manifeſt, that there is a great deal of difference, between the aſhes (taking that word in a large ſenſe) of metals, and of ſome minerals, where almoſt the whole body is by the fire converted into a dry and heavy powder, and the aſhes of incinerated vegetables, which uſually leave but a little quantity of earth behind them, in compariſon of the matter, which the violence of the fire hath driven away.

But ſetting aſide the above-mentioned metalline calces, which without queſtion remain compounded bodies, if metals themſelves be ſo; and to forbear examining, whether they be not further compounded with corpuſcles of the fire, or fuel, that are embodied with them: I conſider, that the qualities, which make other aſhes paſs for elementary earth, may be produced in portions of matter, that are not ſimple, either by compoſition or change of texture.

I have elſewhere occaſion to take notice of bodies, which, though, when they are ſingle, they will eaſily diſſolve in water, yet the reſult of them will not: and you may find inſtances of this kind, among the magiſteries, as chymiſts call them, made of ſeveral bodies, by precipitating their ſolutions (made in acid liquors) with oil of tartar *per deliquium.*

From oil of vitriol and ſpirit of wine, though both moſt readily diſſoluble in water, we have, by bare digeſtion and diſtillation, obtained a pretty quantity of a ſubſtance, that we found not to diſſolve in water, and which ſeemed to be inſipid and fixt enough.

There are ſtones, ſome more and ſome leſs precious, which though I could by the help of the fire deprive of their colour, and bring to a white powder; yet it did not appear to me, that they were really calcined, or would in water yield any ſalt: ſo that if theſe ſtones be compounded bodies, as Spagyriſts tell us they are, we ſee, that there may be other corpuſcles beſides metalline ones, which though reduced by the help of the fire to a white powder, inſipid, and not diſſoluble in water, are yet remote enough from an elementary nature.

But I need ſeek no further, for inſtances of this kind, ſince Spagyriſts themſelves teach us, that the aſhes of wood may, by the violence of the fire, be turned into glaſs; which being a body compoſed of the earthy and ſaline part of the aſhes (for they tell us, that earth ſeparated from the ſalt will never be vitrified) muſt be according to their own confeſſion a compounded body: which being at laſt made by the utmoſt violence of the fire, muſt be fixed, indiſſoluble in water, and conſequently inſipid. And without taking this vitrification upon the chymiſts authority, it is manifeſt, that in glaſs made the common way, there is a great deal of borellia, ſal alkali, or other lixiviate ſalt mixt with the ſand: as is evident, not only becauſe artificers find the ſalt needful to diſſolve the ſand, and bring it to fuſion, but becauſe the glaſs uſes to weigh very much more, ſometimes (as an ingenious maſter of a glaſs-houſe anſwered me) thirty, or forty pound in an hundred, than the ſand, that was put in.

I shall add, that, ſince chymiſts aſcribe all odours to ſulphur, I may reaſonably conclude againſt them, that in ſpite of all the violent fire, which is required to the making common glaſs, there is ſtore of ſulphur, as well as ſalt in it. For I have often tried, that by barely rubbing two large pieces of glaſs, one againſt another, there would quickly be produced a ſtrong offenſive ſmell. And yet glaſs, which thus appears to be, not only a compounded, but a decompounded body, ſince the ſand or cugali (as the Venetian glaſs-men call their pebbles) or other ſtones being themſelves mixt bodies, are further compounded with the ſalts that diſſolve them: glaſs, I ſay, is a body, that manifeſtly poſſeſſes all theſe three qualities, which chymiſts require in their earth, being taſteleſs, indiſſoluble in the water, and fixed in the fire. And if aſhes alone be (as chymiſts teach us they are) capable of vitrification (and indeed an inquiſitive owner of a glaſs-houſe anſwered me, that once, if he much miſ-remembered not, he made, but not eaſily, glaſs of aſhes alone without ſand;) how are we ſure, but that in common aſhes, freed the common way, from their fixt ſalt, that which is called the ſimple earth, may not be a body compounded of two or more ſubſtances, which by their coalition, and new texture produced by the action of the fire, have been brought to a kind of vitrification, or otherwiſe have acquired the few obvious qualities, that chymiſts are wont to think ſufficient to give a production of the fire, the name of earth?

It is obvious to obſerve, that divers bodies, when they come to be of ſenſible bulk, will ſink in liquors, in which their corpuſcles would freely ſwim, if ſo many of them did not ſtick together. As of ſalt and ſugar, the lumps, and even the grains, whilſt they remain ſuch, will fall to the bottom of water, in which when they are diſperſed into minute and inviſible corpuſcles, they will eaſily ſwim. And I have obſerved, that in ſtopt glaſſes ſome ſalts, and other bodies, that for many months remained undiſtinguiſhed in the liquors, that harboured them, would in tract of time have conventions made of their particles, that would then ſubſide, and be no more carried up and down by the particles of the liquor. And I ſee no impoſſibility, that ſomewhat of this kind may happen to the particles, whereof water conſiſts: for if ſome of theſe, by frequent occurſions and attritions come to apply themſelves to one another, ſo as to have a fuller, and more immediate contact than formerly, and to be intangled among themſelves, and perhaps alſo to exclude ſome very thin and ſubtile air, that may be ſuſpected to lurk about them, and contribute to their ſuſtentation; if, I ſay, this union, or ſtrict adheſion of aqueous corpuſcles, ſhall happen to be made, the aggregates or cluſters, though as to ſenſe but very ſmall, may be too great and unwieldy to be any longer parts of water, but may ſubſide in that liquor; and if their union or adheſion be ſtrict enough, they will upon the ſame account be unfit to be carried up in the form of vapours, and exhalations by heat, and ſo may be like earth fixed in the fire, as well as not diſſoluble in water.

I HAVE ſometimes alſo had a ſuſpicion, that the production of an earthy ſubſtance in water may be furthered by the particles of fire, that are employed to make it circulate; and that of thoſe igneous particles, which, as I am apt to think, pervade the glaſs, ſome of the groſſer, or rather the leſs ſubtile, may in their paſſage faſten themſelves to ſome aqueous particles, fitted to adhere to them, and may with theſe begin to make ſome inviſible concretions; to which afterwards, other congruous particles may, little by little, ſtick, in their paſſage, and ſo at length compoſe ſenſible aggregates of powder: which may be illuſtrated by what happens in the precipitation of quickſilver without addition, where the mercurial particles being aſſociated by, and probably with ſome of thoſe of the fire, begin to form concretions, at firſt very minute, which afterwards increaſe more and more, by the acceſſion of other adhering particles, till all the mercury, or the greateſt part of it be reduced, from a fluid body, to a red powder. And perhaps it may countenance my conjecture, about the production of an earthy ſubſtance, by a briſk concurrence of the particles of fire, if I add, that though I have kept high rectified ſpirit of wine for above a year together hermetically ſealed, and for the moſt part of that time in a digeſtive furnace, without finding any earthy reſidence; yet, when I ordered a bolt-head, that, though it were hermetically ſealed, the alcohol of wine that was put into it might be boiled without breaking the glaſs, I found, that in a ſhort time this liquor would afford a not inconſiderable quantity of ſuch a ſubſiding talcky ſubſtance, as I obtained from the water, formerly mentioned. But theſe things I need propoſe but as illuſtrations, that may ſomewhat help you, to conceive how water may be turned into earth. For, whether it be by theſe, or any other ways, that the thing is performed, yet ſince the experiment formerly recited, that water by frequent cohobations may be more and more turned into earth, argues the matter of fact, our not being able to explicate the manner, does not hinder the thing from being true, nor the argument we build on it from being good: ſince even water, to which, by ſome operations of the fire and the alkaheſt, it is pleaded, that bodies are ultimately reduced, may itſelf by a further and very ſimple operation of the fire be reduced into earth.

I HAVE ſomewhere mentioned, for I remember not in what paper I have obſerved, ſalt-petre diſtilled with clay, to loſe much more of its weight, than can be ſuppoſed to have aſcended in the form either of ſpirit or phlegm. And now to make this experiment more ſhort and eaſy, I ſhall add, that I lately made it in a crucible (inſtead of a retort) wherein a double weight of finely powdered tobacco-pipe clay, very well mixt with pulverized cryſtals of nitre, were kept three hours, in a violent fire, and then the mixture being taken out, the remaining fixt ſalt was carefully extracted, but amounted to very little in compariſon of what nitre was wont to yield, when calcined with charcoal. And that this ſcant proportion of fixt ſalt did not proceed chiefly from a very copious avolation of nitrous ſubſtance, appeared probable by this, that the *caput mortuum* was much more ponderous, than was to be expected, upon the ſcore of the tobacco-pipe clay, that was firſt employed, and the alkali, that was extracted; ſo that the new weight acquired by the clay ſeemed manifeſtly to proceed from the acceſſion of a portion of the ſalt-petre, that by this operation was turned into earth. Inſomuch, that of ſix drachms, that four ounces of clay had acquired in weight, after the crucible was taken out, not ſo many grains could even by boiling water be obtained from the whole *caput mortuum*: which, when firſt taken out of the crucible, was almoſt quite inſipid.

THAT earth may be *de novo* produced, may likewiſe be probably argued from the experiment I formerly related about the deſtruction of the ſalt of tartar, by igniting it, and putting it into fair water; for there remained after the numerous filtrations, and, if I miſremember not, after every one of them, a ſubſtance in the filter, which, for aught appears, may be as well called earth, as that, which was ſeparated from the calcined tartar, the firſt time it was put in the water, to divide the ſalt from the earth. For in our experiment, as well as in the common operation I come from mentioning, the way of proceeding is the ſame, and in both there remains in the filter a ſubſtance, which, by its ſtaying there, ſhews it was not diſſoluble in the water, and which before it came thither, ſhewed, by its enduring a violent fire, that it was alſo fixt as earth ought to be. Nor would it much move me, if it ſhould be found, that this factitious earth may have ſome ſuch operation upon ſome particular body, as is not thought to belong to true elementary earth. For ſince it is obtained by a chymical analyſis, if it have thoſe qualities, that in the general eſtimations of naturaliſts make up the notion of what they call earth; I think that ought to ſuffice us, at leaſt till the chymiſts give us ſome accurate notion of genuine earth, and ſhew us ſuch a thing, and teach us a better way of analyſis, to obtain it.

FOR in many bodies, that are, without ſcruple, looked upon as earth, I obſerve qualities, that do not belong to the received notion of elementary earth. This I ſay, becauſe I ſee not, why ſuch a texture, as will ſuffice to make a portion of matter indiſſoluble in water, fixt in the fire, and inſipid upon the tongue, may not alſo make it fit to operate actively upon ſome bodies, and modify the operation of ſome others, that act upon it. And if our earth from ſalt of tartar be rejected as ſpurious, they ought to confeſs the inſufficiency of their common way of ſeparating a true earth from the bodies they analyze: for it ſeems calcination, and ſolution in water, and filtration, which make up their uſual method, will not ſuffice to make our earth of

tartar

tartar paſs for true; though it appear not to be near ſo remote from an elementary nature, as ſome other bodies, that are obtained from earth by the vulgar analyſis. Of which I ſhall at the cloſe of theſe notes give an inſtance, in well-dulcified quicklime, which, according to the doctrine of the chymiſts, ought to be an elementary earth; and yet ſeems not more ſo, (if it be ſo much) as our earth from ſalt of tartar. And for the preſent, I ſhall add, that the *caput mortuum* of vitriol remaining after it had long endured a violent fire, though it were diligently freed from ſaltneſs by reiterated ablutions with hot water, was yet far from being an elementary earth; as appeareth not only by its deep purpliſh colour, and its ponderouſneſs, far exceeding that of earth, but by a trial, that I purpoſely made to examine it.

UPON this occaſion I remember, that a learned man of my acquaintance, who viſited the mines of *Hungary* (and dealt much in Hungarian vitriol) affirmed to me, when I told him I conceived the *caput mortuum* of it to retain much of the metalline nature, that he had, upon a certain occaſion, out of the colcothar of a certain ſort of Hungarian vitriol, not only received a pretty deal of good copper, but ſeparated from that copper a pretty portion of ſilver, and ſome grains of true gold.

BEFORE I put a period to theſe notes about earth, though my argument do not need nor require, that I ſhould do it, yet upon this fair occaſion I ſhall here take leave to doubt, whether ſuch an elementary earth, as the ſchools tell us of, do yet appear to be more than a notional thing. For to what I have already ſaid concerning the earths ſuppoſed to be produced by chymical analyſes, I ſhall now add, that I have not yet ſeen it proved, that nature does, any more than art, afford us a true elementary earth; at leaſt I can ſay, that ſome, which ſeem to be of the more ſimple ſort, I found upon examination to have qualities not aſcribed to pure earth. For though tobacco-pipe clay, by reaſon of its fixity, whiteneſs, and inſipidneſs, and its lying oftentimes deep enough beneath the ſurface of the ground, may, as probably as almoſt any other native earth we know, be looked upon as elementary; yet tobacco-pipes well baked may ſometimes be made to ſtrike fire. And I have more than once tried, that by briſkly rubbing two pieces of a new tobacco-pipe, one againſt another, they would in a minute or two of an hour grow warm; and being immediately ſmelt to, manifeſtly afford a rank odour, between ſulphureous and bituminous, almoſt like that, which proceeds from pebbles or flints, when they are rubbed hard againſt one another. As if tobacco-pipe clay were not a true earth, but a fine white ſand, conſiſting of grains too ſmall to be diſtinctly taken notice of, like thoſe of other ſand. On which occaſion I ſhall add, that I found, by a hydroſtatical trial, that its ſpecific gravity was but little differing from that of pebbles, its proportion in weight to water of the ſame bulk being as two and a quarter to one. A tobacco-pipe may alſo be ſomewhat melted by a very vehement fire (for a leſs will ſcarce ſerve the turn) as may be argued from this, that it may by ſuch a fire be brought to bend.

PORCELANE, or the matter, whereof China diſhes are made, is not, as ſome travellers and learned men have fondly imagined, a compoſition, that requires to be buried under ground, for I know not how many years, to ripen it into porcelane; but as ſome late authors inform us, and as I have been aſſured by a perſon, that went purpoſely to that place in *China*, that is ſo famous for the making of porcelane veſſels, it is a pure ſort of clay; but yet this I find not to be elementary earth. For beſides that I have obſerved, that a violent fire will make it ſomewhat melt; I find, that with ſteel it will eaſily enough ſtrike fire almoſt like a flint. The lik I have obſerved in porcellane very artificially imitated with a ſort of Engliſh clay. And I found too, that the matter even of dark-coloured jugs of the better ſort, and well

baked,

baked, would with a ſteel afford ſparks of fire. I forgot to tell you, when I was ſpeaking of fine porcelane, that I found its ſpecific gravity to be very near the ſame with that of flints, and tobacco-pipe clay. But I remember I went once to viſit a grove or pit, where at the depth of divers yards, they were wont to dig up a certain white earth, which diſtilled, by an acquaintance of mine, afforded a liquor, that was put into my hand to try, and which I found to be very rich in a volatile ſalt, that taſted and ſmelt much like ſpirit of urine, or hartſhorn, and had almoſt the ſame effects in changing the colours of ſome bodies, and precipitating of others. I remember too, that I found by the operation of a menſtruum or two upon tripoli, that, as white and pure a virgin earth as it ſeemed, yet it was not elementary: and, on the other ſide, a maſter of ſome Engliſh mines having preſented me, among other minerals, which he knew not what to make of, a very white ſubſtance, whereof he had ſtore, which he thought an earth, and which was judged by an excellent artificer, much converſant with tripoli, to be finer even than that earth, I gueſſed it upon examination to be a kind of talc, whoſe leaves were exceeding fine and minute. The reſult of theſe relations may be, not only, that we may yet retain our doubts, whether the aſſertors of elementary earth can ſhew us any native ſubſtance, that deſerves that name; but alſo, whether thoſe things, that remain after chymical analyſes, though they have all the qualities, that are judged ſufficient to denominate a portion of matter, earth, may not yet either be compounded bodies, or be endowed with qualities, which belong not to ſimple earth. To explain and confirm which, I ſhall give an inſtance in ſome quick-lime, that I purpoſely examined. For though it had been, by frequent ablutions in warm water, carefully dulcified, and ſo was as well inſipid, as fixt, and indiſſoluble in water; yet I found, I could readily diſſolve it in divers menſtruums, and even ſpirit of vinegar: whereas true elementary earth ought to be as well indiſſoluble in ſuch liquors as in water.

If I had not been to deal with chymiſts, but Ariſtotelians, I might have ſaved myſelf the labour of ſolicitouſly endeavouring to prove, that earth and water may each of them be produced from bodies of a differing nature from it. For though the Peripatetics will not allow the whole elements to have been produced, becauſe they look upon them as integrant parts of the world, which *Ariſtotle* laboriouſly (though not ſolidly) maintains to be eternal; yet the portions of the elements they will have to be interchangeably tranſmutable. So that what was once water may be earth, and earth may, by intermediate alterations, be turned into water.

But thoſe I have to do with, being the vulgar chymiſts, who will have the material principles or ſimple ingredients of mixt bodies co-eval with the world, and uncapable of being either deſtroyed or produced; it was not allowable for me to proceed upon the Ariſtotelian hypotheſis, of the tranſmutableneſs of what they call elements; eſpecially becauſe, that though I do admit it, as it is, according to my opinion, a part of a more general truth; yet I do not think, they have well proved it by their arguments: which ſince I myſelf do not like, I think it were diſ-ingenuous to preſs them upon others. And without having recourſe to their doctrine, there will occur ſome other particulars, that may be added to thoſe already mentioned, to countenance the producibleneſs of the principles of mixt bodies, in ſome other papers, that are to follow theſe notes; though in ſtrictneſs I was not obliged to ſay ſo much, as I have already ſaid, ſince pretending to call in queſtion no more than the three hypoſtatical principles of the chymiſts, I might eaſily have excuſed myſelf for having let alone the production of water and earth; ſince the generality of chymiſts reckon not thoſe bodies amongſt their hypoſtatical principles, but look upon them, as receptacles, or, as others would have them recrements of theſe, or as on ſome other account, related to them. But I was not willing to omit water and earth, becauſe ſome of the more learned of the modern Spagyriſts have adopted them into the number of the principles of mixt bodies; and becauſe I find by experience, that in chymical analyſes they are at leaſt as often to be met with, as ſome of the principles confeſſedly hypoſtatical. But what has been delivered about earth and water having much added to the bulk of theſe notes, it is time I ſhould put a period to this part of them, in reference to which I hope it will be conſidered, that I do not pretend, that every ſingle experiment by me alleged does ſufficiently prove, that the body obtained by it was in the ſtricteſt ſenſe produced. For if the ſeveral experiments, and other proofs, do in conjunction, and, as it were in a body, make up a good argument, that the ingredients they relate to, may be produced; it is as much as will, I hope, be expected from theſe notes, which having been written by way of appendix to the *Sceptical Chymiſt*, may be allowed, as well as that book, to employ ſome proofs, not altogether cogent, and may be judged not improper, though ſome of the arguments propoſed in them, to ſhew, that chymical principles are not all ingenerable and indeſtructible, ſhould be but meerly probable. And yet this I ſhall venture to intimate, that vulgar chymiſts and Ariſtotelians may not, perchance, find it ſo eaſy a thing, as it is like many of them will imagine, to confute divers paſſages of the foregoing tract; ſince probably their objections will ſuppoſe ſomething or other, which, though taken for granted among them, and perhaps by ſeveral other learned men, I do not admit as true, or think demonſtrable, but rather queſtionable, upon very rational grounds. And though perhaps I ſhould not have brought in ſome of the experiments mentioned in the preceding notes, if I had not had a mind to throw together what I thought might contribute to ſo uſeful a thing, as a natural hiſtory of chymical principles, on which others, if not I, may hereafter ground a theory of them; yet this may alſo deſerve to be conſidered, that if any of the foregoing experiments, though never ſo few, do really prove (as it is like ſome of them will be judged to do) that any one of the chymical principles may be, *de novo*, produced; that alone will ſuffice to deſtroy the univerſality and intireneſs of their hypotheſis; and beſides give cauſe to ſuſpect, that by further induſtry, the producibleneſs of other principles alſo may be diſcovered.

EX:

EXPERIMENTS AND CONSIDERATIONS TOUCHING COLOURS.

First occaſionally written, among ſome other *Eſſays*, to a Friend; and now ſuffered to come abroad as

The BEGINNING of an

EXPERIMENTAL HISTORY of COLOURS.

The PREFACE.

HAVING, in convenient places of the following treatiſe, mentioned the motives, that induced me to write it, and the ſcope I propoſed to myſelf in it, I think it ſuperfluous to entertain the reader now with what he will meet with hereafter. And I ſhould judge it needleſs, to trouble others, or myſelf, with any thing of preface; were it not, that I can ſcarce doubt, but this book will fall into the hands of ſome readers, who being unacquainted with the difficulty of attempts of this nature, will think it ſtrange that I ſhould publiſh any thing about colours, without a particular theory of them. But I dare expect, that intelligent and equitable readers will conſider on my behalf, that the profeſſed deſign of this treatiſe is to deliver things rather hiſtorical than dogmatical, and conſequently, if I have added divers new ſpeculative conſiderations and hints which perhaps may afford no deſpicable aſſiſtance towards the framing of a ſolid and comprehenſive hypotheſis, I have done at leaſt as much as I promiſed, or as the nature of my undertaking exacted. But another thing there is, which if it ſhould be objected, I fear I ſhould not be able ſo eaſily to anſwer it; and that is, that in the following treatiſe (eſpecially in the third part of it) the experiments might have been better marſhalled, and ſome of them delivered in fewer words. For I muſt confeſs, that this Eſſay was written to a private friend, and that too by ſnatches, at ſeveral times and places, and (after my manner) in looſe ſheets, of which I oftentimes had not all by me that I had already written, when I was writing more; ſo that it needs be no wonder if all the experiments be not ranged to the beſt advantage, and if ſome connexions and conſecutions of them might eaſily have been mended: eſpecially ſince, having careleſsly laid by the looſe papers, for ſeveral years after they were written, when I came to put them together to diſpatch them to the preſs, I found ſome of thoſe I reckoned upon, to be very unſeaſonably wanting. And to make any great change in the order of the reſt was more than the printer's importunity, and that of my own avocations (and perhaps alſo conſiderable ſollicitations) would permit. But though ſome few preambles of the particular experiments might have (perchance) been ſpared, or ſhortened, if I had had all my papers under my view at once; yet in the moſt of thoſe introductory paſſages, the reader will (I hope) find hints, or advertiſements, as well as tranſitions. If I ſometimes ſeem to inſiſt long upon the circumſtances of a trial, I hope I ſhall be eaſily excuſed by thoſe, that both know how nice divers experiments of colours are, and conſider, that I was not barely to relate them, but ſo as to teach a young gentleman to make them. And if I was not ſollicitous to make a nicer diviſion of the whole treatiſe, than into three parts, whereof the one contains ſome conſiderations about colours in general; the other exhibits a ſpecimen of an account of particular colours, exemplified in whiteneſs and blackneſs; and the third, promiſcuous experiments about the remaining colours (eſpecially red) in order to a theory of them: if, I ſay, I contented myſelf with this eaſy diviſion of my diſcourſe, it was perhaps, becauſe I did not think it ſo neceſſary to be curious about the method or contrivance of a treatiſe, wherein I do not pretend to preſent my reader with a compleat fabric, or ſo much as model; but only to bring in materials proper for the building. And if I did not well know, how ingenious the curioſity and civility of friends makes them, to perſuade men by ſpecious allegations, to gratify their deſires; I ſhould have been made to believe by perſons very well qualified to judge of matters of this nature, that the following experiments will not need the addition of accurate method and ſpeculative notions to procure acceptance for the treatiſe that contains them. For it hath been repreſented, that in moſt of them, as the novelty will make them ſurprizing, and the quickneſs of performance keep them from being tedious; ſo the ſenſible changes, that are effected by them, are ſo manifeſt, ſo great, and ſo ſudden, that ſcarce any will be diſpleaſed to ſee them, and thoſe that are any thing curious will ſcarce be able to ſee them, without finding themſelves excited to make reflections upon them. But though with me, who love to meaſure phyſical things by their uſe, not their ſtrangeneſs, or prettineſs, the partiality of others prevails not to make me over-value theſe, or look upon them in themſelves as other than trifles; yet I confeſs, that ever ſince I did divers years ago ſhew ſome of them to a learned company of *Virtuoſi*, ſo many perſons of differing conditions, and even ſexes, have been curious to ſee them, and pleaſed not to diſlike them, that I cannot deſpair, but that by complying with thoſe that urge the publication of them, I may both gratify and excite the curious, and lay perhaps a foundation, whereon either others or myſelf may in time ſuperſtruct a ſubſtantial theory of colours. And if *Ariſtotle*, after his maſter *Plato*, have rightly obſerved admiration to be the parent of philoſophy, the wonder, ſome of theſe trifles have been wont to produce in all ſorts of beholders, and the acceſs they have ſometimes gained even to the cloſets of ladies, ſeem to promiſe, that ſince the ſubject is ſo pleaſing, that the ſpeculation appears as delightful as difficult, ſuch eaſy and recreative experiments, which require but little time, or charge, or trouble in the making, and when made are ſenſible and ſurprizing enough, may contribute more than others (far more important, but as much more difficult) to recommend thoſe parts of learning (chymiſtry and corpuſcular philoſophy) by which they have been produced, and to which they give teſtimony even to ſuch kind of perſons, as value a pretty trick more than a true notion, and would ſcarce admit philoſophy, if it approached them in another dreſs. Without the ſtrangeneſs or endearments of pleaſantneſs to recommend it, I know, that I do but ill conſult my own advantage in the conſenting to the publication of the following treatiſe: for thoſe things, which, whilſt men knew not how they were performed, appeared ſo ſtrange, will, when the way of making them, and the grounds on which I deviſed them, ſhall be public, quickly loſe all, that their being rarities, and their being thought myſteries, contributed to recommend them. But it is fitter for mountebanks than naturaliſts to deſire to have their diſcoveries rather admired than underſtood; and for my part I had much rather deſerve the thanks of the ingenious, than enjoy the applauſe of the ignorant. And if I can ſo far contribute to the diſcovery of the nature of colours, as to help the curious to it, I ſhall have reached my end, and ſaved myſelf ſome labour, which elſe I may chance to be tempted to undergo in proſecuting that ſubject, and adding to this treatiſe, which I therefore call a hiſtory, becauſe it chiefly contains matters of fact, and which hiſtory the title declares me to look upon but as begun. Becauſe though that above a hundred, not to ſay a hundred and fifty experiments (ſome looſe, and others interwoven amongſt the diſcourſes themſelves) may ſuffice to give a beginning to a hiſtory not hitherto, that I know, begun by any; yet the ſubject is ſo fruitful, and ſo worthy, that thoſe, who are curious of theſe matters, will be far more wanting to themſelves than I can ſuſpect, if what I now publiſh prove any more than a beginning. For, as I hope my endeavours may afford them ſome aſſiſtance towards this work, ſo thoſe endeavours are too much unfiniſhed to give them any diſcouragement, as if there were little left for others to do towards the hiſtory of colours.

For (firſt) I have been willing to leave unmentioned the moſt part of thoſe phænomena of colours, that nature preſents us of her own accord (that is, without being guided or over-ruled by man;) ſuch as the different colours, that ſeveral ſorts of fruits paſs through before they are perfectly ripe, and thoſe that appear upon the fading of flowers and leaves, and the putrefaction (and its ſeveral degrees) of fruits, &c. together with a thouſand other obvious inſtances of the changes of colours. Nor have I much meddled with thoſe familiar phænomena, wherein man is not an idle ſpectator; ſuch as the greenneſs produced by ſalt in beef much powdered, and the redneſs produced in the ſhells of lobſters upon the boiling of thoſe fiſhes: for I was willing to leave the gathering of obſervations to thoſe, that have not the opportunity to make experiments. And for the ſame reaſons, among others, I did purpoſely omit the lucriferous practice of tradeſmen about colours; as the ways of making pigments, of blanching wax, of dying ſcarlet, &c. though to divers of them I be not a ſtranger, and of ſome I have myſelf made trial.

Next; I did purpoſely paſs by divers experiments of other writers that I had made trial of (and that not without regiſtring ſome of their events) unleſs I could ſome way or other improve them; becauſe I wanted leiſure to inſert them, and had thoughts of proſecuting the work once begun, of laying together thoſe I had examined by themſelves, in caſe of my not being prevented by others diligence. So that there remains not a little, among the things that are already publiſhed, to employ thoſe, that have a mind to exerciſe themſelves in repeating and examining them. And I will not undertake, that none of the things delivered, even in this treatiſe, though never ſo faithfully ſet down, may not prove to be thus far of this ſort, as to afford the curious ſomewhat to add about them. For I remember, that I have ſomewhere in the book itſelf acknowledged, that having written it by ſnatches, partly in the country and partly at unſeaſonable times of the year, when the want of fit inſtruments, and of a competent variety of flowers, ſalts, pigments, and other materials, made me leave ſome of the following experiments (eſpecially thoſe about emphatical colours) far more unfiniſhed than they ſhould have been, if it had been as eaſy for me to ſupply what was wanting to compleat them, as to diſcern. Thirdly, to avoid diſcouraging the young gentleman I call *Pyrophilus*, whom the leſs familiar and more laborious operations of chymiſtry would probably have frighted, I purpoſely declined, in what I writ to him, the ſetting down any number of ſuch chymical experiments, as by being very elaborate or tedious, would either require much ſkill, or exerciſe his patience. And yet that this ſort of experiments is exceedingly numerous, and might more than a little enrich the hiſtory of colours, thoſe that are verſed in chymical proceſſes will, I preſume, eaſily allow me.

And (laſtly) for as much as I have occaſion more than once in my ſeveral writings to treat either purpoſely or incidentally of matters relating to colours, I did not, perhaps, conceive myſelf obliged to deliver in one treatiſe all that I would ſay concerning that ſubject.

But to conclude, by ſumming up what I would ſay concerning what I have, and what I have not done, in the following papers; I ſhall not (on the one ſide) deny, that conſidering, that I pretended not to write an accurate treatiſe of colours, but an occaſional eſſay to acquaint a private friend with what then occurred to me of the things I had thought or tried concerning them; I might preſume I did enough for once, if I did clearly and faithfully ſet down, though not all the experiments I could, yet at leaſt ſuch a variety of them, that an attentive reader, that ſhall conſider the grounds on which they have been made, and the hints that are purpoſely (though diſperſedly) couched in them, may eaſily compound them, and otherwiſe vary them, ſo as very much to increaſe their number. And yet (on the other ſide) I am ſo ſenſible both of how much I have, either out of neceſſity or choice, left undone, and of the fruitfulneſs of the ſubject I have begun to handle; that though I had performed far more than it is like many readers will judge I have, I ſhould yet be very free to let them apply to my attempts that of *Seneca*, where having ſpoken of the ſtudy of nature's myſteries, and particularly of the cauſe of earthquakes, he ſubjoins; *Nulla res conſummata eſt dum incipit. Nec in hac tantum re omnium maxima ac involutiſſimâ, in quâ etiam cum multum actum erit, omnis ætas, quod agat, inveniet; ſed in omni alio negotio, longè ſemper à perfecto fuere principia.*

L. Annæi Senecæ Natur. Quæſt. l. 6. c. 5.

The Publiſher to the Reader.

Friendly Reader,

HERE is preſented to thy view one of the abſtruſeſt as well as the genteeleſt ſubjects of natural philoſophy, the *Experimental Hiſtory of Colours*; which, though the noble author be pleaſed to think but begun, yet I muſt take leave to ſay, that I think it ſo well begun, that the work is more than half diſpatched. Concerning which I cannot but give this advertiſement to the reader, that I have heard the author expreſs himſelf, that it would not ſurprize him, if it ſhould happen to be objected, that ſome of theſe experiments have been already publiſhed, partly by chymiſts, and partly by two or three very freſh writers upon other ſubjects. And though the number of theſe experiments be but very ſmall, and though they be none of the conſiderableſt, yet it may on this occaſion be further repreſented, that it is eaſy for our author to name ſeveral men (of whoſe number I can truly name myſelf) who remember either their having ſeen him make, or their having read his accounts of the experiments delivered in the following tract ſeveral years ſince, and long before the publication of the books, wherein they are mentioned. Nay, in divers paſſages (where he could do it without any great inconvenience) he hath ſtruck out experiments, which he had tried many years ago, becauſe he ſince found them divulged by perſons, from whom he had not the leaſt hint of them. Which yet is not touched, with deſign to reflect upon any ingenious man, as if he were a plagiary: for, though our generous author were not reſerved enough in ſhewing his experiments to thoſe that expreſſed a curioſity to ſee them (amongſt whom a very learned man hath been pleaſed publickly to acknowledge it ſeveral years *;) yet the ſame thing may be well enough lighted on by perſons, that know nothing of one another. And eſpecially chymical laboratories may many times afford the ſame phænomenon, about colours, to ſeveral perſons, at the ſame or differing times. And as for the few phænomena mentioned in the ſame chymical writers, as well as in the following treatiſe, our author hath given an account, why he did not decline rejecting them in the annotations upon the 47th experiment of the third part. Not here to mention, what he elſewhere ſaith, to ſhew what uſe may be juſtifiably made of experiments not of his own deviſing by a writer of natural hiſtory, if, what he employs of other mens, be well examined or verified by himſelf.

In the mean time, this treatiſe is ſuch, that there needs no other invitation to peruſe it, but that it is compoſed by one of the deepeſt and moſt indefatigable ſearchers of nature, which, I think the world, as far as I know it, affords. For mine own part, I feel a ſecret joy within me, to ſee ſuch beginnings upon ſuch themes, it being demonſtratively true, *mota facilius moveri*; which cauſeth me to entertain ſtrong hopes, that this illuſtrious virtuoſo and reſtleſs inquirer into nature's ſecrets will not ſtop here, but go on and proſper in the diſquiſition of the other principal colours, green, red, and yellow. The reaſoning faculty ſet once afloat will be carried on, and that with eaſe; eſpecially, when the productions thereof meet, as they do here, with ſo greedy an entertainment at home and abroad. I am confident, that the ROYAL SOCIETY, lately conſtituted by his moſt Excellent Majeſty *for improving Natural Knowledge* will judge it their intereſt to exhort our author to the proſecution of this argument; conſidering, how much it is their deſign and buſineſs to accumulate a good ſtock of ſuch accurate obſervations and experiments, as may afford them and their offspring genuine matter to raiſe a maſculine philoſophy upon, whereby the mind of man may be ennobled with the knowledge of ſolid truths, and the life of man benefited with ampler accommodations, than it hath been hitherto.

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* He that deſires more inſtances of this kind and matter, that according to this doctrine may much help the theory of colours, and particularly the force both of ſulphureous and volatile, as likewiſe of alkalizate and acid ſalts, and in what particulars colours likely depend not in their cauſation from any ſalt at all; may beg his information from Mr. *Boyle*, who hath ſome while ſince honoured me with the ſight of his papers concerning this ſubject, containing many excellent experiments, made by him for the elucidation of this doctrine, &c. Dr. *R. Sharrock*, in his ingenious and uſeful *Hiſtory of the Propagation and Improvement of Vegetables*, publiſhed in the year 1660.

OUR great author, one of the pillars of that illuſtrious corporation, is conſtantly furniſhing large ſymbolas to this work; and is now fallen, as you ſee, upon ſo comprehenſive and important a theme, as will, if inſiſted on and compleated, prove one of the conſiderableſt pieces of that ſtructure. To which if he ſhall pleaſe to add his treatiſe of heat and flame, as he is ready to publiſh his experimental accounts of cold; I eſteem, the world will be obliged to him for having ſhewed them both the right and left-hand of nature, and the operations thereof.

THE conſidering reader will by this very treatiſe ſee abundant cauſe to ſollicit the author for more. Sure I am, that whatever of the productions of his ingeny comes into foreign parts (where I am happy in the acquaintance of many intelligent friends) is highly valued; and to my knowledge, there are thoſe among the French, that have lately begun to learn Engliſh, on purpoſe to enable themſelves to read his books, being impatient of their traduction into Latin. If I durſt ſay all I know of the elogies received by me from abroad concerning him, I ſhould perhaps make this preamble too prolix, and certainly offend the modeſty of our author.

WHEREFORE I ſhall leave this, and conclude with deſiring the reader, that if he meet with other faults beſides thoſe that the Errata take notice of (as I believe he may) he will pleaſe to conſider both the weakneſs of the author's eyes for not reviewing, and the manifold avocations of the publiſher for not doing his part; who taketh his leave with inviting thoſe, that have alſo conſidered this nice ſubject experimentally, to follow the example of our noble author, and impart ſuch and the like performances to the now very inquiſitive world. Farewel.

I *H. O.*

THE EXPERIMENTAL HISTORY OF COLOURS BEGUN.

THE FIRST PART.

CHAP. I.

1. I HAVE ſeen you ſo paſſionately addicted, *Pyrophilus*, to the delightful art of limning and painting, that I cannot but think myſelf obliged to acquaint you with ſome of thoſe things that have occurred to me concerning the changes of colours. And I may expect that I ſhall as well ſerve the Virtuoſi in general, as gratify you in particular, by furniſhing a perſon, who, I hope, will both improve my communications, and communicate his improvements, with ſuch experiments and obſervations as may both invite you to enquire ſeriouſly into the nature of colours, and aſſiſt you in the inveſtigation of it. This being the principal ſcope of the following tract, I ſhould do that which might prevent my own deſign, if I ſhould here attempt to deliver you an accurate and particular theory of colours; for that were to preſent you with what I deſire to receive from you; and, as far as in me lay, to make that ſtudy needleſs, to which I would engage you.

2. WHEREFORE my preſent work ſhall be but to divert and recreate, as well as excite you by the delivery of matters of fact, ſuch as you may for the moſt part try with much eaſe, and poſſibly not without ſome delight. And leſt you ſhould expect any thing of elaborate or methodical in what you will meet with here, I muſt confeſs to you before-hand, that the ſeaſons I was wont to chuſe to deviſe and try experiments about colours, were thoſe days, wherein having taken phyſic, and finding myſelf as unfit to ſpeculate, as unwilling to be altogether idle, I choſe this diverſion as a kind of mean betwixt the one and the other. And I have the leſs ſcrupled to ſet down the following experiments, as ſome of them came to my mind, and as the notes wherein I had ſet down the reſt, occurred to my hands; that by declining a methodical way of delivering them, I might leave you and myſelf the greater liberty and convenience to add to them, and tranſpoſe them as ſhall appear expedient.

3. YEA, that you may not think me too reſerved, or look upon an inquiry made up of mere narratives, as ſomewhat jejune, I am content to premiſe a few conſide-

rations, that now offer themſelves to my thoughts, which relate in a more general way, either to the nature of colours, or to the ſtudy of it. And I ſhall inſert an eſſay, as well ſpeculative as hiſtorical, of the nature of whiteneſs and blackneſs, that you may have a ſpecimen of the hiſtory of colours, I have ſometimes had thoughts of; and if you diſlike not the method I have made uſe of, I hope you, and ſome of the Virtuoſi your friends, may be thereby invited to go thorough with red, blue, yellow, and the reſt of the particular colours, as I have done with white and black, but with far more ſagacity and ſucceſs. And if I can invite ingenious men to undertake ſuch taſks, I doubt not but the curious will quickly obtain a better account of colours, than as yet we have, ſince in our method the theorical part of the inquiry being attended, and as it were interwoven with the hiſtorical conjectures, the philoſophy of colours will be promoted by the indiſputable experiments.

CHAP. II.

1. TO come then in the firſt place to our more general conſiderations, I ſhall begin with ſaying ſomething as to the importance of examining the colours of bodies. For there are ſome, eſpecially chymiſts, who think that a conſiderable diverſity of colours does conſtantly argue an equal diverſity of nature, in the bodies wherein it is conſpicuous; but I confeſs I am not altogether of their mind: for not to mention changeable taffaties, the blue and golden necks of pigeons, and divers water-fowl, rainbows natural and artificial, and other bodies, whoſe colours the philoſophers have been pleaſed to call not real, but apparent and fantaſtical; not to inſiſt on theſe, I ſay (for fear of needleſsly engaging in a controverſy) we ſee in parrots, goldfinches, and divers other birds, not only that the contiguous feathers which are probably as near in properties as place, are ſome of them red, and others white, ſome of them blue, and others yellow, *&c.* but that in the ſeveral parts of the ſelf-ſame feather there may often be ſeen the greateſt diſparity of colours. And ſo in the leaves of tulips, july-flowers, and ſome other vegetables, the ſeveral leaves, and even the ſeveral parts of the ſame leaf, although no difference have been obſerved in their other properties, are frequently found painted with very different colours. And ſuch a variety we have much more admired in that lovely plant which is commonly, and not unjuſtly called the *Marvel of Peru*; for of divers ſcores of fine flowers, which in its ſeaſon that gaudy plant does almoſt daily produce, I have ſcarce taken notice of any two that were dyed perfectly alike. But though, *Pyro*, ſuch things as theſe, among others, keep me from daring to affirm that the diverſity and change of colours does always argue any great difference or alteration betwixt, or in the bodies, wherein it is to be diſcerned; yet that oftentimes the alteration of colours does ſignify conſiderable alterations in the diſpoſition of parts of bodies, may appear in the extraction of tinctures, and divers other chymical operations, wherein the change of colours is the chief, and ſometimes the only thing, by which the artiſt regulates his proceeding; and is taught to know when 'tis ſeaſonable for him to leave off. Inſtances of this ſort are more obvious in divers ſorts of fruits, as cherries, plums, *&c.* wherein, according as the vegetable ſap is ſweetned, or otherwiſe ripened, by paſſing from one degree to another of maturation, the external part of the fruit paſſes likewiſe from one to another colour. But one of the nobleſt inſtances I have met with of this kind, is not ſo obvious; and that is the way of tempering ſteel to make gravers, drills, ſprings, and other mechanical inſtruments, which we have divers times both made artificers practiſe in our preſence, and tried ourſelves after the

following manner. Firſt, the ſlender ſteel to be tempered is to be hardened by heating as much of it as is requiſite among glowing coals, till it be glowing hot, but it muſt not be quenched as ſoon as it is taken from the fire (for that would make it too brittle, and ſpoil it) but muſt be held over a baſon of water, till it deſcend from a white heat to a red one, which as ſoon as ever you perceive, you muſt immediately quench as much as you deſire to harden in the cold water. The ſteel thus hardened will, if it be good, look ſomewhat white, and muſt be made bright at the end, that its change of colours may be there conſpicuous; and then holding it ſo in the flame of a candle, that the bright end may be, for about half an inch or more, out of the flame, that the ſmoak do not ſtain or ſully the brightneſs of it, you ſhall after a while ſee that clean end, which is almoſt contiguous to the flame, paſs very nimbly from one colour to another, as from a brighter yellow, to a deeper and reddiſh yellow, which artificers call a ſanguine; and from that to a fainter firſt, and then a deeper blue. And to bring home this experiment to our preſent purpoſe, it is found by daily experience, that each of theſe ſucceeding colours argue ſuch a change made in the texture of the ſteel, that if it be taken from the flame, and immediately quenched in the tallow (whereby it is ſettled in whatever temper it had before) when it is yellow, it is of ſuch a hardneſs as makes it fit for gravers, drills, and ſuch like tools; but if it be kept a few minutes longer in the flame till it grow blue, it becomes much ſofter, and unfit to make gravers for metals, but fit to make ſprings for watches, and ſuch like inſtruments, which are therefore commonly of that colour: and if the ſteel be kept in the flame, after this deep blue hath diſcloſed itſelf, it will grow ſo ſoft, as to need to be new hardened again, before it can be brought to a temper fit for drills or penknives. And I confeſs, *Pyro*, I have taken much pleaſure to ſee the colours run along from the parts of the ſteel contiguous to the flame, to the end of the inſtrument, and ſucceed one another ſo faſt, that if a man be not vigilant, to thruſt the ſteel into the tallow at the very nick of time, at which it has attained its due colour, he ſhall miſs of giving his tool the right temper. But becauſe the flame of a candle is offenſive to my weak eyes, and becauſe it is apt to either black or ſully the contiguous part of the ſteel which is held in it, and thereby hinder the change of colours from being ſo long and clearly diſcerned, I have ſometimes made this experiment by laying the ſteel to be tempered upon a heated bar of iron, which we find alſo to be employed by ſome artificers in the tempering of ſuch great inſtruments, as are too big to be ſoon heated ſufficiently by the flame of a candle. And you may eaſily ſatisfy yourſelf, *Pyro*, of the differing hardneſs and toughneſs, which is aſcribed to ſteel tempered at different colours, if you break but ſome ſlender wires of ſteel ſo tempered, and obſerve how they differ in brittleneſs, and if with a file you alſo make trial of their various degrees of hardneſs.

2. BUT, *Pyrophilus*, I muſt not at preſent any further proſecute the conſideration of the importance of experiments about colours, not only becauſe you will in the following papers find ſome inſtances, that would here be preſented you out of their due place, of the uſe that may be made of ſuch experiments, in diſcovering in divers bodies what kind the ſalt is, that is predominant in them; but alſo becauſe a ſpeculative Naturaliſt might juſtly enough allege, that as light is ſo pleaſing an object, as to be well worth our looking on, though it diſcovered to us nothing but itſelf; ſo modified light, called colour, were worth our contemplation, though by underſtanding its nature we ſhould be taught nothing elſe. And however, I need not make either you or myſelf excuſes for entertaining you on the ſubject I am now about to treat of; ſince the pleaſure *Pyro* takes in mixing and laying on of colours, will I preſume keep

him, and will (I am ſure) keep me from thinking it troubleſome to ſet down, eſpecially after the tedious proceſſes (about other matters) wherewith I fear I may have tired him, ſome eaſy, and not unpleaſant experiments relating to that ſubject.

3. But, before we deſcend to the more particular conſiderations we are to preſent you concerning colours, I preſume it will be ſeaſonable to propoſe at the very entrance a diſtinction; the ignorance or neglect of which, ſeeems to me to have frequently enough occaſioned either miſtakes or confuſion in the writings of divers modern philoſophers. For colour may be conſidered, either as it is a quality reſiding in the body that is ſaid to be coloured, or to modify the light after ſuch or ſuch a manner; or elſe as the light itſelf, which ſo modified, ſtrikes upon the organ of ſight, and ſo cauſes that ſenſation which we call colour: and that this latter may be looked upon as the more proper, though not the uſual acceptation of the word colour, will be made probable by divers paſſages in the enſuing part of our diſcourſe. And indeed it is the light itſelf, which after a certain manner, either mingled with ſhades or ſome other ways troubled, ſtrikes our eyes, that does more immediately produce that motion in the organ, upon whoſe account men ſay they ſee ſuch or ſuch a colour in the object: yet, becauſe there is in the body that is ſaid to be coloured, a certain diſpoſition of the ſuperficial particles, whereby it ſends the light reflected, or refracted, to our eyes thus and thus altered, and not otherwiſe, it may alſo in ſome ſenſe be ſaid, that colour depends upon the viſible body; and therefore we ſhall not be againſt that way of ſpeaking of colours, that is moſt uſed among the modern Naturaliſts, provided we be allowed to have recourſe, when occaſion ſhall require, to the premiſed diſtinction, and to take the more immediate cauſe of colour to be the modified light itſelf, as it affects the ſenſory; though the diſpoſition alſo of the coloured body, as that modifies the light, may be called by that name metonymically (to borrow a ſchool-term) or efficiently, that is, in regard of its turning the light, that rebounds from it, or paſſes through it, into this or that particular colour.

4. I know not whether I may not on this occaſion add, that colour is ſo far from being an inherent quality of the object in the ſenſe that is wont to be declared by the ſchools, or even in the ſenſe of ſome modern Atomiſts, that, if we conſider the matter more attentively, we ſhall ſee cauſe to ſuſpect, if not to conclude, that though light do more immediately affect the organ of ſight, than do the bodies that ſend it thither, yet light itſelf produces the ſenſation of a colour, but as it produces ſuch a determinate kind of local motion in ſome part of the brain; which, though it happen moſt commonly from the motion whereinto the ſlender ſtring of the retina are put, by the appulſe of light; yet if the like motion happen to be produced by any other cauſe, wherein the light concurs not at all, a man ſhall think he ſees the ſame colour. For proof of this, I might put you in mind, that it is uſual for dreaming men to think they ſee the images that appear to them in their ſleep, adorned ſome with this, and ſome with that lively colour, whilſt yet, both the curtains of their bed, and thoſe of their eyes, are cloſe drawn. And I might add the confidence with which diſtracted perſons do oftentimes, when they are awake, think they ſee black fiends in places, where there is no black object in ſight without them. But I will rather obſerve, that not only when a man receives a great ſtroke upon his eye, or a very great one upon ſome other part of his head, he is wont to ſee, as it were, flaſhes of lightning, and little vivid, but vaniſhing flames, though perhaps his eyes be ſhut: but the like apparitions may happen, when the motion proceeds not from ſomething without, but from ſomething within the body, provided the unwonted fumes that wander up and down in the head, or the propagated concuſſion of any internal part in the body, do

cauſe, about the inward extremities of the optic nerve, ſuch a motion as is wont to be there produced, when the ſtroke of the light upon the retina makes us conclude, that we ſee either light or ſuch and ſuch a colour. This the moſt ingenious *Des Cartes* hath very well obſerved; but becauſe he ſeems not to have exemplified it by any unobvious or peculiar obſervation, I ſhall endeavour to illuſtrate this doctrine by a few inſtances.

5. AND firſt, I remember, that having, through God's goodneſs, been free for ſeveral years from troubleſome coughs, being afterwards, by an accident, ſuddenly caſt into a violent one, I did often when I was awaked in the night by my diſtempers, obſerve, that upon coughing ſtrongly, it would ſeem to me, that I ſaw very vivid, but immediately diſappearing flames; which I took particular notice of, becauſe of the conjecture I am now mentioning.

6. AN excellent and very diſcreet perſon, very near allied both to you and me, was relating to me, that ſome time ſince, whilſt ſhe was talking with ſome other ladies, upon a ſudden, all the objects ſhe looked upon appeared to her dyed with unuſual colours, ſome of one kind, and ſome of another, but all ſo bright and vivid, that ſhe ſhould have been as much delighted, as ſurprized with them; but that finding the apparition to continue, ſhe feared it portended ſome very great alteration as to her health: and indeed, the day after ſhe was aſſaulted with ſuch violence by hyſterical and hypocondrical diſtempers, as both made her rave for ſome days, and gave her, during that time, a baſtard palſy.

7. BEING a while ſince in a town, where the plague had made great havock, and inquiring of an ingenious man, that was ſo bold, as without much ſcruple to viſit thoſe that were ſick of it, about the odd ſymptoms of a diſeaſe that had ſwept away ſo many there; he told me, among other things, that he was able to tell divers patients, to whom he was called, before they took their beds, or had any evident ſymptoms of the plague, that they were indeed infected, upon peculiar obſervations, that being aſked, they would tell him that the neighbouring objects, and particularly his clothes, appeared to them beautified with moſt glorious colours, like thoſe of the rainbow, oftentimes ſucceeding one another: and this he affirmed to be one of the moſt uſual, as well as the moſt early ſymptoms, by which this odd peſtilence diſcloſed itſelf. And when I aſked how long the patients were wont to be thus affected, he anſwered, that it was moſt commonly for about a day; and when I further inquired whether or no vomits, which in that peſtilence were uſually given, did not remove this ſymptom (for ſome uſed the taking of a vomit, when they came aſhore, to cure themſelves of the obſtinate and troubleſome giddineſs cauſed by the motion of the ſhip) he replied, that generally, upon the evacuation made by the vomit, that ſtrange apparition of colours ceaſed, though the other ſymptoms were not ſo ſoon abated; yet he added (to take notice of that upon the by, becauſe the obſervation may perchance do good) that an excellent phyſician, in whoſe company he was wont to viſit the ſick, did give to almoſt all thoſe to whom he was called, in the beginning, before nature was much weakened, a pretty odd vomit, conſiſting of eight or ten drachms of infuſion of *Crocus Metallorum*, and about half a drachm, or much more, of white vitriol, with ſuch ſucceſs, that ſcarce one of ten to whom it was ſeaſonably adminiſtred, miſcarried.

8. BUT to return to the conſideration of colours: as an apparition of them may be produced by motions from within, without the aſſiſtance of an outward object; ſo I have obſerved, that it is ſometimes poſſible that the colour that would otherwiſe be produced by an outward object, may be changed by ſome motion, or new texture

already produced in the ſenſory, as long as that unuſual motion, or new diſpoſition laſts; for I have divers times tried, that after I have through a teleſcope looked upon the ſun, though thorough a thick, red, or blue glaſs, to make its ſplendor ſupportable to the eye, the impreſſion upon the retina would be not only ſo vivid, but ſo permanent, that if afterwards I turned my eye towards a flame, it would appear to me of a colour very differing from its uſual one. And if I did divers times ſucceſſively ſhut and open the ſame eye, I ſhould ſee the adventitious colour (if I may ſo call it) changed or impaired by degrees, till at length (for this unuſual motion of the eye would not preſently ceaſe) the flame would appear to me of the ſame hue that it did to other beholders. A not unlike effect I found by looking upon the moon, when ſhe was near full, thorough an excellent teleſcope, without coloured glaſs to ſcreen my eye with: but that which I deſire may be taken notice of, becauſe we may elſewhere have occaſion to reflect upon it, and becauſe it ſeems not agreeable to what Anatomiſts and optical writers deliver, touching the relation of the two eyes to each other, is this circumſtance; that though my right eye, with which I looked thorough the teleſcope, were thus affected by the over-ſtrong impreſſion of the light, yet when the flame of a candle, or ſome other bright object appeared to me of a very unuſual colour, whilſt looked upon with the diſcompoſed eye, or (though not ſo notably) with both eyes at once; yet if I ſhut that eye, and looked upon the ſame object with the other, it would appear with no other than its uſual colour, though if I again opened, and made uſe of the dazzled eye, the vivid adventitious colour would again appear. And on this occaſion I muſt not pretermit an obſervation which may perſuade us, that an over-vehement ſtroke upon the ſenſory, eſpecially if it be naturally of a weak conſtitution, may make a more laſting impreſſion than one would imagine; which impreſſion may in ſome caſes, as it were, mingle with, and vitiate the action of vivid objects for a long time after.

For I know a lady of unqueſtionable veracity, who having lately, by a deſperate fall, received ſeveral hurts, and particularly a conſiderable one upon a part of her face near her eye, had her ſight ſo troubled and diſordered, that, as ſhe hath more than once related to me, not only when the next morning one of her ſervants came to her bed-ſide, to aſk how ſhe did, his clothes appeared adorned with ſuch variety of dazzling colours, that ſhe was fain preſently to command him to withdraw; but the images in her hangings did, for many days after, appear to her, if the room were not extraordinarily darkened, embelliſhed with ſeveral offenſively vivid colours, which no body elſe could ſee in them. And when I enquired whether or no white objects did not appear to her adorned with more luminous colours than others, and whether ſhe ſaw not ſome which ſhe could not well deſcribe to any, whoſe eyes had never been diſtempered, ſhe anſwered me, that ſometimes ſhe thought ſhe ſaw colours ſo new and glorious, that they were of a peculiar kind, and ſuch as ſhe could not deſcribe by their likeneſs to any ſhe had beheld either before or ſince; and that white objects did ſo much diſorder her ſight, that if, ſeveral days after her fall, ſhe looked upon the inſide of a book, ſhe fancied ſhe ſaw there colours like thoſe of the rainbow: and even when ſhe thought herſelf pretty well recovered, and made bold to leave her chamber, the coming into a place where the walls and ceiling were whited over, made thoſe objects appear to her cloathed with ſuch glorious and dazzling colours, as much offended her ſight, and made her repent her venturouſneſs. And ſhe added, that this diſtemper of her eyes laſted not leſs than five or ſix weeks, though ſince that, ſhe hath been able to read and write much without finding the leaſt inconvenience in doing ſo. I would gladly have known, whether if ſhe had ſhut the injured eye, the phænomena would have been the ſame, when ſhe employed only the other; but I heard not of this accident early enough to ſatisfy that inquiry.

9. WHEREFORE, I ſhall now add, that ſome years before, a perſon exceedingly eminent for his profound ſkill in almoſt all kinds of philological learning, coming to adviſe with me about a diſtemper in his eyes, told me, among other circumſtances of it, that having upon a time looked too fixedly upon the ſun, thorough a teleſcope, without any coloured glaſs, to take off from the dazzling ſplendor of the object, the exceſs of light did ſo ſtrongly affect his eye, that ever ſince, when he turns it towards a window, or any white object, he fancies he ſeeth a globe of light, of about the bigneſs the ſun then appeared of to him, to paſs before his eyes: and having inquired of him, how long he had been troubled with this indiſpoſition, he replied, that it was already nine or ten years ſince the accident, that occaſioned it, firſt befel him.

10. I COULD here ſubjoin, *Pyrophilus*, ſome memorable relations that I have met with in the account given us by the experienced *Epiphanius Ferdinandus*, of the ſymptoms he obſerved to be incident to thoſe that are bitten with the Tarantula; by which (relations) I could probably ſhew, that without any change in the object, a change in the inſtruments of viſion may for a great while make ſome colours appear charming, and make others provoking, and both to a high degree, though neither of them produced any ſuch effects before. Theſe things, I ſay, I could here ſubjoin in confirmation of what I have been ſaying, to ſhew that the diſpoſition of the organ is of great importance in the dijudications we make of colours, were it not that theſe ſtrange ſtories belonging more properly to another diſcourſe, I had rather (contenting myſelf to have given you an intimation of them here) that you ſhould meet with them fully delivered there.

CHAP. III.

1. BUT, *Pyrophilus*, I would not, by all that I have hitherto diſcourſed, be thought to have forgotten the diſtinction (of colour) that I mentioned to you about the beginning of the third ſection of the former chapter; and therefore, after all I have ſaid of colour, as it is modified light, and immmediately affects the ſenſory, I ſhall now remind you, that I did not deny, but that colour might in ſome ſenſe be conſidered as a quality reſiding in the body that is ſaid to be coloured; and indeed the greateſt part of the following experiments refer to colour principally under that notion, for there is in the bodies we call coloured, and chiefly in their ſuperficial parts, a certain diſpoſition, whereby they do ſo trouble the light that comes from them to our eye, as that it there makes that diſtinct impreſſion, upon whoſe account we ſay, that the ſeen body is either white or black, or red or yellow, or of any one determinate colour. But becauſe we ſhall (God permitting) by the experiments that are to follow ſome pages hence, more fully and particularly ſhew, that the changes, and conſequently in divers places the production and the appearance of colours, depends upon the continuing or altered texture of the object; we ſhall in this place intimate (and that too but as by the way) two or three things about this matter.

2. AND firſt, it is not without ſome reaſon, that I aſcribe colour (in the ſenſe formerly explained) chiefly to the ſuperficial parts of bodies; for not to queſtion how much opacous corpuſcles may abound even in thoſe bodies we call diaphanous, it ſeems plain that of opacous bodies we do indeed ſee little elſe than the ſuperficies. For if we found the beams of light that rebound from the object to the eye, to pierce deep into the coloured body, we ſhould not judge it opacous, but either tranſlucid,

or at leaſt ſemi-diaphanous: and though the ſchools ſeem to teach us that colour is a penetrative quality, that reaches to the innermoſt parts of the object, as if a piece of ſealing-wax be broken into never ſo many pieces, the internal fragments will be as red as the external ſurface did appear; yet that is but a particular example, that will not overthrow the reaſon lately offered, eſpecially ſince I can allege other examples of a contrary import, and two or three negative inſtances are ſufficient to overthrow the generality of a poſitive rule, eſpecially if that be built but upon one or a few examples. Not (then) to mention cherries, plums, and I know not how many other bodies, wherein the ſkin is of one colour, and what it hides of another, I ſhall name a couple of inſtances drawn from the colours of durable bodies that are thought far more homogeneous, and have not parts that are either organical, or of a nature approaching thereunto.

3. To give you the firſt inſtance, I ſhall need but to remind you of what I told you a little after the beginning of this eſſay, touching the blue and red and yellow, that may be produced upon a piece of tempered ſteel; for theſe colours, though they be very vivid, yet if you break the ſteel they adorn, they will appear to be but ſuperficial; not only the innermoſt parts of the metal, but thoſe that are within a hair's breadth of the ſuperficies, having not any of theſe colours, but retaining that of the ſteel it ſelf. Beſides that, we may as well confirm this obſervation, as ſome other particulars we elſewhere deliver concerning colours, by the following experiment which we purpoſely made.

4. We took a good quantity of clean lead, and melted it with a ſtrong fire, and then immediately pouring it out into a clean veſſel of a convenient ſhape and matter (we uſed one of iron, that the great and ſudden heat might not injure it) and then carefully and nimbly taking off the ſcum that floated on the top, we perceived, as we expected, the ſmooth and gloſſy ſurface of the melted matter to be adorned with a very glorious colour, which being as tranſitory as delightful, did almoſt immediately give place to another vivid colour, and that was as quickly ſucceeded by a third, and this as it were chaſed away by a fourth; and ſo theſe wonderfully vivid colours ſucceſſively appeared and vaniſhed (yet the ſame now and then appearing the ſecond time) till the metal ceaſing to be hot enough to afford any longer this pleaſing ſpectacle, the colours that chanced to adorn the ſurface, when the lead thus began to cool, remained upon it; but were ſo ſuperficial, that how little ſoever we ſcraped off the ſurface of the lead, we did in ſuch places ſcrape off all the colour, and diſcover only that which is natural to the metal itſelf; which receiving its adventitious colours, only when the heat was very intenſe, and in that part which was expoſed to the comparatively very cold air (which by other experiments ſeems to abound with ſubtile ſaline parts, perhaps not uncapable of working upon lead ſo diſpoſed:) theſe things, I ſay, together with my obſerving that whatever parts of the ſo ſtrongly melted lead were expoſed a while to the air, turned into a kind of ſcum or litharge, how bright and clean ſoever they appeared before, ſuggeſted to me ſome thoughts or ravings, which I have not now time to acquaint you with. One that did not know me, *Pyrophilus*, would perchance think I endeavoured to impoſe upon you by relating this experiment, which I have ſeveral times tried; but the reaſon why the phænomena mentioned have not been taken notice of, may be, that unleſs lead be brought to a much higher degree of fuſion or fluidity than is uſual, or than is indeed requiſite to make it melt, the phænomena I mentioned will ſcarce at all diſcloſe themſelves; and we have alſo obſerved, that this ſucceſſive appearing and vaniſhing of vivid colours was wont to be impaired or determined whilſt the metal expoſed to the air remained

yet hotter than one would readily ſuſpect. And one thing I muſt further note, of which I leave you to ſearch after the reaſon, namely, that the ſame colours did not always and regularly ſucceed one another, as is uſual in ſteel, but in the diverſified order mentioned in this following note, which I was ſcarce able to write down, the ſucceſſion of the colours was ſo very quick: whether that proceeded from the differing degrees of heat in the lead expoſed to the cool air, or from ſome other reaſon, I leave you to examine.

[*Blue, yellow, purple, blue*; *green, purple, blue, yellow, red*; *purple, blue, yellow and blue*; *yellow, blue, purple, green mixt*; *yellow, red, blue, green*; *yellow, red, purple, green.*]

5. THE Atomiſts of old, and ſome learned men of late, have attempted to explicate the variety of colours in opacous bodies from the various figures of their ſuperficial parts; the attempt is ingenious, and the doctrine ſeems partly true: but I confeſs I think there are divers other things that muſt be taken in as concurrent to produce thoſe differing forms of aſperity, whereon the colours of opacous bodies ſeem to depend. To declare this a little, we muſt aſſume, that the ſurfaces of all ſuch bodies, how ſmooth or polite ſoever they may appear to our dull ſight and touch, are exactly ſmooth only in a popular, or at moſt in a phyſical ſenſe, but not in a ſtrict and rigid ſenſe.

6. THIS excellent microſcopes ſhew us in many bodies, that ſeem ſmooth to our naked eyes; and this not only as to the little hillocks or protuberances that ſwell above that which may be conceived to be the plane or level of the conſidered ſurface; for it is obvious enough to thoſe that are any thing converſant with ſuch glaſſes: but as to numerous depreſſions beneath that level, of which ſort of cavities, by the help of a microſcope, which the greateſt artificer that makes them, judges to be the greateſt magnifying glaſs in *Europe*, except one that equals it, we have on the ſurface of a thin piece of cork that appeared ſmooth to the eye, obſerved about ſixty in a row, within the length of leſs than a 31 and 32d part of an inch (for the glaſs takes in no longer a ſpace at one view;) and theſe cavities (which made that little piece of cork almoſt like an empty honey-comb) were not only very diſtinct, and figured like one another, but of a conſiderable bigneſs, and a ſcarce credible depth; inſomuch that their diſtinct ſhadows as well as ſides were plainly diſcerned and eaſy to be reckoned, and might have been well diſtinguiſhed, though they had been ten times leſſer than they were. Which I thought it not amiſs to mention to you, *Pyrophilus*, upon the by, that you may thence make ſome eſtimate, what a ſtrange inequality, and what a multitude of little ſhades there may really be, in a ſcarce ſenſible part of the phyſical ſuperficies, though the naked eye ſees no ſuch matter. And as excellent microſcopes ſhew us this ruggedneſs in many bodies that paſs for ſmooth, ſo there are divers experiments, though we muſt not now ſtay to urge them, which ſeem to perſuade us of the ſame thing, as to the reſt of ſuch bodies as we are now treating of; ſo that there is no ſenſible part of an opacous body, that may not be conceived to be made up of a multitude of ſingly inſenſible corpuſcles. But in the giving theſe ſurfaces that diſpoſition, which makes them alter the light that reflects thence to the eye after the manner requiſite to make the object appear green, blue, &c. the figures of theſe particles have a great, but not the only ſtroke. It is true indeed, that the protuberant particles may be of very great variety of figures, ſpherical, elliptical, polyedrical, and ſome very irregular; and that according to the nature of theſe, and the ſituation of the lucid body, the light muſt be variouſly affected, after one manner from ſurfaces (I now ſpeak of phyſical ſurfaces) conſiſting of ſpherical, and in another

other from thoſe that are made up of conical or cylindrical corpuſcles; ſome being fitted to reflect more of the incident beams of light, others leſs, and ſome towards one part, others towards another. But beſides this difference of ſhape, there may be divers other things that may eminently concur to vary the forms of aſperity that colours ſo much depend on. For, willingly allowing the figure of the particles in the firſt place, I conſider ſecondly, that the ſuperficial corpuſcles, if I may ſo call them, may be bigger in one body, and leſs in another, and conſequently fitted to allay the light falling on them with greater ſhades. Next, the protuberant particles may be ſet more or leſs cloſe together, that is, there may be a greater or a ſmaller number of them within the compaſs of one, than within the compaſs of another ſmall part of the ſurface of the ſame extent; and how much theſe qualities may ſerve to produce colour, may be ſomewhat gueſſed at, by that which happens in the agitation of water: for if the bubbles that are thereby made be great, and but few, the water will ſcarce acquire a ſenſible colour; but if it be reduced to a froth, conſiſting of bubbles, which being very minute and contiguous to each other, are a multitude of them crouded into a narrow room, the water (turned to froth) does then exhibit a very manifeſt white colour, to which theſe laſt named conditions of the bubbles do, as well as their convex figure, contribute; and that for reaſons to be mentioned anon. Beſides, it is not neceſſary that the ſuperficial particles that exhibit one colour ſhould be all of them round, or all conical, or all of any one ſhape; but corpuſcles of differing figures may be mingled on the ſurface of the opacous body, as when the corpuſcles that make a blue colour, and thoſe that make a yellow, come to be accurately and ſkilfully mixed, they make up a green; which, though it ſeem one ſimple colour, yet, in this caſe, appears to be made by corpuſcles of very differing kinds, duly commixed. Moreover, the figure and bigneſs of the little depreſſions, cavities, furrows, or pores intercepted betwixt theſe protuberant corpuſcles, are as well to be conſidered as the ſizes and ſhapes of the corpuſcles themſelves: for we may conceive the phyſical ſuperficies of a body, where (as we ſaid) its colour does, as it were, reſide, to be cut tranſverſely by a mathematical plane, which you know is conceived to be without any depth or thickneſs at all; and then, as ſome parts of the phyſical ſuperficies will be protuberant, or ſwell above this laſt plane, ſo others may be depreſſed beneath it, as (to explain myſelf by a groſs compariſon) in divers places of the ſurface of the earth, there are not only neighbouring hills, trees, &c. that are raiſed above the horizontal level of the valley, but rivers, wells, pits and other cavities that are depreſſed beneath it. And that ſuch protuberant and concave parts of a ſurface may remit the light ſo differingly, as much to vary a colour, ſome examples, and other things that we ſhall hereafter have occaſion to take notice of in this tract, will ſufficiently declare; till when, it may ſuffice to put you in mind, that of two flat ſides of the ſame piece of, for example, red marble, the one being diligently poliſhed, and the other left to its former roughneſs, the differing degrees or ſorts of aſperity, for the ſide that is ſmooth to the touch wants not its roughneſs, will ſo diverſify the light reflected from the ſeveral planes to the eye, that a painter would employ two differing colours to repreſent them.

See the Diſcourſe of the nature of whiteneſs and blackneſs.

7. And I hope, *Pyrophilus*, you will not think it ſtrange or impertinent, that I employ, in divers paſſages of theſe papers, examples drawn from bodies and ſhadows far more groſs than thoſe minute protuberances and ſhady pores on which, in moſt caſes, the colour of a body, as it is an inherent quality or diſpoſition of its ſurface, ſeems to depend. For ſometimes I employ ſuch examples, rather to declare my meaning, than prove my conjecture; things, whom their ſmallneſs makes inſenſible, being

better repreſented to the imagination by ſuch familiar objects, as being like them enough in other reſpects, are of a viſible bulk. And next, though the beams of light are ſuch ſubtile bodies, that in reſpect of them, even ſurfaces that are ſenſibly ſmooth, are not exactly ſo, have their own degree of roughneſs, conſiſting of little protuberances and depreſſions; and though conſequently ſuch inequalities may ſuffice to give bodies differing colours, as we ſee in marble that appears white or black, or red or blue, even when the moſt carefully poliſhed; yet it is plain, by the late inſtance of red marble, and many others, that even bigger protuberances and greater ſhades may likewiſe ſo diverſify the roughneſs of a body's ſuperficies, as manifeſtly to concur to the varying of its colour, whereby ſuch examples appear to be proper enough to be employed in ſuch a ſubject as we have now in hand. And having hinted thus much on this occaſion, I now proceed.

8. The ſituation alſo of the ſuperficial particles is conſiderable, which I diſtinguiſh into the poſture of the ſingle corpuſcles, in reſpect of the light, and of the eye, and the order of them in reference alſo to one another; for a body may otherwiſe reflect the light, when its ſuperficial particles are more erected upon the plane, that may be conceived to paſs along their baſis, and when the points or extremes of ſuch particles are obverted to the eye, than when thoſe particles are ſo inclined, that their ſides are in great part diſcernible; as the colour of pluſh or velvet will appear varied to you, if you carefully ſtroke part of it one way, and part of it another; the poſture of the particular thrids, in reference to the light, or the eye, becoming thereby different. And you may obſerve in a field of ripe corn blown upon by the wind, that there will appear as it were waves of a colour (at leaſt gradually) differing from that of the reſt of the field; the wind, by depreſſing ſome of the ears, and not at the ſame time others, making the one reflect more from the lateral and ſtrawy parts than do the reſt. And ſo, when dogs are ſo angry as to erect the hairs upon their necks, and upon ſome other parts of their bodies, thoſe parts ſeem to acquire a colour varied from that which the ſame hairs made, when in their uſual poſture they did far more ſtoop. And that the order wherein the ſuperficial corpuſcles are ranged, is not to be neglected, we may gueſs by turning of water into froth, the beating of glaſs, and the ſcraping of horns, in which caſes the corpuſcles that were before ſo marſhalled as to be perſpicuous, do by the troubling of that order become diſpoſed to terminate and reflect more light, and thereby to appear whitiſh. And there are other ways in which the order of the protuberant parts, in reference to the eye, may much contribute to the appearing of a particular colour; for I have often obſerved, that when peas are planted, or ſet in parallel lines, and are ſhot up about half a foot above the ſurface of the ground, by looking on the field or plot of ground from that part towards which the parallel lines tended, the greater part of the ground by far, would appear of its own dirty colour; but if I looked upon it tranſverſely, the plot would appear very green, the upper parts of the peas hindering the intercepted parts of the ground, which, as I ſaid, retained their wonted colour from being diſcovered by the eye. And I know not, *Pyrophilus*, whether I might not add, that even the motion of the ſmall parts of a viſible object may in ſome caſes contribute, though it be not ſo eaſy to ſay how, to the producing, or the varying of a colour: for I have ſeveral times made a liquor which, when it has well ſettled in a cloſe phial, is tranſparent and colourleſs; but as ſoon as the glaſs is unſtopped, begins to fly away very plentifully in a white and opacous fume. And there are other bodies, whoſe fumes, when they fill a receiver, would make one ſuſpect it contains milk; and yet when theſe fumes ſettle into a liquor, that liquor is not white, but tranſparent; and ſuch white fumes I have ſeen afforded by unſtopping a

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liquor I know, which yet is itſelf diaphanous and red: nor are theſe the only inſtances of this kind, that our trials can ſupply us with. And if the ſuperficial corpuſcles be of the groſſer ſort, and be ſo framed, that their differing ſides or faces may exhibit differing colours, then the motion or reſt of thoſe corpuſcles may be conſiderable, as to the colour of the ſuperficies they compoſe; upon this account, that ſometimes more, ſometimes fewer of the ſides diſpoſed to exhibit ſuch a colour may by this means become or continue more obverted to the eye than the reſt, and compoſe a phyſical ſurface, that will be more or leſs ſenſibly interrupted. As, to explain my meaning, by propoſing a groſs example, I remember, that in ſome ſorts of leafy plants thick ſet by one another, the two ſides of whoſe leaves were of ſomewhat differing colours, there would be a notable diſparity as to colour, if you looked upon them both, when the leaves, being at reſt, had their upper and commonly expoſed ſides obverted to the eye, and when a breath of wind paſſing thorough them, made great numbers of the uſually hidden ſides of the leaves become conſpicuous. And though the little bodies we were lately ſpeaking of, may ſingly and apart ſeem almoſt colourleſs; yet when many of them are placed by one another, ſo near that the eye does not eaſily diſcern an interruption, within a ſenſible ſpace, they may exhibit a colour: as we ſee, that though the ſlendereſt thrid of dyed ſilk does whilſt looked on ſingle, ſeem almoſt quite devoid of redneſs (for inſtance) yet when numbers of theſe thrids are brought together into one ſkein, their colour becomes notorious.

9. But the ſame occaſion that invited me to ſay what I have mentioned concerning the leaves of trees, invites me alſo to give you ſome account of what happens in changeable taffaties, where we ſee differing colours, as it were, emerge and vaniſh upon the ruffling of the ſame piece of ſilk; as I have divers times with pleaſure obſerved, by the help of ſuch a microſcope, as though it do not very much magnify the object, has in recompence this great conveniency, that you may eaſily, as faſt as you pleaſe, remove it from one part to another of a large object, of which the glaſs taking a great part at once, you may thereby preſently ſurvey the whole. Now by the help of ſuch a microſcope I could eaſily (as I began to ſay) diſcern, that in a piece of changeable taffaty (that appeared, for inſtance, ſometimes red, and ſometimes green) the ſtuff was compoſed of red thrids and green, paſſing under and over each other, and croſſing one another in almoſt innumerable points: and if I looked through the glaſs upon any conſiderable portion of the ſtuff that (for example ſake) to the naked eye appeared to be red, I could plainly ſee, that in that poſition, the red thrids were conſpicuous, and reflected a vivid light. And though I could alſo perceive, that there were green ones, yet by reaſon of their diſadvantageous poſition in the phyſical ſurface of the taffaty, they were in part hid by the more protuberant thrids of the other colour: and for the ſame cauſe, the reflection from as much of the green as was diſcovered, was comparatively but dim and faint. And if, on the contrary, I looked through the microſcope upon any part that appeared green, I could plainly ſee that the red thrids were leſs fully expoſed to the eye, and obſcured by the green ones, which therefore made up the predominant colour. And by obſerving the texture of the ſilken ſtuff, I could eaſily ſo expoſe the thrids either of the one colour or of the other, to my eye, as at pleaſure to exhibit an apparition of red or green, or make thoſe colours ſucceed one another: ſo that, when I obſerved their ſucceſſion by the help of the glaſs, I could mark how the predominant colour did as it were ſtart out, when the thrids that exhibited it came to be advantageouſly placed; and by making little folds in the ſtuff after a certain manner, the ſides that met and terminated in thoſe folds, would appear to the naked eye, one of them red, and the

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other green. When thrids of more than two differing colours chance to be interwoven, the reſulting changeableneſs of the taffaty may be alſo ſomewhat different. But I chuſe to give an inſtance in the ſtuff I have been ſpeaking of, becauſe the mixture being more ſimple, the way whereby the changeableneſs is produced, may be the more eaſily apprehended: and though reaſon alone might readily enough lead a conſidering man to gueſs at the explication, in caſe he knew how changeable taffaties are made; yet I thought it not impertinent to mention it, becauſe both ſcholars and gentlemen are wont to look upon the inquiry into manufactures, as a mechanic employment, and conſequently below them; and becauſe alſo with ſuch a microſcope as I have been mentioning, the diſcovery is as well pleaſant as ſatisfactory, and may afford hints of the ſolution of other phænomena of colours. And it were not amiſs, that ſome diligent inquiry were made, whether the microſcope would give us an account of the variableneſs of colour, that is ſo conſpicuous and ſo delightful in mother of pearl, in opals, and ſome other reſembling bodies. For though I remember I did formerly attempt ſomething of that kind (fruitleſsly enough) upon mother of pearl, yet not having then the advantage of my beſt microſcope, nor ſome conveniencies that might have been wiſhed, I leave it to you, who have better eyes, to try what you can do further; ſince it will be ſome diſcovery to find, that in this caſe the beſt eyes and microſcopes themſelves can make none.

10. I CONFESS, *Pyrophilus*, that a great part of what I have delivered (or propoſed rather) concerning the differing forms of aſperity in bodies, by which differences, the incident light either comes to be reflected with more or leſs of ſhade, and with that ſhade more or leſs interrupted, or elſe happens to be alſo otherwiſe modified or troubled, is but conjectural. But I am not ſure, that if it were not for the dulneſs of our ſenſes, either theſe or ſome other notions of kin to them, might be better countenanced; for I am apt to ſuſpect, that if we were ſharp-ſighted enough, or had ſuch perfect microſcopes, as I fear are more to be wiſhed than hoped for, our promoted ſenſe might diſcern in the phyſical ſurfaces of bodies, both a great many latent ruggedneſſes, and the particular ſizes, ſhapes, and ſituations of the extremely little bodies that cauſe them, and perhaps might perceive among other varieties that we now can but imagine, how thoſe little protuberances and cavities do interrupt and dilate the light, by mingling with it a multitude of little and ſingly undiſcernable ſhades, though ſome of them more, and ſome of them leſs minute, ſome leſs, and ſome more numerous, according to the nature and degree of the particular colour we attribute to the viſible object. As we ſee, that in the moon we can with excellent teleſcopes diſcern many hills and valleys, and as it were pits and other parts, whereof ſome are more, and ſome leſs vividly illuſtrated, and others have a fainter, others a deeper ſhade, though the naked eye can diſcern no ſuch matter in that planet. And with an excellent microſcope, where the naked eye did ſee but a green powder, the aſſiſted eye, as we noted above, could diſcern particular granules, ſome of them of a blue, and ſome of them of a yellow colour, which corpuſcles we had beforehand cauſed to be exquiſitely mixed to compound the green.

11. AND, *Pyrophilus*, that you may not think me altogether extravagant in what I have ſaid of the poſſibility (for I ſpeak of no more) of diſcerning the differing forms of aſperity in the ſurfaces of bodies of ſeveral colours, I'll here ſet down a memorable particular that chanced to come to my knowledge, ſince I writ a good part of this eſſay; and it is this. Meeting caſually the other day with the deſervedly famous * Dr. *J. Finch*, extraordinary anatomiſt to that great patron of the Virtuoſi, the now Great Duke of *Tuſcany*, and inquiring of this ingenious perſon, what might be the

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* *Since, for his eminent qualities and loyalty, graced by his Majeſty with the honour of knighthood.*

chief rarity he had ſeen in his late return out of *Italy* into *England*, he told me, it was a man at *Maeſtricht* in the *Low-Countries*, who at certain times can diſcern and diſtinguiſh colours by the touch with his fingers. You will eaſily conclude, that this is far more ſtrange than what I propoſed but as not impoſſible; ſince the ſenſe of the retina ſeeming to be much more tender and quick than that of thoſe groſſer filaments, nerves or membranes of our fingers, wherewith we uſe to handle groſs and hard bodies, it ſeems ſcarce credible, that any accuſtomance, or diet, or peculiarity of conſtitution, ſhould enable a man to diſtinguiſh, with ſuch groſs and unſuitable organs, ſuch nice and ſubtile differences as thoſe of the forms of aſperity, that belong to differing colours; to receive whoſe languid and delicate impreſſions by the intervention of light, nature ſeems to have appointed and contexed into the retina the tender and delicate pith of the optic nerve. Wherefore I confeſs, I propoſed divers ſcruples, and particularly whether the doctor had taken care to bind a napkin or handkerchief over his eyes ſo carefully, as to be ſure he could make no uſe of his ſight, though he had but counterfeited the want of it; to which I added divers other queſtions, to ſatisfy myſelf, whether there were any likelihood of colluſion or other tricks. But I found that the judicious doctor having gone far out of his way, purpoſely to ſatisfy himſelf and his learned prince about this wonder, had been very watchful and circumſpect to keep himſelf from being impoſed upon. And that he might not through any miſtake in point of memory miſinform me, he did me the favour, at my requeſt, to look out the notes he had written for his own and his prince's information, the ſum of which memorials, as far as we ſhall mention them here, was this, that the doctor having been informed at *Utrecht*, that there lived one at ſome miles diſtance from *Maeſtricht*, who could diſtinguiſh colours by the touch; when he came to the laſt named town, he ſent a meſſenger for him, and having examined him, was told upon inquiry theſe particulars.

THAT the man's name was *John Vermaaſen*, at that time about 33 years of age; that when he was but two years old, he had the ſmall-pox, which rendered him abſolutely blind; that at this preſent he is an organiſt, and ſerves that office in a public choir.

THAT the doctor diſcourſing with him over night, the blind man affirmed, that he could diſtinguiſh colours by the touch, but that he could not do it, unleſs he were faſting; any quantity of drink taking from him that exquiſiteneſs of touch, which is requiſite to ſo nice a ſenſation.

THAT hereupon the doctor provided againſt the next morning ſeven pieces of ribbon, of theſe ſeven colours, black, white, red, blue, green, yellow, and grey; but as for mingled colours, this *Vermaaſen* would not undertake to diſcern them, though if offered, he would tell that they were mixed.

THAT to diſcern the colour of the ribbon, he places it betwixt the thumb and the fore finger, but his moſt exquiſite perception was in his thumb, and much better in the right thumb than in the left.

THAT after the blind man had four or five times told the doctor the ſeveral colours (though blinded with a napkin fot fear he might have ſome ſight) the doctor found he was twice miſtaken, for he called the white black, and the red blue; but ſtill, he, before his error, would lay them by in pairs, ſaying, that though he could eaſily diſtinguiſh them from all others, yet thoſe two pairs were not eaſily diſtinguiſhed amongſt themſelves. Whereupon the doctor deſired to be told by him what kind of diſcrimination he had of colours by his touch, to which he gave a reply, for whoſe ſake chiefly I inſert all this narrative in this place; namely, that all the difference more or leſs aſperity; for ſays he (I give you the doctor's own words) black feels as if you were feeling needles points, or ſome harſh ſand, and red feels very ſmooth.

THAT the doctor having deſired him to tell in order the difference of colours to his touch, he did as follows.

BLACK and white are the moſt aſperous or unequal of all colours, and ſo like, that it is very hard to diſtinguiſh them; but black is the moſt rough of the two; green is next in aſperity, grey next to green in aſperity, yellow is the fifth in degree of aſperity: red and blue are ſo like, that they are as hard to diſtinguiſh as black and white; but red is ſomewhat more aſperous than blue, ſo that red has the ſixth place, and blue the ſeventh in aſperity.

12. To theſe informations the obliging doctor was pleaſed to add the welcome preſent of three of thoſe very pieces of ribbon, whoſe colours in his preſence the blind man had diſtinguiſhed, pronouncing the one grey, the other red, and the third green; which I keep by me as rarities, and the rather, becauſe he feared the reſt were miſcarried.

13. BEFORE I ſaw the notes that afforded me the precedent narrative, I confeſs I ſuſpected this man might have thus diſcriminated colours rather by the ſmell than by the touch; for ſome of the ingredients imployed by dyers to colour things, have ſcents, that are not ſo languid, nor ſo near of kin: but that I thought it not impoſſible that a very critical noſe might diſtinguiſh them, and this I the rather ſuſpected, becauſe he required, that the ribbons, whoſe colours he was to name, ſhould be offered him faſting in the morning; for I have obſerved in ſetting dogs, that the feeding of them (eſpecially with ſome ſorts of aliments) does very much impair the exquiſite ſcent of their noſes. And though ſome of the foregoing particulars would have prevented that conjecture, yet I confeſs to you (*Pyrophilus*) that I would gladly have had the opportunity of examining this man myſelf, and of queſtioning him about divers particulars which I do not find to have been yet thought upon. And though it be not incredible to me, that ſince the liquours that dyers employ to tinge, are qualified to do ſo by multitudes of little corpuſcles of the pigment or dying ſtuff, which are diſſolved and extracted by the liquor, and ſwim to and fro in it, thoſe corpuſcles of colour (as the Atomiſts call them) inſinuating themſelves into, and filling all the pores of the body to be dyed, may aſperate its ſuperficies more or leſs according to the bigneſs and texture of the corpuſcles of the pigment; yet I can ſcarce believe, that our blind man could diſtinguiſh all the colours he did, meerly by the ribbons having more or leſs of aſperity; ſo that I cannot but think, notwithſtanding this hiſtory, that the blind man diſtinguiſhed colours not only by the degrees of aſperity in the bodies offered to him, but by forms of it, though this (latter) would perhaps have been very difficult for him to make an intelligible mention of, becauſe thoſe minute diſparities having not been taken notice of by men for want of touch as exquiſite as our blind man's, are things he could not have intelligibly expreſſed; which will eaſily ſeem probable, if you conſider, that under the name of ſharp, and ſweet, and ſour, there are abundance of, as it were, immediate peculiar reliſhes or taſtes in differing ſorts of wine, which, though critical and experienced palates can eaſily diſcern themſelves, cannot make them be underſtood by others; ſuch minute differences not having hitherto any diſtinct names aſſigned them. And it ſeems that there was ſomething in the forms of aſperity that was requiſite to the diſtinction of colours, beſides the degree of it, ſince he found it ſo difficult to diſtinguiſh black and white from one another, though not from other colours. For I might urge, that he ſeems not conſonant to himſelf about the red, which, as you have ſeen in one place, he repreſents as ſomewhat more aſperous than the blue; and in another, very

ſmooth: but becauſe he ſpeaks of this ſmoothneſs in that place, where he mentions the roughneſs of black, we may favourably preſume that he might mean but a comparative ſmoothneſs; and therefore I ſhall not inſiſt on this, but rather countenance my conjecture by this, that he found it ſo difficult, not only to diſcriminate red and blue (though the firſt of our promiſcuous experiments will inform you, that the red reflects by great odds more light than the other) but alſo to diſtinguiſh black and white from one another, though not from other colours. And indeed, though in the ribbons that were offered him, they might be almoſt equally rough, yet in ſuch ſlender corpuſcles, as thoſe of colour, there may eaſily enough be conceived, not only a greater cloſeneſs of parts, or elſe paucity of protuberant corpuſcles, and the little extant particles may be otherwiſe figured, and ranged in the white than in the black, but the cavities may be much deeper in the one than the other.

14. AND perhaps (*Pyrophilus*) it may prove ſome illuſtration of what I mean, and help you to conceive how this may be, if I repreſent, that where the particles are ſo exceeding ſlender, we may allow the parts expoſed to the ſight and touch to be a little convex in compariſon of the erected particles of black bodies, as if there were wires I know not how many times ſlenderer than a hair: whether you ſuppoſe them to be figured like needles, or cylindrically, like the hairs of a bruſh, with hemiſpherical (or at leaſt convex) tops, they will be ſo very ſlender, and conſequently the points both of the one ſort and the other ſo very ſharp, that even an exquiſite touch will be able to diſtinguiſh no greater difference between them, than that which our blind man allowed, when comparing black and white bodies, he ſaid, that the latter was the leſs rough of the two. Nor is every kind of roughneſs, though ſenſible enough, inconſiſtent with whiteneſs, there being caſes, wherein the phyſical ſuperficies of a body is made by the ſame operation both rough and white; as when the level ſurface of clear water being by agitation aſperated with a multitude of unequal bubbles, does thereby acquire a whiteneſs; and as a ſmooth piece of glaſs, by being ſcratched with a diamond, does in the aſperated part of its ſurface diſcloſe the ſame colour. But more (perchance) of this elſewhere.

15. AND therefore, we ſhall here paſs by the queſtion, whether any thing might be conſidered about the opacity of the corpuſcles of black pigments, and the comparative diaphaneity of thoſe of many white bodies, applied to our preſent caſe; and proceed to repreſent, that the newly mentioned exiguity and ſhape of the extant particles being ſuppoſed, it will then be conſiderable what we lately but hinted (and therefore muſt now ſomewhat explain) that the depth of the little cavities, intercepted between the extant particles, without being ſo much greater in black bodies than in white ones, as to be perceptibly ſo to the groſs organs of touch, may be very much greater in reference to their diſpoſition of reflecting the imaginary ſubtile beams of light. For in black bodies, thoſe little intercepted cavities, and other depreſſions, may be ſo figured, ſo narrow and ſo deep, that the incident beams of light, which the more extant parts of the phyſical ſuperficies are diſpoſed to reflect inwards, may be detained there, and prove unable to emerge; whilſt, in a white body, the ſlender particles may not only by their figure be fitted to reflect the light copiouſly outwards, but the intercepted cavities being not deep, nor perhaps very narrow, the bottoms of them may be ſo conſtituted, as to be fit to reflect outwards much of the light that falls even upon them; as you may poſſibly better apprehend, when we ſhall come to treat of whiteneſs and blackneſs. In the mean time, it may ſuffice, that you take notice with me, that the blind man's relations import no neceſſity of concluding, that though, becauſe, according to the judgment of his touch, black was the rougheſt,

as it is the darkeſt of colours, therefore white, which (according to us) is the lighteſt, ſhould be alſo the ſmootheſt: ſince I obſerve, that he makes yellow to be two degrees more aſperous than blue, and as much leſs aſperous than green; whereas, indeed, yellow does not only appear to the eye a lighter colour than blue, but (by our firſt experiment hereafter to be mentioned) it will appear, that yellow reflected much more light than blue, and manifeſtly more than green; which we need not much wonder at, ſince in this colour, and the two others (blue and yellow) it is not only the reflected light that is to be conſidered, ſince to produce both theſe, refraction ſeems to intervene, which by its varieties may much alter the caſe: which both ſeems to ſtrengthen the conjecture I was formerly propoſing, that there was ſomething elſe in the kinds of aſperity, as well as in the degrees of it, which enabled our blind man to diſcriminate colours, and does at leaſt ſhow, that we cannot, in all caſes, from the bare difference in the degrees of aſperity betwixt colours, ſafely conclude, that the rougher of any two always reflects the leaſt light.

16. BUT this notwithſtanding *(Pyrophilus)* and whatever curioſity I may have had to move ſome queſtions to our ſagacious blind man; yet thus much I think you will admit us to have gained by his teſtimony, that ſince many colours may be felt with the circumſtances above related, the ſurfaces of ſuch coloured bodies muſt certainly have differing degrees, and in all probability have differing forms or kinds of aſperity belonging to them, which is all the uſe that my preſent attempt obliges me to make of the hiſtory above delivered; that being ſufficient to prove, that colour does much depend upon the diſpoſition of the ſuperficial parts of bodies, and to ſhew in general, wherein it is probable that ſuch a diſpoſition does (principally at leaſt) conſiſt.

17. BUT to return to what I was ſaying, before I began to make mention of our blind organiſt; what we have delivered touching the cauſes of the ſeveral forms of aſperity that may diverſify the ſurfaces of coloured bodies, may perchance ſomewhat aſſiſt us to make ſome conjectures in the general, at ſeveral of the ways whereby it is poſſible for the experiments, hereafter to be mentioned, to produce the ſudden changes of colours that are wont to be conſequent upon them: for moſt of theſe phænomena being produced by the intervention of liquors, and theſe for the moſt part abounding with very minute, active, and variouſly figured ſaline corpuſcles, liquors ſo qualified may well enough very nimbly alter the texture of the body they are employed to work upon, and ſo may change the form of aſperity, and thereby make them remit to the eye the light that falls on them after another manner than they did before, and by that means vary the colour, ſo far forth as it depends upon the texture or diſpoſition of the ſeen parts of the object; which I ſay, *Pyrophilus*, that you may not think I would abſolutely exclude all other ways of modifying the beams of light between their parting from the lucid body, and their reception into the common ſenſory.

18. Now there ſeem to me divers ways, by which we may conceive that liquors may nimbly alter the colour of one another, and of other bodies, upon which they act; but my preſent haſte will allow me to mention but ſome of them, without inſiſting ſo much as upon thoſe I ſhall name.

19. AND firſt, the minute corpuſcles that compoſe a liquor may eaſily inſinuate themſelves into thoſe pores of bodies, whereto their ſize and figure makes them congruous; and theſe pores they may either exactly fill, or but inadequately: and in this latter caſe they will for the moſt part alter the number and figure, and always the bigneſs of the former pores. And in what capacity ſoever theſe corpuſcles of a liquor come to be lodged or harboured in the pores that admit them, the ſurface of

the body will for the moſt part have its aſperity altered, and the incident light that meets with a groſſer liquor in the little cavities that before contained nothing but air, or ſome yet ſubtiler fluid, will have its beams either refracted, or imbibed, or elſe reflected more or leſs interruptedly than they would be, if the body had been unmoiſtened: as we ſee, that even fair water falling on white paper, or linen, and divers other bodies apt to ſoak it in, will for ſome ſuch reaſons as thoſe newly mentioned, immediately alter the colour of them, and for the moſt part make it ſadder than that of the unwetted parts of the ſame bodies. And ſo you may ſee, that when in the ſummer the highways are dry and duſty, if there falls ſtore of rain, they will quickly appear of a much darker colour than they did before; and if a drop of oil be let fall upon a ſheet of white paper, that part of it, which by the imbibition of the liquor, acquires a greater continuity, and ſome tranſparency, will appear much darker than the reſt, many of the incident beams of light being now tranſmitted, that otherwiſe would be reflected towards the beholder's eyes.

20. Secondly, A liquor may alter the colour of a body, by freeing it from thoſe things that hindered it from appearing in its genuine colour; and though this may be ſaid to be rather a reſtoration of a body to its own colour, or a retection of its native colour, than a change, yet ſtill there intervenes in it a change of the colour which the body appeared to be of before this operation. And ſuch a change a liquor may work, either by diſſolving, or corroding, or by ſome ſuch way of carrying off that matter, which either veiled or diſguiſed the colour that afterwards appears. Thus we reſtore old pieces of dirty gold to a clean and nitid yellow, by putting them into the fire, and into aqua-fortis, which take off the adventitious filth that made that pure metal look of a dirty colour: and there is alſo an eaſy way to reſtore ſilver coins to their due luſtre, by fetching off that which diſcoloured them. And I know a chymical liquor, which I employed to reſtore pieces of cloth ſpotted with greaſe to their proper colour, by imbibing the ſpotted part with this liquor, which incorporating with the greaſe, and yet being of a very volatile nature, does eaſily carry it away with itſelf. And I have ſometimes tried, that by rubbing upon a good touch-ſtone a certain metalline mixture ſo compounded, that the impreſſion it left upon the ſtone appeared of a very differing colour from that of gold, yet a little of aqua fortis would in a trice make the golden colour diſcloſe itſelf, by diſſolving the other metalline corpuſcles that concealed thoſe of the gold, which you know that menſtruum will leave untouched.

21. Thirdly, A liquor may alter the colour of a body by making a comminution of its parts, and that principally two ways; the firſt by disjoining and diſſipating thoſe cluſters of particles, if I may ſo call them, which ſtuck more looſely together, being faſtened only by ſome more eaſily diſſoluble cement, which ſeems to be the caſe of ſome of the following experiments, where you will find the colour of many corpuſcles brought to cohere by having been precipitated together, deſtroyed by the affuſion of very piercing and inciſive liquors. The other of the two ways I was ſpeaking of, is, by dividing the groſſer and more ſolid particles into minute ones, which will be always leſſer, and for the moſt part otherwiſe ſhaped than the entire corpuſcle ſo divided, as it will happen in a piece of wood reduced into ſplinters or chips, or as when a piece of cryſtal heated red-hot and quenched in cold water is cracked into a multitude of little fragments, which, though they fall not aſunder, alter the diſpoſition of a body of the cryſtal, as to its manner of reflecting the light, as we ſhall have occaſion to ſhew hereafter.

22. There is a fourth way contrary to the third, whereby a liquor may change the colour of another body, eſpecially of another fluid; and that is, by procuring the coalition of ſeveral particles that before lay too ſcattered and diſperſed to exhibit the colour that afterwards appears. Thus ſometimes when I have had the ſolution of gold ſo dilated, that I doubted whether the liquor had really imbibed any true gold or no, by pouring in a little mercury, I have been quickly able to ſatisfy myſelf, that the liquor contained gold; that metal after a little while cloathing the ſurface of the quickſilver with a thin film of its own livery. And chiefly, though not only by this way of bringing the minute parts of bodies together in ſuch numbers, as to make them become notorious to the eye, many of theſe colours ſeem to be generated which are produced by precipitations, eſpecially by ſuch as are wont to be made with fair water; as when reſinous gums diſſolved in ſpirit of wine, are let fall again, if the ſpirit be copiouſly diluted with that weakening liquor. And ſo out of the rectified and tranſparent butter of antimony, by the bare mixture of fair water, there will be plentifully precipitated that milk-white ſubſtance, which by having its looſer ſalts well waſhed off, is turned into that medicine, which vulgar chymiſts are pleaſed to call *Mercurius Vitæ*.

23. A Fifth way, by which a liquor may change the colour of a body, is, by diſlocating the parts, and putting them out of their former order into another, and perhaps alſo altering the poſture of the ſingle corpuſcles as well as their order or ſituation in reſpect of one another. What certain kinds of commotion or diſlocation of the parts of a body may do towards the changing its colour, is not only evident in the mutations of colour obſervable in quickſilver, and ſome other concretes long kept by chymiſts in a convenient heat, though in cloſe veſſels, but in the obvious degenerations of colour, which every body may take notice of in bruiſed cherries, and other fruit, by comparing after a while the colour of the injured with that of the ſound part of the ſame fruit. And that alſo ſuch liquors, as we have been ſpeaking of, may greatly diſcompoſe the textures of many bodies, and thereby alter the diſpoſition of their ſuperficial parts, the great commotion made in metals, and ſeveral other bodies by aqua fortis, oil of vitriol, and other ſaline menſtruums, may eaſily perſuade us; and what ſuch varied ſituations of parts may do towards the diverſifying of the manner of their reflecting the light, may be gueſſed in ſome meaſure by the beating of tranſparent glaſs into a white powder, but far better by the experiments lately pointed at, and hereafter delivered, as the producing and deſtroying colours by the means of ſubtile ſaline liquors, by whoſe affuſion the parts of other liquors are manifeſtly both agitated, and likewiſe diſpoſed after another manner than they were before ſuch affuſion. And in ſome chymical oils, as particularly that of lemon peels, by barely ſhaking the glaſs that holds it into bubbles, that tranſpoſition of the parts which is conſequent to the ſhaking, will ſhew you on the ſurfaces of the bubbles exceeding orient and lively colours, which, when the bubbles relapſe into the reſt of the oil, do immediately vaniſh.

24. I know not, *Pyrophilus*, whether I ſhould mention as a diſtinct way, becauſe it is of a ſomewhat more general nature, that power whereby a liquor may alter the colour of another body, by putting the parts of it into motion; for though poſſibly the motion ſo produced does, as ſuch, ſeldom ſuddenly change the colour of the body whoſe parts are agitated, yet this ſeems to be one of the moſt general, however not immediate cauſes of the quick change of colours in bodies. For the parts being put into motion by the adventitious liquor, divers of them that were before united, may become thereby disjoined, and when that motion ceaſes or decays, others of

them may ſtick together, and that in a new order, by which means the motion may ſometimes produce permanent changes of colours, as in the experiment you will meet with hereafter, of preſently turning a ſnowy white body into a yellow, by the bare affuſion of fair water, which probably ſo diſſolves the ſaline corpuſcles that remained in the calx, and ſets them at liberty to act upon one another, and the metal, far more powerfully than the water without the aſſiſtance of ſuch ſaline corpuſcles could do. And though you rub blue vitriol, how venereal and unſophiſticated ſoever it be, upon the whetted blade of a knife, it will not impart to the iron its latent colour; but if you moiſten the vitriol with your ſpittle, or common water, the particles of the liquor disjoining thoſe of the vitriol, and thereby giving them the various agitation requiſite to fluid bodies, the metalline corpuſcles of the thus diſſolved vitriol will lodge themſelves in throngs in the ſmall and congruous pores of the iron they are rubbed on, and ſo give the ſurface of it the genuine colour of the copper.

25. THERE remains yet a way, *Pyrophilus*, to be mentioned, by which a liquor may alter the colour of another body, and this ſeems the moſt important of all, becauſe though it be named but as one, yet it may indeed comprehend many; and that is, by aſſociating the ſaline corpuſcles, or any other ſort of the more rigid ones of the liquor, with the particles of the body that it is employed to work upon. For theſe adventitious corpuſcles aſſociating themſelves with the protuberant particles of the ſurface of a coloured body, muſt neceſſarily alter their bigneſs, and will moſt commonly alter their ſhape. And how much the colours of bodies depend upon the bulk and figure of their ſuperficial particles, you may gueſs by this, that eminent antient philoſophers, and divers moderns, have thought that all colours might, in a general way, be made out by theſe two; whoſe being diverſified will, in our caſe, be attended with theſe two circumſtances; the one, that the protuberant particles being increaſed in bulk, they will oftentimes be varied as to the cloſeneſs or laxity of their order, fewer of them being contained within the ſame ſenſible (though minute) ſpace than before; or elſe by approaching to one another, they muſt ſtraiten the pores, and it may be too they will, by their manner of aſſociating themſelves with the protuberant particles, intercept new pores. And this invites me to conſider farther, that the adventitious corpuſcles I have been ſpeaking of, may likewiſe produce a great change, as well in the little cavities or pores, as in the protuberances of a coloured body; for, beſides what we have juſt now taken notice of, they may, by lodging themſelves in thoſe little cavities, fill them up, and it may well happen, that they may not only fill the pores they inſinuate themſelves into, but likewiſe have their upper parts extant above them; and partly by theſe new protuberances, partly by increaſing the bulk of the former, theſe extraneous corpuſcles may much alter the number and bigneſs of the ſurface's pores, changing the old and intercepting new ones. And then it is odds, but the order of the little extancies, and conſequently that of the little depreſſions in point of ſituation will be altered likewiſe: as if you diſſolve quickſilver in ſome kind of aqua fortis, the ſaline particles of the menſtruum, aſſociating themſelves with the mercurial corpuſcles, will make a green ſolution, which afterwards eaſily enough degenerates. And red lead, or minium, being diſſolved in ſpirit of vinegar, yields not a red, but a clear ſolution, the redneſs of the lead being by the liquor deſtroyed. But a better inſtance may be taken from copper; for I have tried, that if upon a copper-plate, you let ſome drops of weak aqua fortis reſt for a while, the corpuſcles of the menſtruum joining with thoſe of the metal, will produce a very ſenſible aſperity upon the ſurface of the plate, and will concoagulate that way into very minute grains of a pale blue vitriol; whereas, if upon

another part of the ſame plate you ſuffer a little ſtrong ſpirit of urine to reſt a competent time, you ſhall find the aſperated ſurface adorned with a deeper and richer blue. And the ſame aqua fortis, that will quickly change the redneſs of red lead into a darker colour, will, being put upon crude lead, produce a whitiſh ſubſtance, as with copper it did a blueiſh. And as with iron it will produce a reddiſh, and on white quills a yellowiſh, ſo much may the coalition of the parts of the ſame liquor, with the differingly figured particles of ſtable bodies, divers ways aſperate the differingly diſpoſed ſurfaces, and ſo diverſify the colour of thoſe bodies. And you will eaſily believe, that in many changes of colour, that happen upon the diſſolutions of metals, and precipitations made with oil of tartar, and the like fixed ſalts, there may intervene a coalition of ſaline corpuſcles with the particles of the body diſſolved or precipitated, if you examine how much the vitriol of a metal may be heavier than the metalline part of it alone, upon the ſcore of the ſaline parts concoagulated therewith; and, that in ſeveral precipitations the weight of the calx does for the ſame reaſon much exceed that of the metal, when it was firſt put in to be diſſolved.

26. BUT, *Pyrophilus*, to conſider theſe matters more particularly would be to forget that I declared againſt adventuring, at leaſt for this time, at particular theories of colours, and that accordingly you may juſtly expect from me rather experiments than ſpeculations: and therefore I ſhall diſmiſs this ſubject of the forms of ſuperficial aſperity in coloured bodies, as ſoon as I ſhall but have named to you, by way of ſupplement to what we have hitherto diſcourſed in this ſhort ſection, a couple of particulars (which you will eaſily grant me); the one, that there are divers other ways for the ſpeedy production even of true and permanent colours in bodies, beſides thoſe practicable by the help of liquors: for proof of which advertiſement, though ſeveral examples might be alledged, yet I ſhall need but remind you of what I mentioned to you above, touching the change of colours ſuddenly made on tempered ſteel, and on lead, by the operation of heat, without the intervention of a liquor. But the other particular I am to obſerve to you, is of more importance to our preſent ſubject; and it is, that though nature and art may in ſome caſes ſo change the aſperity of the ſuperficial parts of a body, as to change its colour by either of the ways I have propoſed, ſingle or unaſſiſted; yet for the moſt part it is by two or three, or perhaps by more of the forementioned ways aſſociated together, that the effect is produced. And if you conſider how variouſly theſe ſeveral ways and ſome others allied unto them, which I have left unmentioned, may be compounded and applied, you will not much wonder that ſuch fruitful, whether principles (or manners of diverſification) ſhould be fitted to change or generate no ſmall ſtore of differing colours.

27. HITHERTO, *Pyrophilus*, we have in diſcourſing of the aſperity of bodies conſidered the little protuberances of other ſuperficial particles which make up that roughneſs, as if we took it for granted, that they muſt be perfectly opacous and impenetrable by the beams of light, and ſo muſt contribute to the variety of colours, as they terminate more or leſs light, and reflect it to the eye mixed with more or leſs of thus or thus mingled ſhades. But to deal ingenuouſly with you, *Pyrophilus*, before I proceed any further, I muſt not conceal from you, that I have often thought it worth a ſerious inquiry, whether or no particles of matter, each of them ſingly inſenſible, and therefore ſmall enough to be capable of being ſuch minute particles, as the Atomiſts both of old and of late have (not abſurdly) called *Corpuſcula Coloris*, may not yet conſiſt each of them of divers yet minuter particles, betwixt which we may conceive little commiſſures where they adhere to one another, and, however, may

not be porous enough to be, at leaſt in ſome degree, pervious to the unimaginably ſubtile corpuſcles that make up the beams of light, and conſequently to be in ſuch a degree diaphanous. For, *Pyrophilus*, that the propoſed inquiry may be of moment to him that ſearches after the nature of colour, you will eaſily grant, if you conſider, that whereas perfectly opacous bodies can but reflect the incident beams of light, thoſe that are diaphanous are qualified to refract them too; and that refraction has ſuch a ſtroke in the production of colours, as you cannot but have taken notice of, and perhaps admired in the colours generated by the trajection of light through drops of water that exhibit a rainbow, through priſmatical glaſſes, and through divers other tranſparent bodies. But 'tis like, *Pyrophilus*, you will more eaſily allow that about this matter it is rather important to have a certainty, than that it is rational to entertain a doubt; wherefore I muſt mention to you ſome of the reaſons that make me think it may need a further inquiry: for I find that in a darkened room, where the light is permitted to enter but at one hole, the little wandering particles of duſt, that are commonly called motes, and, unleſs in the ſun-beams, are not taken notice of by the unaſſiſted ſight; I have, I ſay, often obſerved that theſe roving corpuſcles being looked on by an eye placed on the one ſide of the beams that entered the little hole, and by the darkneſs having its pupil much enlarged, I could diſcern that theſe motes as ſoon as they came within the compaſs of the luminous, whether cylinder or inverted cone, if I may ſo call it, that was made up by the unclouded beams of the ſun, did in certain poſitions appear adorned with very vivid colours, like thoſe of the rainbow, or rather like thoſe of very minute, but ſparkling fragments of diamonds: and as ſoon as the continuance of their motion had brought them to an inconvenient poſition in reference to the light and the eye, they were only viſible without darting any lively colours as before. Which ſeems to argue, that theſe little motes, or minute fragments of ſeveral ſorts of bodies reputed opacous, and only crumbled as to their exterior and looſer parts into duſt, did not barely reflect the beams that fell upon them, but remit them to the eye refracted too. We may alſo obſerve, that ſeveral bodies (as well ſome of a vegetable, as others of an animal nature) which are wont to paſs for opacous, appear in great part tranſparent, when they are reduced into thin parts, and held againſt a powerful light. This I have not only taken notice of in pieces of ivory reduced into thick leaves, as alſo in divers conſiderable thick ſhells of fiſhes, and in ſhaving of wood; but I have alſo found that a piece of deal, far thicker than one would eaſily imagine, being purpoſely interpoſed betwixt my eye placed in a room, and the clear day-light, was not only ſomewhat tranſparent, but (perhaps by reaſon of its gummous nature) appeared quite through of a lovely red. And in the darkened room above mentioned, bodies held againſt the hole at which the light entered, appeared far leſs opacous than they would elſewhere have done; inſomuch that I could eaſily and plainly ſee, through the whole thickneſs of my hand, the motions of a body placed (at a very near diſtance indeed, but yet) beyond it. And even in minerals, the opacity is not always ſo great as many think, if the body be made thin: for white marble, though of a pretty thickneſs, being within a due diſtance placed betwixt the eye and a convenient light, will ſuffer the motions of one's finger to be well diſcerned through it, and ſo will pieces, thick enough, of many common flints. But above all, that inſtance is remarkable, that is afforded us by *Muſcovy* glaſs (which ſome call *Selenites*, others *Lapis Specularis*;) for though plates of this mineral, though but of a moderate thickneſs, do often appear opacous, yet if one of theſe be dexterouſly ſplit into the thinneſt leaves it is made up of, it will yield ſuch a number of them, as ſcarce any thing but experience could have perſuaded me; and theſe leaves will afford the moſt tranſparent ſort of conſiſtent bodies, that, for aught I have obſerved, are yet unknown; and a ſingle leaf or plate will be ſo far from being opacous, that it will ſcarce be ſo much as viſible. And multitudes of bodies there are, whoſe fragments ſeem opacous to the naked eye, which yet, when I have included them in good microſcopes, appeared tranſparent; but, *Pyrophilus*, on the other ſide I am not yet ſure that there are no bodies, whoſe minute particles even in ſuch a microſcope as that of mine, which I was lately mentioning, will not appear diaphanous. For having conſidered mercury precipitated *per ſe*, the little granules that made up the powder, looked like little fragments of coral beheld by the naked eye at a diſtance (for very near at hand coral will ſometimes, eſpecially if it be good, ſhew ſome tranſparency). Filings likewiſe of ſteel and copper, though in an excellent microſcope, and a fair day, they ſhowed like pretty big fragments of thoſe metals, and had conſiderable brightneſs on ſome of their ſurfaces, yet I was not ſatisfied, that I perceived any reflection from the inner parts of any of the filings. Nay, having looked in my beſt microſcope upon the red calx of lead (commonly called *Minium)* neither I, nor any I ſhewed it to, could diſcern it to be other than opacous, though the day were clear, and the object ſtrongly enlightened. And the deeply red colour of vitriol appeared in the ſame microſcope (notwithſtanding the great comminution effected by the fire) but like groſſy beaten brick. So that, *Pyrophilus*, I ſhall willingly reſign you the care of making ſome further inquiries into the ſubject we have now been conſidering; for I confeſs, as I told you before, that I think that the matter may need a further ſcrutiny, nor would I be forward to determine how far or in what caſes the tranſparency or ſemi-diaphaneity of the ſuperficial corpuſcles of bigger bodies may have an intereſt in the production of their colours; eſpecially becauſe that even in divers white bodies, as beaten glaſs, ſnow and froth, where it ſeems manifeſt that the ſuperficial parts are ſingly diaphanous (being either water, or air, or glaſs) we ſee not that ſuch variety of colours are produced as uſually are by the refraction of light even in thoſe bodies, when by their bigneſs, ſhape, *&c.* they are conveniently qualified to exhibit ſuch various and lively colours as thoſe of the rainbow, and of priſmatical glaſſes.

28. BY what has been hitherto diſcourſed, *Pyrophilus*, we may be aſſiſted to judge of that famous controverſy which was of old diſputed betwixt the Epicureans and other Atomiſts on one ſide, and moſt other philoſophers on the other ſide; the former denying bodies to be coloured in the dark, and the latter making colour to be an inherent quality, as well as figure, hardneſs, weight, or the like. For though this controverſy be revived, and hotly agitated among the moderns, yet I doubt whether it be not in great part a nominal diſpute; and therefore let us, according to the doctrine formerly delivered, diſtinguiſh the acceptations of the word colour, and ſay, that if it be taken in the ſtricter ſenſe, the Epicureans ſeem to be in the right; for if colour be indeed, though not according to them, but light modified, how can we conceive that it can ſubſiſt in the dark, that is, where it muſt be ſuppoſed there is no light: but, on the other ſide, if colour be conſidered as a certain conſtant diſpoſition of the ſuperficial parts of the object to trouble the light they reflect after ſuch and ſuch a determinate manner, this conſtant, and if I may ſo ſpeak, modifying diſpoſition perſevering in the object, whether it be ſhined upon or no, there ſeems no juſt reaſon to deny, but that in this ſenſe, bodies retain their colour as well in the night as day; or, to ſpeak a little otherwiſe, it may be ſaid, that bodies are potentially coloured in the dark, and actually in the light. But of this matter diſcourſing more fully elſewhere, as it is a difficulty that concerns qualities in general, I ſhall forbear to inſiſt on it here.

CHAP. IV.

1. OF greater moment in the inveſtigation of the nature of colours is the controverſy, whether thoſe of the rainbow, and thoſe that are often ſeen in clouds, before the riſing, or after the ſetting of the ſun; and in a word, whether thoſe other colours, that are wont to be called emphatical, ought or ought not to be accounted true colours. I need not tell you that the negative is the common opinion, eſpecially in the ſchools, as may appear by that vulgar diſtinction of colours, whereby theſe under conſideration are termed apparent, by way of oppoſition to thoſe that in the other member of the diſtinction are called true or genuine. This queſtion I ſay ſeems to me of importance, upon this account, that it being commonly granted (or however, eaſy enough to be proved) that emphatical colours are light itſelf modified by refractions chiefly, with a concurrence ſometimes of reflections, and perhaps ſome other accidents depending on theſe two; if theſe emphatical colours be reſolved to be genuine, it will ſeem conſequent, that colours, or at leaſt divers of them, are but diverſified light, and not ſuch real and inherent qualities as they are commonly thought to be.

2. Now ſince we are wont to eſteem the echoes and other ſounds of bodies, to be true ſounds, all their odours to be true odours, and (to be ſhort) ſince we judge other ſenſible qualities to be true ones, becauſe they are the proper objects of ſome or other of our ſenſes; I ſee not why emphatical colours, being the proper and peculiar objects of the organ of ſight, and capable to affect it as truly and as powerfully as other colours, ſhould be reputed but imaginary ones.

And if we have (which perchance you will allow) formerly evinced colour (when the word is taken in its more proper ſenſe) to be but modified light, there will be ſmall reaſon to deny theſe to be true colours, which more manifeſtly than others diſcloſe themſelves to be produced by diverſifications of the light.

3. There is indeed taken notice of, a difference betwixt theſe apparent colours, and thoſe that are wont to be eſteemed genuine, as to the duration, which has induced ſome learned men to call the former rather evanid than fantaſtical. But as the ingenious *Gaſſendus* does ſomewhere judiciouſly obſerve, if this way of arguing were good, the greenneſs of a leaf ought to paſs for apparent, becauſe, ſoon fading into a yellow, it ſcarce laſts at all, in compariſon of the greenneſs of an emerald. I ſhall add, that if the ſun-beams be in a convenient manner trajected through a glaſs priſm, and thrown upon ſome well ſhaded object within a room, the rainbow thereby painted on the ſurface of the body that terminates the beams, may oftentimes laſt longer than ſome colours I have produced in certain bodies, which would juſtly, and without ſcruple be accounted genuine colours, and yet ſuddenly degenerate, and loſe their nature.

4. A greater diſparity betwixt emphatical colours, and others, may perhaps be taken from this, that genuine colours ſeem to be produced in opacous bodies by reflection, but apparent ones in diaphanous bodies, and principally by refraction; I ſay principally, rather than ſolely, becauſe in ſome caſes reflection alſo may concur: but ſtill this ſeems not to conclude theſe latter colours not to be true ones. Nor muſt what has been newly ſaid of the differences of true and apparent colours, be interpreted in too unlimited a ſenſe, and therefore it may perhaps ſomewhat aſſiſt you, both to reflect upon the two foregoing objections, and to judge of ſome other paſſages which you will meet with in this tract, if I take this occaſion to obſerve to you,

that if water be agitated into froth, it exhibits, you know, a white colour, which ſoon after it loſes upon the reſolution of the bubbles into air and water. Now in this caſe either the whiteneſs of the froth is a true colour, or not; if it be, then true colours, ſuppoſing the water pure and free from mixtures of any thing tenacious, may be as ſhort-lived as thoſe of the rainbow; alſo the matter, wherein the whiteneſs did reſide, may in a few moments perfectly loſe all footſteps or remains of it. And beſides, even diaphanous bodies may be capable of exhibiting true colours by reflection; for that whiteneſs is ſo produced, we ſhall anon make it probable. But if on the other ſide it be ſaid, that the whiteneſs of froth is an emphatical colour, then it muſt no longer be ſaid, that fantaſtical colours require a certain poſition of the luminary and the eye, and muſt be varied or deſtroyed by the change thereof, ſince froth appears white, whether the ſun be riſing or ſetting, or in the meridian, or any where between it and the horizon, and from what (neighbouring) place ſoever the beholder's eye looks upon it. And ſince by making a liquor tenacious enough, yet without deſtroying its tranſparency, or ſtaining it with any colour, you may give the little films, whereof the bubbles conſiſt, ſuch a texture as may make the froth laſt very many hours, if not ſome days, or even weeks, it will render it ſomewhat improper to aſſign duration for the diſtinguiſhing character to diſcriminate genuine from fantaſtical colours. For ſuch froth may much outlaſt the undoubtedly true colours of ſome of nature's productions, as in that gaudy plant, not undeſervedly called the *Marvel of Peru*, the flowers do often fade the ſame day they are blown; and I have often ſeen a Virginian flower, which uſually withers within the compaſs of a day; and I am credibly informed, that not far from hence, a curious herboriſt has a plant, whoſe flowers periſh in about an hour. But, if the whiteneſs of water turned into froth muſt therefore be reputed emphatical, becauſe it appears not that the nature of the body is altered, but only that the diſpoſition of its parts, in reference to the incident light, is changed, why may not the whiteneſs be accounted emphatical too; which I ſhall ſhew anon to be producible, barely by ſuch another change in black horn: and yet this ſo eaſily acquired whiteneſs ſeems to be as truly its colour as the blackneſs was before, and at leaſt is more permanent than the greenneſs of leaves, the redneſs of roſes, and in ſhort, than the genuine colours of the moſt part of nature's productions. It may indeed be further objected, that according as the ſun or other luminous body changes place, theſe emphatical colours alter or vaniſh. But not to repeat what I have juſt now ſaid, I ſhall add, that if a piece of cloth in a draper's ſhop (in ſuch the light being ſeldom primary) be variouſly folded, it will appear of differing colours, as the parts happen to be more illuminated, or more ſhaded; and if you ſtretch it flat, it will commonly exhibit ſome one uniform colour: and yet theſe are not wont to be reputed emphatical, ſo that the difference ſeems to be chiefly this, that in the caſe of the rainbow, and the like, the poſition of the luminary varies the colour, and in the cloth I have been mentioning, the poſition of the object does it. Nor am I forward to allow, that in all caſes, the apparition of emphatical colours requires a determinate poſition of the eye; for if men will have the whiteneſs of froth emphatical, you know what we have already inferred from thence. Beſides, the ſunbeams trajected through a triangular glaſs, after the manner lately mentioned, will, upon the body that terminates them, paint a rainbow, that may be ſeen, whether the eye be placed on the right hand of it, or the left, or above, or beneath it, or before or behind it: and though there may appear ſome little variation in the colours of the rainbow beheld from differing parts of the room, yet ſuch a diverſity may be alſo obſerved by an attentive eye in real colours, looked upon under the like circumſtances.

Nor will it follow, that becauſe there remain no footſteps of the colour upon the object, when the priſm is removed, that therefore the colour was not real, ſince the light was truly modified by the refraction and reflection it ſuffered in its trajection through the priſm; and the object in our caſe ſerved for a ſpecular body, to reflect that colour to the eye. And that you may not be ſtartled, *Pyrophilus*, that I ſhould venture to ſay, that a rough and coloured object may ſerve for a ſpeculum to reflect the artificial rainbow I have been mentioning, conſider what uſually happens in darkened rooms, where a wall, or other body conveniently ſituated within, may ſo reflect the colours of bodies without the room, that they may very clearly be diſcerned and diſtinguiſhed; and yet it is taken for granted, that the colours ſeen in a darkened room, though they leave no traces of themſelves upon the wall or body that receives them, are the true colours of the external objects, together with which the colours of the images are moved, or do reſt. And the error is not in the eye, whoſe office is only to perceive the appearances of things, and which does truly ſo; but in the judging or eſtimative faculty, which miſtakingly concludes that colour to belong to the wall, which does indeed belong to the object, becauſe the wall is that from whence the beams of light, that carry the viſible ſpecies, do come in ſtrait lines directly to the eye: as for the ſame reaſon we are wont, at a certain diſtance from concave ſpherical glaſſes, to perſuade ourſelves, that we ſee the image come forth to meet us, and hang in the air betwixt the glaſs and us, becauſe the reflected beams, that compoſe the image croſs in that place where the image ſeems to be, and thence, and not from the glaſs, do in direct lines take their courſe to the eye. And upon the like cauſe it is, that divers deceptions in ſounds and other ſenſible objects do depend, as we elſewhere declare.

5. I KNOW not whether I need add, that I have purpoſely tried (as you will find ſome pages hence, and will perhaps think ſomewhat ſtrange) that colours, that are called emphatical, becauſe not inherent in the bodies in which they appear, may be compounded with one another, as thoſe that are confeſſedly genuine may. But when all this is ſaid, *Pyrophilus*, I muſt advertiſe you, that it is but problematically ſpoken; and that though I think the opinion I have endeavoured to fortify probable, yet a great part of our diſcourſe concerning colours may be true, whether that opinion be ſo or not.

CHAP. V.

1. THERE are, you know, *Pyrophilus*, beſides thoſe obſolete opinions about colours, which have been long ſince rejected, very various theories, that have each of them, even at this day, eminent men for their abettors: for the Peripatetic ſchools, though they diſpute amongſt themſelves divers particulars concerning colours, yet in this they ſeem unanimouſly enough to agree, that colours are inherent and real qualities, which the light doth but diſcloſe, and not concur to produce. Beſides, there are moderns, who with a ſlight variation adopt the opinion of *Plato*; and as he would have colour to be nothing but a kind of flame conſiſting of minute corpuſcles, as it were darted by the object againſt the eye, to whoſe pores their littleneſs and figure made them congruous; ſo theſe would have colour to be an internal light of the more lucid parts of the object, darkened, and conſequently altered by the various mixtures of the leſs luminous parts. There are alſo others, who, in imitation of ſome of the antient Atomiſts, make colour not to be lucid ſteam, but yet a corporeal effluvium iſſuing out of the coloured body; but the knowingeſt of theſe have of late reformed their hypotheſis, by acknowledging and adding, that ſome external

light is neceſſary to excite, and, as they ſpeak, ſolicit theſe corpuſcles of colour, as they call them, and bring them to the eye. Another and more principal opinion of the modern philoſophers, to which this laſt named may by a favourable explication be reconciled, is that, which derives colours from the mixture of light and darkneſs, or rather light and ſhadows. And as for the Chymiſts, it is known, that the generality of them aſcribe the origin of colours to the ſulphureous principle in bodies; though I find, as I elſewhere largely ſhew, that ſome of the chiefeſt of them derive colours rather from ſalt than ſulphur, and others from the third hypoſtatical principle, mercury. And as for the Carteſians, I need not tell you, that they, ſuppoſing the ſenſation of light to be produced by the impulſe made upon the organs of ſight, by certain extremely minute and ſolid globules, to which the pores of the air and other diaphanous bodies are pervious, endeavour to derive the varieties of colours from the various proportion of the direct progreſs or motion of theſe globules to their circumvolution or motion about their own center, by which varying proportion they are by this hypotheſis ſuppoſed qualified to ſtrike the optic nerve after ſeveral diſtinct manners, ſo as to produce the perception of differing colours.

2. BESIDES theſe ſix principal hypotheſes, *Pyrophilus*, there may be ſome others, which though leſs known, may perhaps as well as theſe deſerve to be taken into conſideration by you; but that I ſhould copiouſly debate any of them at preſent, I preſume you will not expect, if you conſider the ſcope of theſe papers, and the brevity I have deſigned in them; and therefore I ſhall at this time only take notice to you in the general of two or three things, that do more peculiarly concern the treatiſe you have now in your hands.

3. AND firſt, though the embracers of the ſeveral hypotheſes I have been naming to you, by undertaking each ſect of them to explicate colours indefinitely by the particular hypotheſes they maintain, ſeem to hold it forth as the only needful theory about that ſubject; yet for my part I doubt, whether any one of all theſe hypotheſes have a right to be admitted excluſively to all others: for I think it probable, that whiteneſs and blackneſs may be explicated by reflection alone without refraction, as you will find endeavoured in the diſcourſe you will meet with ere long, of the origin of whiteneſs and blackneſs; and on the other ſide, ſince I have not found, that by any mixture of white and true black (for there is a blueiſh black, which many miſtake for a genuine) there can be a blue, a yellow, or a red, to name no other colours, produced; and ſince we do find, that theſe colours may be produced in the glaſs priſm and other tranſparent bodies, by the help of refractions, it ſeems, that refraction is to be taken in, into the explication of ſome colours, to whoſe generation they ſeem to concur, either by making a further or other commixture of ſhades with the refracted light, or by ſome other way not now to be diſcourſed. And as it ſeems not improbable, that in caſe the pores of the air, and other diaphanous bodies be every where almoſt filled with ſuch globuli, as the Carteſians ſuppoſe, the various kinds of motion of theſe globuli may in many caſes have no ſmall ſtroke in varying our perception of colour; ſo without the ſuppoſition of theſe globuli, which it is not ſo eaſy to evince, I think we may probably enough conceive in general, that the eye may be variouſly affected, not only by the entire beams of light that fall upon it, as they are ſuch; but by the order, and by the degree of ſwiftneſs, and in a word, by the manner, according to which the particles that compoſe each particular beam arrive at the ſenſory: ſo that whatever be the figure of the little corpuſcles, of which the beams of light conſiſt, not only the celerity or ſlowneſs of their revolution or rotation, in reference to their progreſſive motion, by their more abſolute celerity, their direct or

undulating motion, and other accidents, which may attend their appulſe to the eye, may fit them to make differing impreſſions on it.

4. Secondly, For theſe and the like conſiderations, *Pyrophilus*, I muſt deſire, that you would look upon this little treatiſe, not as a diſcourſe written principally to maintain any of the fore-mentioned theories, excluſively to all others, or ſubſtitute a new one of my own; but as the beginning of a hiſtory of colours, upon which, when you and your ingenious friends ſhall have enriched it, a ſolid theory may be ſafely built. But yet becauſe this hiſtory is not meant barely for a regiſter of the things recorded in it, but for an apparatus to a ſound and comprehenſive hypotheſis, I thought fit ſo to temper the whole diſcourſe, as to make it as conducible as conveniently I can to that end: and therefore I have not ſcrupled to let you ſee, that I was willing to ſave you the labour of cultivating ſome theories, that I thought would never enable you to reach the ends you aim at, ſo to contract your enquiries into a narrow compaſs. For both which purpoſes I thought it requiſite to do theſe two things; the one, to ſet down ſome experiments, which by the help of the reflections and inſinuations that attend them, may aſſiſt you to diſcover the infirmneſs and inſufficiency both of the common Peripatetic doctrine, and of the now more applauded theory of the chymiſts about colour; becauſe theſe two doctrines having poſſeſſed themſelves, the one of the moſt part of the ſchools, and the other of the eſteem of the generality of phyſicians and other learned men, whoſe profeſſions and ways of ſtudy do not exact, that they ſhould ſcrupulouſly examine the very firſt and ſimpleſt principles of nature: I feared it would be to little purpoſe, without doing ſomething to diſcover the inſufficiency of theſe hypotheſes, that I ſhould (which was the other thing I thought requiſite for me to do) ſet down among my other experiments thoſe in the greateſt number, that may let you ſee, that, till I ſhall be better informed, I incline to take colour to be a modification of light; and would invite you chiefly to cultivate that hypotheſis, and improve it to the making out of the generation of particular colours, as I have endeavoured to apply it to the explication of whiteneſs and blackneſs.

5. Thirdly, But *Pyrophilus*, though this be at preſent the hypotheſis I prefer, yet I propoſe it but in a general ſenſe, teaching only, that the beams of light, modified by the bodies whence they are ſent (reflected or refracted) to the eye, produce there that kind of ſenſation men commonly call colour. But whether I think this modification of the light to be performed by mixing it with ſhades, or by varying the proportion of the progreſs and rotation of the Carteſian *Globuli Cæleſtes*, or by ſome other way, which I am not now to mention, I pretend not here to declare; much leſs do I pretend to determine, or ſcarce ſo much as to hope to know all that were requiſite to be known, to give you, or even myſelf, a perfect account of the theory of viſion and colours. For in order to ſuch an undertaking, I would firſt know what light is, and if it be a body (as a body or the motion of a body it ſeems to be) what kind of corpuſcles for ſize and ſhape it conſiſts of, with what ſwiftneſs they move forwards, and whirl about their own centers. Then I would know the nature of refraction, which I take to be one of the abſtruſeſt things (not to explicate plauſibly, but to explicate ſatisfactorily) that I have met with in phyſicks. I would further know, what kind and what degree of commixture of darkneſs or ſhades is made by refractions, or reflections, or both, in the ſuperficial particles of thoſe bodies, that being ſhined upon, conſtantly exhibit the one, for inſtance, a blue, the other a yellow, the third a red colour. I would further know, why this contemperation of light and ſhade, that is made, for example, by the ſkin of a ripe cherry, ſhould exhibit a

red, and not a green, and the leaf of the ſame tree ſhould exhibit a green rather than a red. And indeed, laſtly, why ſince the light that is modified into theſe colours conſiſts but of corpuſcles moved againſt the retina or pith of the optic nerve, it ſhould there not barely give a ſtroke, but produce a colour; whereas a needle wounding likewiſe the eye would not produce colour, but pain. Theſe, and perhaps other things I ſhould think requiſite to be known, before I ſhould judge myſelf to have fully comprehended the true and whole nature of colours: and therefore, though by making the experiments and reflections delivered in this paper, I have endeavoured ſomewhat to leſſen my ignorance in this matter, and think it far more deſirable to diſcover a little, than to diſcover nothing; yet I pretend but to make it probable by the experiments I mention, that ſome colours may be plauſibly enough explicated in the general by the doctrine here propoſed. For whenſoever I would deſcend to the minute and accurate explication of particulars, I find myſelf very ſenſible of the great obſcurity of things, without excepting thoſe, which we never ſee but when they are enlightened, and confeſs with *Scaliger*, *Latet natura hæc* (ſays he, ſpeaking of that colour) *& ſicut aliarum rerum ſpecies in profundiſſima caligne inſcitiæ humanæ*.

Exercitat. 325. Parag. 4.

THE EXPERIMENTAL HISTORY OF COLOURS

PART II.

Of the Nature of Whiteneſs and Blackneſs.

CHAP. I.

1. THOUGH after what I have acknowledged, *Pyrophilus*, of the abſtruſe nature of colours in particular, you will eaſily believe, that I pretend not to give you a ſatisfactory account of whiteneſs and blackneſs; yet not wholly to fruſtrate your expectation of my offering ſomething by way of ſpecimen towards the explication of ſome colours in particular, I ſhall make choice of theſe as the moſt ſimple ones (and by reaſon of their mutual oppoſition the leaſt hardly explicable) about which to preſent you my thoughts, upon condition you will take them at moſt to be my conjectures, not my opinions.

2. WHEN I applied myſelf to conſider, how the cauſe of whiteneſs might be explained by intelligible and mechanical principles, I remembered not to have met with

any thing among the antient corpuſcularian philoſophers, touching the quality we call whiteneſs, ſave that *Democritus* is by *Ariſtotle* ſaid to have aſcribed the whiteneſs of bodies to their ſmoothneſs, and on the contrary their blackneſs to their aſperity. But though about the latter of thoſe qualities his opinion be allowable, as we ſhall ſee anon; yet that he needs a favourable interpretation in what is delivered concerning the firſt (at leaſt if his doctrine be not miſrepreſented in this point, as it has been in many others) we ſhall quickly have occaſion to manifeſt. But amongſt the moderns, the moſt learned *Gaſſendus* in his ingenious epiſtle publiſhed in the year 1642, *De apparente magnitudine ſolis humilis & ſublimis*, reviving the atomical philoſophy, has, though but incidentally, delivered ſomething towards the explication of whiteneſs upon mechanical principles. And becauſe no man, that I know of, has done ſo before him, I ſhall, to be ſure to do him right, give you his ſenſe in his own words: *Cogites velim* (ſays he) *lucem quidem in diaphano nullius coloris videri, ſed in opaco tamen terminante candicare, ac tantò magis, quantò denſior ſeu collectior fuerit. Deinde aquam non eſſe quidam coloris ex ſe candidi, & radium tamen ex eâ reflexum verſus oculum candicare. Rurſus cum plana aquæ ſuperficies non niſi ex una parte eam reflexionem faciat: ſi contigerit tamen illam in aliquot bullas intumeſcere, bullam unamquamque reflectionem facere, & candoris ſpeciem creare certa ſuperficiei parte. Ad hæc ſpumam ex aqua pura non alia ratione videri candeſcere & albeſcere, quam quod ſit congeries confertiſſima minutiſſimarum bullarum, quarum unaquæque ſuum radium reflectit, unde continens candor alborve apparet. Denique nivem nihil aliud videri quam ſpeciem puriſſimæ ſpumæ ex bullulis quam minutiſſimis & confertiſſimis cohærentis. Sed ridiculum me exhibeam, ſi tales meas nugas uberius proponem.*

Album quippe & nigrum, hoc quidem aſperum eſſe dicit, hoc verò læve. *De ſenſu & ſenſil.* 3. 3.

Epiſt. 2. pag. 45.

3. But though in this paſſage, that very ingenious perſon has anticipated part of what I ſhould ſay; yet I preſume you will for all that expect, that I ſhould give you a fuller account of that notion of whiteneſs, which I have the leaſt exceptions to, and of the particulars whence I deduce it; which to do, I muſt mention to you the following experiments and obſervations.

Whiteness then conſidered as a quality in the object ſeems chiefly to depend upon this, that the ſuperficies of the body, that is called white, is aſperated by almoſt innumerable ſmall ſurfaces; which being of an almoſt ſpecular nature, are alſo ſo placed, that ſome looking this way, and ſome that way, they yet reflect the rays of light that fall on them, not towards one another, but outwards towards the ſpectator's eye. In this rude and general account of whiteneſs, it ſeems, that beſides thoſe qualities, which are common to bodies of other colours, as for inſtance the minuteneſs and number of the ſuperficial parts, the two chief things attributed to bodies as white are made to be, firſt, that little protuberances and ſuperficial parts be of ſomewhat a ſpecular nature, that they may, as little looking-glaſſes, each of them reflect the beams it receives (or the little picture of the ſun made on it) without otherwiſe conſiderably altering them; whereas in moſt other colours, they are wont to be much changed, by being alſo refracted, or by being returned to the eye, mixt with ſhades or otherwiſe. And next, that its ſuperficial parts be ſo ſituated, that they retain not the incident rays of light by reflecting them inwards, but ſend them almoſt all back; ſo that the outermoſt corpuſcles of a white body, having their various little ſurfaces of a ſpecular nature, a man can from no place behold the body, but that there will be among thoſe innumerable *ſuperficieculæ*, that look ſome one way, and ſome another, enough of them obverted to his eye, to afford, like a broken looking-glaſs, a confuſed idea, or repreſentation of light, and make ſuch an impreſſion on the organ, as that for which men are wont to call a body white. But this motion will perhaps be beſt explained by the ſame experiments and obſervations, on which it is built, and therefore I ſhall now advance to them.

4. AND in the firſt place I conſider, that the ſun, and other powerfully lucid bodies, are not only wont to offend, which we call to dazzle our eyes; but that if any colour be to be aſcribed to them as they are lucid, it ſeems it ſhould be whiteneſs. For the ſun at noon-day, and in clear weather, and when his face is leſs troubled, and as it were ſtained by the ſteams of ſublunary bodies, and when his beams have much leſs of the atmoſphere to traject in their paſſage to our eyes, appears of a colour more approaching to white, than when nearer the horizon: the interpoſition of certain ſorts of fumes and vapours make him oftentimes appear either red, or at leaſt more yellow. And when the ſun ſhines upon that natural looking-glaſs, a ſmooth water, that part of it, which appears to this or that particular beholder the moſt ſhined on, does to his eye ſeem far whiter than the reſt. And here I ſhall add, that I have ſometimes had the opportunity to obſerve a thing, that may make to my preſent purpoſe; namely, that when the ſun was veiled over as it were, with a thin white cloud, and yet was too bright to be looked upon directly without dazzling, by caſting my eyes upon a ſmooth water, as we ſometimes do to obſerve eclipſes without prejudice to our eyes, the ſun then not far from the meridian appeared to me not red, but ſo white, that it was not without ſome wonder, that I made the obſervation. Beſides, though we in *Engliſh* are wont to ſay, a thing is red-hot, as an expreſſion of its being ſuperlative *ignitum* (if I may ſo ſpeak for want of a proper *Engliſh* word) yet in the forges of ſmiths, and the furnaces of other artificers, by that which they call a white heat, they mean a further degree of ignition, than by that which both they and we call a red heat.

5. SECONDLY, I conſider, that common experience informs us, that as much light overpowers the eye, ſo when the ground is covered with ſnow (a body extremely white) thoſe that have weak eyes are wont to complain of too much light: and even thoſe, that have not, are generally ſenſible of an extraordinary meaſure of light in the air; and if they are fain to look very long upon ſnow, find their ſight offended by it. On which occaſion we may call to mind what *Xenophon* relates, that his *Cyrus* marching his army for divers days through mountains covered with ſnow, the dazzling ſplendor of its whiteneſs prejudiced the ſight of very many of his ſoldiers, and blinded ſome of them; and other ſtories of that nature may be met with in writers of good note. And the like has been affirmed to me by credible perſons of my own acquaintance, and eſpecially by one, who, though ſkilled in phyſic, and not antient, confeſſed to me, when I purpoſely aſked him, that not only during his ſtay in *Muſcovy* he found his eyes much impaired, by being much reduced frequently to travel in the ſnow; but that the weakneſs of his eyes did not leave him when he left that country, but has followed him into theſe parts, and yet continues to trouble him. And to this doth agree what I as well as others have obſerved, namely, that when I travelled by night, when the ground was all covered with ſnow, though the night otherwiſe would not have been lightſome, yet I could very well ſee to chuſe my way. But much more remarkable to my preſent purpoſe is that, which I have met with in *Olaus Magnus*, concerning the way of travelling in winter in the Northern regions, where the days of that ſeaſon are ſo very ſhort: for after other things not needful to be here tranſcribed; *Iter*, ſays he, *diurnum duo ſcilicet montana milliaria (quæ* 12 *Italica ſunt) conficiunt. Nocte verò ſub ſplendidiſſima luna, duplatum iter conſumunt aut triplatum. Neque id incommodè fit, cum nivium reverberatione lunaris ſplendor ſublimes & declives campos illuſtret, ac etiam montium præcipitia ac noxias feras à longè proſpiciant evitandas.*

Cent. Septent. Hiſtor. lib. 4. cap. 13.

evitandas. Which teſtimony I the leſs ſcruple to allege, becauſe it agrees very well with what has been affirmed to me by a phyſician of *Muſcow*, whom the notion I have been treating of concerning whiteneſs invited me to aſk, whether he could not ſee much farther, when he travelled by night in *Ruſſia* than he could do in *England*, or elſewhere, when there was no ſnow upon the ground; for this ingenious perſon informed me, that he could ſee things at a far greater diſtance, and with more clearneſs, when he travelled by night on the *Ruſſian* ſnow, though without the aſſiſtance of moon-ſhine, than we in theſe parts would eaſily be perſuaded. Though it ſeems not unlikely to me, that the intenſeneſs of the cold may contribute ſomething to the conſiderableneſs of the effect, by much clearing the air of darkiſh ſteams, which in theſe more temperate climates are wont to thicken it in ſnowy weather: for having purpoſely enquired of this doctor, and conſulted that ingenious navigator Captain *James's* voyage hereafter to be further mentioned, I find both their relations agree in this, that in dark froſty nights they could diſcover more ſtars, and ſee the reſt clearer, than we in *England* are wont to do.

6. I know indeed, that divers learned men think, that ſnow ſo ſtrongly affects our eyes, not by a borrowed, but a native light; but I venture to give it as a proof, that white bodies reflect more light than others, becauſe having once purpoſely placed a parcel of ſnow in a room carefully darkened, that no celeſtial light might come to fall upon it, neither I, nor an ingenious perſon (ſkilled in optics) whom I deſired for a witneſs, could find, that it had any other light than what it received. And however, it is uſual among thoſe that travel in dark nights, that the guides wear ſomething of white to be diſcerned by, there being ſcarce any night ſo dark, but that in the free air there remains ſome light, though broken and debilitated perhaps by a thouſand reflections from the opacous corpuſcles that ſwim in the air, and ſend it to one another before it comes to arrive at the eye.

7. Thirdly, And the better to ſhew that white bodies reflect ſtore of light, in compariſon of thoſe that are otherwiſe coloured, I did in the darkened room, formerly mentioned, hold not far from the hole, at which the light was admitted, a ſheet only of white paper, from whence caſting the ſun-beams upon a white wall, whereunto it was obverted, it manifeſtly appeared both to me, and to the perſon I took for a witneſs of the experiment, that it reflected a far greater light, than any of the other colours formerly mentioned; the light ſo thrown upon the wall notably enlightning it, and by it a good part of the room. And yet further to ſhew you, that white bodies reflect the beams from them, and not towards themſelves, let me add, that ordinary burning glaſſes, ſuch as are wont to be employed to light tobacco, will not in a great while burn, or ſo much as diſcolour a ſheet of white paper. Inſomuch that even when I was a boy, and loved to make trials with burning-glaſſes, I could not but wonder at this odd phænomenon, which ſet me very early upon gueſſing at the nature of whiteneſs; eſpecially becauſe I took notice, that the image of the ſun upon a white paper was not ſo well defined (the light ſeeming too diffuſed) as upon black, and becauſe I tried, that blacking over the paper with ink, not only the ink would be quickly dried up, but the paper, that I could not burn before, would be quickly ſet on fire. I have alſo tried, that my expoſing my hand with a thin black glove over it to the warm ſun, it was thereby very quickly and conſiderably more heated, than if I took off the glove, and held my hand naked, or put on it another glove of thin but white leather. And having thus ſhewn you, *Pyrophilus*, that white bodies reflect the moſt light of any, let us now proceed to conſider, what is further to be taken notice of in them, in order to our preſent enquiry.

8. AND fourthly, whereas among the diſpoſitions we attributed to white bodies, we alſo intimated this, that ſuch bodies are apt, like ſpeculums, though but imperfect ones, to reflect the light that falls on them untroubled or unſtained, we ſhall, beſides other particulars to be met with in theſe papers, offer you this in favour of the conjecture; that in the darkened room ſeveral times mentioned in this treatiſe, we tried, that the ſun beams being caſt from a coloured body upon a neighbouring white wall, the determinate colour of the body was from the wall reflected to the eye; whereas we could in divers caſes manifeſtly alter the colour arriving at the eye, by ſubſtituting at a convenient diſtance, a (conveniently) coloured (and gloſſy) body, inſtead of the white wall: as by throwing the beams from a yellow body upon a blue, there would be exhibited a kind of green, as in the experiments about colours is more fully declared.

9. I KNOW not whether I ſhould on this occaſion take notice, that when, as when looking upon the calm and ſmooth ſurface of a river betwixt my eye and the ſun, it appeared to be a natural ſpeculum, wherein that part, which reflected to my eye the entire and defined image of the ſun, and the beams leſs remote from thoſe which exhibited that image, appeared indeed of a great and whitiſh brightneſs, but the reſt comparatively dark enough; if afterwards the ſuperficies chanced to be a little, but not much troubled by a gentle breath of wind, and thereby reduced into a multitude of ſmall and ſmooth ſpeculums, the ſurface of the river would, ſuitably to the doctrine lately delivered, at a diſtance appear very much of kin to white, though it would loſe that brightneſs or whiteneſs upon the return of the ſurface to calmneſs and an uniform level. And I have ſometimes, for trial ſake, brought by a lenticular glaſs the image of a river, ſhined upon by the ſun, into an upper room darkened, and diſtant about a quarter of a mile from the river; by which means the numerous declining ſurfaces of the water appeared ſo contracted, that upon the body, that received the images, the whole river appeared a very white object at two or three paces diſtance. But if we drew near it, this whiteneſs appeared to proceed from an innumerable company of lucid reflections, from the ſeveral gently waved ſuperficies of the water, which looked near at hand like a multitude of very little, but ſhining ſcales of fiſh, of which many did every moment diſappear, and as many were by the ſun, wind and river generated anew. But though this obſervation ſeemed ſufficiently to diſcover, how the appearing whiteneſs in that caſe was produced, yet in ſome other caſes water may have the ſame, though not ſo vivid a colour upon other accounts; for oftentimes it happens, that the ſmooth ſurface of the water does appear bright or whitiſh, by reaſon of the reflection not immediately of the images of the ſun, but of the brightneſs of the ſky; and in ſuch caſes a convenient wind may, where it paſſes along, make the ſurface look black, by cauſing many ſuch furrows and cavities, as may make the inflected ſuperficies of the water reflect the brightneſs of the ſky rather inward than outward. And again, if the wind increaſe into a ſtorm, the water may appear white, eſpecially near the ſhore and the ſhip; namely becauſe the rude agitation breaks it into foam or froth. So much do whiteneſs and blackneſs depend upon the diſpoſition of the ſuperficial parts of a body, to reflect the beams of light inward or outward. But that as white bodies reflect the moſt light of any, ſo their ſuperficial particles are, in the ſenſe newly delivered, of a ſpecular nature; I ſhall now further endeavour to ſhew, both by the making of ſpecular bodies white, and the making of a white body ſpecular.

10. IN the fifth place then, I will inform you, that (not to repeat what *Gaſſendus* obſerves concerning water) I have for curioſity ſake diſtilled quickſilver in a cucurbit,

bit, fitted with a capacious glaſs-head, and obſerved, that when the operation was performed by the degrees of fire requiſite for my purpoſe, there would ſtick to the inſide of the alembic a multitude of little round drops of mercury: and as you know, that mercury is a ſpecular body, ſo each of theſe little drops was a ſmall round looking-glaſs; and a multitude of them lying thick and near one another, they did both in my judgment, and that of thoſe invited to ſee it, make the glaſs they were faſtened to, appear manifeſtly a white body. And yet, as I ſaid, this whiteneſs depended upon the minuteneſs and nearneſs of the little mercurial globuli, the convexity of whoſe ſurfaces fitted them to repreſent in a narrow compaſs a multitude of little lucid images to differingly ſituated beholders. And here let me obſerve a thing, that ſeems much to countenance the notion I have been recommending; namely, that whereas divers parts of the ſky, and eſpecially the milky way, do to the naked eye appear white (as the name itſelf imports) yet the galaxy looked upon through the teleſcope does not ſhew white, but appears to be made up of a vaſt multitude of little ſtars; ſo that a multitude of lucid bodies, if they be ſo ſmall, that they cannot ſingly or apart be diſcerned by the eye, and if they be ſufficiently thick ſet by one another, may by their confuſed beams appear to the eye one white body. And why is it not poſſible, that the like may be done, when a multitude of bright and little corpuſcles being crouded together, are made to ſend together vivid beams to the eye, though they ſhine but, as the planets, by a borrowed light?

11. But to return to our experiments. We may take notice, that the white of an egg, though in part tranſparent, yet by its power of reflecting ſome incident rays of light, is in ſome meaſure a natural ſpeculum, being long agitated with a whiſk or ſpoon, loſes its tranſparency, and becomes very white, by being turned into froth, that is, into an aggregate of numerous ſmall bubbles, whoſe convex ſuperficies fits them to reflect the light every way outwards. And it is worth noting, that when water, for inſtance, is agitated into froth, if the bubbles be great and few, the whiteneſs will be but faint, becauſe the number of ſpecula within a narrow compaſs is but ſmall, and they are not thick ſet enough to reflect ſo many little images or beams of the lucid body, as are requiſite to produce a vigorous ſenſation of whiteneſs. And partly, leſt it ſhould be ſaid, that the whiteneſs of ſuch globulous particles proceeds from the air included in the froth (which to make good, it ſhould be proved that the air itſelf is white); and partly, to illuſtrate the better the notion we have propoſed of whiteneſs, I ſhall add, that I purpoſely made this experiment: I took a quantity of fair water, and put to it, in a clear glaſs phial, a convenient quantity of oil or ſpirit of turpentine, becauſe that liquor will not incorporate with water, and yet is almoſt as clear and colourleſs as it. Theſe being gently ſhaken together, the agitation breaks the oil (which, as I ſaid, is indiſpoſed to mix like wine or milk *per minima* with the water) into a multitude of little globes, which each of them reflecting outwards a lucid image, make the imperfect mixture of the two liquors appear whitiſh; but if by vehemently ſhaking the glaſs, for a competent time, you make a further comminution of the oil into far more numerous and ſmaller globuli, and thereby confound it alſo better with the water, the mixture will appear of a much greater whiteneſs, and almoſt like milk: whereas if the glaſs be a while let alone, the colour will by degrees impair, as the oily globes grow fewer and bigger, and at length will quite vaniſh, leaving both the liquors diſtinct and diaphanous as before. And ſuch a trial hath not ill ſucceeded, when inſtead of the colourleſs oil of turpentine, I took a yellow mixture made of a good proportion of crude turpentine diſſolved in that liquor; and (if I miſ-remember not) it alſo ſucceeded better than one would expect, when I

employed an oil brought by filings of copper, infuſed in it, to a deep green. And this (by the way) may be the reaſon, why oftentimes when the oils of ſome ſpices and of aniſeeds, *&c.* are diſtilled in a limbec with water, the water (as I have ſeveral times obſerved) comes over whitiſh, and will perhaps continue ſo for a good while; becauſe if the fire be made too ſtrong, the ſubtile chymical oil is thereby much agitated and broken, and blended with the water in ſuch numerous and minute globules, as cannot eaſily in a ſhort time emerge to the top of the water, and whilſt they remain in it, make it, for the reaſon newly intimated, look whitiſh. And perhaps upon the ſame ground a cauſe may be rendered, why hot water is obſerved to be uſually more opacous and whitiſh, than the ſame water cold; the agitation turning the more ſpirituous or otherwiſe conveniently diſpoſed particles of the water into vapours, thereby producing in the body of the liquor a multitude of ſmall bubbles, which interrupt the free paſſage, that the beams of light would elſe have every way, and from the innermoſt parts of the water reflect many of them outwards. Theſe and the like examples, *Pyrophilus*, have induced me to ſuſpect, that the ſuperficial particles of white bodies may for the moſt part be as well convex as ſmooth: I content myſelf to ſay, *ſuſpect*, and *for the moſt part*, becauſe it ſeems not eaſy to prove, that when diaphanous bodies, as we ſhall ſee by and by, are reduced into white powders, each corpuſcle muſt needs be of a convex ſuperficies, ſince perhaps it may ſuffice that ſpecular ſurfaces look ſeveral ways. For (as we have ſeen) when a diaphanous body comes to be reduced to very minute parts, it thereby acquires a multitude of little ſurfaces within a narrow compaſs. And though each of theſe ſhould not be of a figure convenient to reflect a round image of the ſun, yet even from ſuch an inconveniently figured body there may be reflected ſome (either ſtreight or crooked) phyſical line of light; which line I call phyſical, becauſe it has ſome breadth in it, and in which line in many caſes ſome refraction of the light falling upon the body it depends on may contribute to the brightneſs: as if a ſlender wire, or ſolid cylinder of glaſs, be expoſed to the light, you ſhall ſee in ſome part of it a vivid line of light; and if we were able to draw out and lay together a multitude of theſe little wires or thrids of glaſs, ſo ſlender, that the eye could not diſcern a diſtance betwixt the luminous lines, there is little doubt (as far as I can gueſs by a trial purpoſely made with very ſlender, but far leſs ſlender thrids of glaſs, whoſe aggregate was looked upon one way white) but the whole phyſical ſuperficies compoſed of them would to the eye appear white; and if ſo, it will not be always neceſſary that the figure of thoſe corpuſcles, that make a body appear white, ſhould be globulous. And as for ſnow itſelf, though the learned *Gaſſendus* (as we have ſeen above) makes it to ſeem nothing elſe but a pure frozen froth, conſiſting of exceedingly minute and thick-ſet bubbles; yet I ſee no neceſſity of admitting that, ſince not only by the variouſly and curiouſly figured ſnow, that I have divers times had the opportunity with pleaſure to obſerve, but alſo by the common ſnow, it rather doth appear both to the naked eye, and in a microſcope, often, if not moſt commonly, to conſiſt principally of little ſlender icicles of ſeveral ſhapes, which afford ſuch numerous lines of light, as we have been newly ſpeaking of.

12. SIXTHLY, If you take a diaphanous body, as for inſtance a piece of glaſs, and reduce it to powder, the ſame body, which, when it was entire, freely tranſmitted the beams of light, acquiring by contuſion a multitude of minute ſurfaces, each of which is as it were a little, but imperfect ſpeculum, is qualified to reflect, in a confuſed manner, ſo many either beams, or little and ſingly unobſervable images of the lucid body, that from a diaphanous it degenerates into a white body. And I remember,

ber, I have for trial's ſake taken lumps of rock cryſtal, and heating them red-hot in a crucible, I found, according to my expectation, that being quenched in fair water, even thoſe, that remained in ſeemingly entire lumps, exchanged their tranſlucency for whiteneſs, the ignition and extinction having as it were cracked each lump into a multitude of minute bodies, and thereby given it a great multitude of new ſurfaces. And even with diaphanous bodies, that are coloured, there may be this way a greater degree of whiteneſs produced, than one would lightly think; as I remember, I have by contuſion obtained whitiſh powders of granates, glaſs of antimony, and emeralds finely beaten; and you may more eaſily make the experiment, by taking good venereal vitriol of a deep blue, and comparing with ſome of the entire cryſtals purpoſely reſerved ſome of the ſubtile powder of the ſame ſalt, which will comparatively exhibit a very conſiderable degree of whiteneſs.

13. Seventhly, And as by a change of poſition in the parts, a body that is not white may be made white; ſo by a ſlight change of the texture of its ſurface, a white body may be deprived of its whiteneſs. For if (as I have tried in goldſmiths ſhops) you take a piece of ſilver, that has been freſhly boiled, as the artificers call it (which is done by firſt bruſhing, and then decocting it with ſalt and tartar, and perhaps ſome other ingredients) you ſhall find it to be of a lovely white. But if you take a piece of ſmooth ſteel, and therewith burniſh a part of it, which may be preſently done, you ſhall find, that part will loſe its whiteneſs, and turn a ſpeculum, looking almoſt every where dark, as other looking-glaſſes do; which may not a little confirm our doctrine. For by this we may gueſs, what it is chiefly that made the body white before, by conſidering that all, that was done to deprive it of that whiteneſs, was only to depreſs the little protuberances, that were before on the ſurface of the ſilver, into one continued ſuperficies, and thereby effect this, that now the image of the lucid body, and conſequently a kind of whiteneſs ſhall appear to your eye; but in ſome place of the greater ſilver looking-glaſs (whence the beams reflected at an angle equal to that wherewith they fall on it, may reach your eye) whilſt the aſperity remained undeſtroyed, the light falling on innumerable little ſpecula obverted ſome one way, and ſome another, did from all ſenſibly diſtinguiſhable parts of the ſuperficies reflect confuſed beams or repreſentations of light to the beholder's eye, from whence ſoever he chance to look upon it. And among the experiments annexed to this diſcourſe, you will find one, wherein, by the change of texture in bodies, whiteneſs is in a trice both generated and deſtroyed.

CHAP. II.

1. WHAT we have diſcourſed of whiteneſs, may ſomewhat aſſiſt us to form a notion of blackneſs, thoſe two qualities being contrary enough to illuſtrate each other. Yet among the antient philoſophers I find leſs aſſiſtance to form a notion of blackneſs than of whiteneſs; only *Democritus* in the paſſage above recited out of *Ariſtotle* has given a general hint of the cauſe of this colour, by referring the blackneſs of bodies to their aſperity. But this I call but a general hint, becauſe thoſe bodies that are green, and purple, and blue, ſeem to be ſo, as well as black ones, upon the account of their ſuperficial aſperity. But among the moderns, the formerly mentioned *Gaſſendus*, perhaps invited by this hint of *Democritus*, has incidentally in another epiſtle given us, though a very ſhort, yet a ſomewhat clearer account of the nature of blackneſs in theſe words: *Exiſtimare par eſt corpora ſuâpte naturâ nigra conſtare ex particulis, quarum ſuperficieculæ ſcabræ ſint, nec facilè lucem extrorſum reflectant.*

I wiſh this ingenious man had enlarged himſelf upon this ſubject; for indeed it ſeems, that as that, which makes a body white, is chiefly ſuch a diſpoſition of its parts, that it reflects (I mean without much interruption) more of the light that falls on it, than bodies of any other colour do; ſo that, which makes a body black, is principally a peculiar kind of texture, chiefly of its ſuperficial particles, whereby it does as it were dead the light that falls on it, ſo that very little is reflected outwards to the eye.

2. AND this texture may be explicated two, and perhaps more than two ſeveral ways; whereof the firſt is by ſuppoſing in the ſuperficies of the black body a particular kind of aſperity, whereby the ſuperficial particles reflect but few of the incident beams outwards, and the reſt inwards towards the body itſelf. As if, for inſtance, we ſhould conceive the ſurface of a black body to be aſperated by an almoſt numberleſs throng of little cylinders, pyramids, cones, and other ſuch corpuſcles, which, by their being thick ſet and erected, reflect the beams of light from one to another inwards, and ſend them to and fro ſo often, that at length they are loſt, before they can come to rebound out again to the eye. And this is the firſt of the two mentioned ways of explicating blackneſs. The other way is by ſuppoſing the texture of black bodies to be ſuch, that either by their yielding to the beams of light, or upon ſome other account, they do as it were dead the beams of light, and keep them from being reflected in any plenty, or with any conſiderable vigour or motion, outwards. According to this notion it may be ſaid, that the corpuſcles, that make up the beams of light, whether they be ſolary effluviums, or minute particles of ſome ætherial ſubſtance, thruſting on one another from the lucid body, do, falling on black bodies, meet with ſuch a texture, that ſuch bodies receive into themſelves, and retain almoſt all the motion communicated to them by the corpuſcles that make up the beams of light, and conſequently reflect but few of them, or thoſe but languidly, towards the eye; it happening here almoſt in like manner as to a ball, which thrown againſt a ſtone or floor would rebound a great way upwards, but rebounds very little or not at all, when it is thrown againſt water, or mud, or a looſe net, becauſe the parts yield, and receive into themſelves the motion, on whoſe account the ball ſhould be reflected outwards. But this laſt way of explicating blackneſs I ſhall content myſelf to have propoſed, without either adopting it, or abſolutely rejecting it. For the hardneſs of touch-ſtones, black marble, and other bodies, that being black are ſolid, ſeem to make it ſomewhat improbable, that ſuch bodies ſhould be of ſo yielding a texture, unleſs we ſhould ſay, that ſome bodies may be more diſpoſed to yield to the impulſes of the corpuſcles of light by reaſon of a peculiar texture, than other bodies, that in other trials appear to be ſofter than they. But though the former of theſe two explications of blackneſs be that, by which we ſhall endeavour to give an account of it; yet, as we ſaid, we ſhall not abſolutely reject this latter, partly becauſe they both agree in this, that black bodies reflect but little of the light that falls on them, and partly becauſe it is not impoſſible, that in ſome caſes both the diſpoſition of the ſuperficial particles, as to figure and poſition, and the yielding of the body, or ſome of its parts, may jointly, though not in an equal meaſure concur to the rendering of a body black. The conſiderations, that induced me to propoſe this notion of blackneſs, as I explained it, are principally theſe:

3. FIRST, That as I lately ſaid, whiteneſs and blackneſs being generally reputed to be contrary qualities, whiteneſs depending, as I ſaid, upon the diſpoſition of the parts of a body to reflect much light, it ſeems likely, that blackneſs may depend upon a contrary diſpoſition of the black bodies ſurface; but upon this I ſhall not inſiſt.

4. Next then we ſee, that if a body of one and the ſame colour be placed, part in the ſun-beams, and part in the ſhade, that part which is not ſhined on will appear more of kin to blackneſs than the other, from which more light rebounds to the eye; and dark colours ſeem the blacker, the leſs light they are looked upon in; and we think all things black in the dark, when they ſend no beams to make impreſſions on our organs of ſight: ſo that ſhadows and darkneſs are near of kin, and ſhadow, we know, is but a privation of light: and accordingly blackneſs ſeems to proceed from the paucity of beams reflected from the black body to the eye; I ſay, the paucity of beams, becauſe thoſe bodies, that we call black, as marble, jett, *&c.* are ſhort of being perfectly ſo, elſe we ſhould not ſee them at all. But though the beams, that fall on the ſides of thoſe erected particles, that we have been mentioning, do few of them return outwards, yet thoſe, that fall upon the points of thoſe cylinders, cones, or pyramids, may thence rebound to the eye, though they make there but a faint impreſſion, becauſe they arrive not there, but mingled with a great proportion of little ſhades. This may be confirmed by my having procured a large piece of black marble well poliſhed, and brought to the form of a large ſpherical and concave ſpeculum; for on the inſide this marble being well poliſhed, was a kind of dark looking-glaſs, wherein I could plainly ſee a little image of the ſun, when that ſhined upon it. But this image was very far from offending and dazzling my eyes, as it would have done from another ſpeculum; nor, though the ſpeculum were large, could I in a long time, or in a hot ſun, ſet a piece of wood on fire, though a far leſs ſpeculum of the ſame form, and of a more reflecting matter, would have made it flame in a trice.

5. And on this occaſion we may as well in reference to ſomething formerly delivered concerning whiteneſs, as in reference to what has been newly ſaid, ſubjoin what we further obſerved touching the differing reflections of light from white and black marble; namely, that having taken a pretty large mortar of white marble, new, and poliſhed in the inſide, and expoſed it to the ſun, we found, that it reflected a great deal of glaring light, but ſo diſperſed, that we could not make the reflected beams concur in any ſuch conſpicuous focus, as that newly taken notice of in the black marble; though perhaps there may enough of them be made to meet near the bottom, to make ſome kind of focus, eſpecially ſince by holding in the night-time a candle at a convenient diſtance, we were able to procure a concourſe of ſome, though not many of the reflected beams, at about two inches diſtant from the bottom of the mortar: but we found the heat even of the ſun-beams ſo diſperſedly reflected to be very languid, even in compariſon of the black marble's focus. And the little picture of the ſun, that appeared upon the white marble as a ſpeculum, was but very faint and exceeding ill defined. Secondly, that taking two pieces of plain and poliſhed ſurfaces, and caſting on them ſucceſſively the beams of the ſame candle, in ſuch manner, as that the neighbouring ſuperficies being ſhaded by an opacous and perforated body, the incident beams were permitted to paſs but through a round hole of about half an inch diameter, the circle of light, that appeared on the white marble, was in compariſon very bright, but very ill defined; whereas that on the black marble was far leſs luminous, but much more preciſely defined.

6. Thirdly, when you look upon a piece of linen, that has ſmall holes in it, thoſe holes appear very black, and men are often deceived in taking holes for ſpots of ink; and painters, to repreſent holes, make uſe of black; the reaſon of which ſeems to be, that the beams, that fall on thoſe holes, fall into them ſo deep, that none of them is reflected back to the eye. And in narrow wells part of the mouth ſeems

black, becauſe the incident beams are reflected downwards from one ſide to another, till they can no more rebound to the eye.

We may conſider too, that if differing parts of the ſame piece of black velvet be ſtroaked oppoſite ways, the piece of velvet will appear of two diſtinct kinds of blackneſs, the one far darker than the other; of which diſparity the reaſon ſeems to be, that in the leſs obſcure part of the velvet, the little ſilken piles, whereof it is made up, being inclined, there is a greater part of each of them obverted to the eye; whereas in the other part the piles of ſilk being more erected, there are far fewer beams reflected outwards from the lateral parts of each pile; ſo that moſt of thoſe, that rebound to the eye, come from the tops of the piles, which make but a ſmall part of the whole ſuperficies, that may be covered by the piece of velvet. Which explication I propoſe, not that I think the blackneſs of the velvet proceeds from the cauſe aſſigned, ſince each ſingle pile of ſilk is black by reaſon of its texture, in what poſition ſoever you look upon it; but that the greater blackneſs of one of theſe tufts ſeems to proceed from the greater paucity of beams reflected from it, and that from the fewneſs of thoſe parts of a ſurface, that reflect beams, and the multitude of thoſe ſhaded parts, that reflect none. And I remember, that I have oftentimes obſerved, that the poſition of particular bodies far greater than piles of ſilk in reference to the eye, may, notwithſtanding their having each of them a colour of its own, make one part of their aggregate appear far darker than the other; for I have near great towns often take notice, that a cart-load of carrots packed up appeared of a much darker colour when looked upon, where the points of the carrots were obverted to the eye, than where the ſides of them were ſo.

7. Fourthly, in a darkened room, I purpoſely obſerved, that if the ſun-beams, which came in at the hole, were received upon white or any other colour, and directed to a convenient place of the room, they would manifeſtly, though not all equally, increaſe the light of that part; whereas if we ſubſtituted, either a piece of black cloth or black velvet, it would ſo dead the incident beams, that the place (newly mentioned) whereto I obverted the black body, would be leſs enlightened than it was before, when it received its light but from the weak and oblique reflections of the floor and walls of a pretty large room, through which the beams, that came in at the hole, were confuſedly and brokenly diſperſed.

8. Fifthly, And to ſhew, that the beams, that fall on black bodies, as they do not rebound outwards to the eye, ſo they are reflected towards the body itſelf, as the nature of thoſe erected particles, to which we have imputed blackneſs, requires, we will add an experiment, that will alſo confirm our doctrine touching whiteneſs; namely, that we took a broad and large tile, and having whitened over one half of the ſuperficies of it, and blacked the other, we expoſed it to the ſummer's ſun; and having let it lie there a convenient time (for the difference is more apparent, if it have not lain there too long) we found, as we expected, that whilſt the whited part of the tile remained cool enough, the blacked part of the ſame tile was grown not only ſenſible, but very hot (ſometimes to a ſtrong degree.) And to ſatisfy ſome of our friends the more, we have ſometimes left upon the ſurface of the tile, beſides the white and black parts thereof, a part, that retained the native red of the tile itſelf; and expoſing them to the ſun, we obſerved this laſt mentioned to have contracted a heat in compariſon of the white, but a heat inferior to that of the black; of which the reaſon ſeems to be, that the ſuperficial particles of black bodies, being, as we ſaid, more erected, than thoſe of white or red ones, the corpuſcles of light falling on their ſides, being for the moſt part reflected inward from one particle to another, and

thereby engaged as it were, and kept from rebounding upwards, they communicate their briſk motion, wherewith they were impelled againſt the black body (upon whoſe account, had they fallen upon a white body, they would have been reflected outwards) to the ſmall parts of the black body, and thereby produce in thoſe ſmall parts ſuch an agitation, as (when we feel it) we are wont to call heat. I have been lately informed, that an obſervation near of kin to ours has been made by ſome learned men in *France* and *Italy*, by long expoſing to a very hot ſun two pieces of marble, the one white and the other black. But though the obſervation be worthy of them, and may confirm the ſame truth with our experiment, yet beſides that our trial needs not the ſummer, nor any great heat to ſucceed, it ſeems to have this advantage above the other, that whereas bodies more ſolid, and of a cloſer texture, though they uſe to be more ſlowly heated, are wont to receive a greater degree of heat from the ſun or fire, than (*cæteris paribus*) bodies of a ſlighter texture. I have found by the information of ſtone-cutters, and by other ways of enquiry, that black marble is much ſolider and harder than white; ſo that poſſibly the difference betwixt the degrees of heat, they receive from the ſun-beams, will by many be aſcribed to the difference of their texture, rather than to that of their colour; though I think our experiment will make it probable enough, that the greater part of that difference may well be aſcribed to that diſpoſition of parts, which makes the one reflect the ſun-beams inward, and the other outwards. And with this doctrine accords very well, that rooms hung with black are not only darker than elſe they would be, but are wont to be warmer too; inſomuch that I have known a great lady, whoſe conſtitution whas ſomewhat tender, complain, that ſhe was wont to catch cold, when ſhe went out into the air, after having made any long viſits to perſons, whoſe rooms were hung with black. And this is not the only lady I have heard complain of the warmth of ſuch rooms; which though perhaps it may be partly imputed to the effluvia of thoſe materials, wherewith the hangings were dyed, yet probably the warmth of ſuch rooms depends chiefly upon the ſame cauſe, that the darkneſs does; as (not to repeat what I formerly noted touching my gloves) to ſatisfy ſome curious perſons of that ſex, I have convinced them, by trial, that of two pieces of ſilken ſtuff given to me by themſelves, and expoſed in their preſence to the ſame window, ſhined on by the ſun, the white was conſiderably heated, when the black was not ſo much as ſenſibly ſo.

9. Sixthly, I remember, that acquainting one day a Virtuoſo of unſuſpected credit, that had viſited hot countries, with part of what I have here delivered concerning blackneſs, he related to me, by way of confirmation of it, a very notable experiment, which he had both ſeen others make, and made himſelf in a warm climate; namely, that having carefully blacked over eggs, and expoſed them to the hot ſun, they were thereby in no very long time well roaſted; to which effect I conceive the heat of the climate muſt have concurred with the diſpoſition of the black ſurface to reflect the ſun-beams inward: for I remember, that having made that among other trials in *England*, though in ſummer-time, the eggs I expoſed acquired indeed a conſiderable degree of heat, but yet not ſo intenſe a one, as proved ſufficient to roaſt them.

10. Seventhly, and laſtly, our conjectures at the nature of blackneſs may be ſomewhat confirmed by the (formerly mentioned obſervation of the blind *Dutchman*, that diſcerns colours with his fingers; for he ſays, that he feels a greater roughneſs upon the ſurfaces of black bodies, than upon thoſe of red, or yellow, or green. And I remember, that the diligent *Bartholinus* ſays, that a blind Earl of *Mansfield* could diſtinguiſh white from black only by the touch; which would ſufficiently argue a great

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diſparity in the aſperities, or other ſuperficial textures of bodies of thoſe two colours, if the learned relator had affirmed the matter upon his own knowledge.

11. THESE, *Pyrophilus*, are the chief things, that occur to me at preſent, about the nature of whiteneſs and blackneſs; which if they have rendered it ſo much as probable, that in moſt, or at leaſt many caſes, the cauſes of theſe qualities may be ſuch as I have adventured to deliver, it is as much as I pretend to. For till I have opportunity to examine the matter by ſome further trials, I am not ſure, but that in ſome white and black bodies, there may concur to the colour ſome peculiar texture or diſpoſition of the body, whereby the motion of the ſmall corpuſcles, that make up the incident beams of light, may be differingly modified, before they reach the eye; eſpecially in this, that white bodies do not only copiouſly reflect thoſe incident corpuſcles outwards, but reflect them briſkly, and do not otherwiſe alter them in the manner of their motion. Nor ſhall I now ſtay to inquire, whether ſome of thoſe other ways (as a diſpoſition to alter the velocity, the rotation, or the order and manner of appulſe to the eye of the reflected corpuſcles, that compoſed the incident beams of light) which we mentioned, when we conſidered the production of colours in general, may not in ſome caſes be applicable to thoſe of white and black bodies: for I am yet ſo much a ſeeker in this matter, and ſo little wedded to the opinions I have propoſed, that what I am to add, ſhall be but the beginning of a collection of experiments and obſervations towards the hiſtory of whiteneſs and blackneſs, without at preſent interpoſing my explications of them; that ſo I may aſſiſt your enquiries, without much foreſtalling or byaſſing your judgment.

EXPERIMENT in CONSORT,

Touching WHITENESS *and* BLACKNESS.

EXPERIMENT I.

HAVING promiſed in the 27th page of the foregoing diſcourſe of whiteneſs and blackneſs, to ſhew, that thoſe two colours may, by a change of texture in bodies, each of them apart diaphanous and colourleſs, be at pleaſure and in a trice as well generated as deſtroyed, we ſhall begin with experiments, that may acquit us of that promiſe.

TAKE then what quantity you pleaſe of fair water, and having heated it, put into it as much good common ſublimate, as it is able to diſſolve, and (to be ſure of having it well glutted) continue putting in the ſublimate, till ſome of it lie untouched in the bottom of the liquor. Filter this ſolution through cap-paper, to have it clear and limpid, and into a ſpoonful or two thereof (put into a clean glaſs-veſſel) ſhake about four or five drops (according as you took more or leſs of this ſolution) of good limpid ſpirits of urine, and immediately the whole mixture will appear white like milk; to which mixture if you preſently add a convenient proportion of rectified aqua fortis (for the number of drops is hard to determine, becauſe of the differing ſtrength of the liquor, but eaſily found by trial) the whiteneſs will preſently diſappear, and the whole mixture become tranſparent; which you may, if you pleaſe, again reduce to a good degree of whiteneſs (though inferior to the firſt) only by a more copious affuſion of freſh ſpirit of urine. *N. B.* Firſt, that it is not ſo neceſſary to employ either aqua fortis or ſpirit of urine about this experiment, but that we have made it with other liquors inſtead of theſe; of which perhaps more elſewhere. Secondly, that this experiment, though not made with the ſame menſtruums, nor producing the ſame colour, is yet much of kin to that other to be mentioned in this tract, among our other experiments of colours, about turning a ſolution of precipitate into an orange colour; and the chymical reaſon being much alike in both, the annexing it to one of them may ſuffice for both.

EXPERIMENT II.

MAKE a ſtrong infuſion of broken galls in fair water; and having filtered it into a clean phial, add more of the ſame liquor to it, till you have made it ſomewhat tranſparent, and ſufficiently diluted the colour, for the credit of the experiment, leſt otherwiſe the darkneſs of the liquor might make it be objected, that it was already almoſt ink. Into this infuſion ſhake a convenient quantity of a clear, but very ſtrong ſolution of vitriol; and you ſhall immediately ſee the mixture turn black almoſt like ink, and ſuch a way of producing blackneſs is vulgar enough; but if preſently after you do upon this mixture drop a ſmall quantity of good oil of vitriol, and, by ſhaking the phial, diſperſe it nimbly through the two other liquors, you ſhall (if you perform your part well, and have employed oil of vitriol clear and ſtrong enough) ſee the darkneſs of the liquor preſently begin to be diſcuſſed, and grow pretty clear and tranſparent, loſing its inky blackneſs, which you may again reſtore to it by the affuſion of a ſmall quantity of a very ſtrong ſolution of ſalt of tartar. And though neither of theſe atramentous liquors will ſeem other than very pale ink, if you write with a clean pen dipt in them; yet that is common to them with ſome ſorts of ink, that prove very good when dry; as I have alſo found, that when I made theſe carefully, what I wrote with either of them, eſpecially with the former, would, when thoroughly dry, grow black enough not to appear bad ink. This experiment of taking away, and reſtoring blackneſs from and to the liquors, we have likewiſe tried in common ink; but there it ſucceeds not ſo well, and but very ſlowly, by reaſon that the gum wont to be employed in the making it does by its tenacity oppoſe the operations of the above-mentioned ſaline liquors. But to conſider gum no more, what ſome kind of precipitation may have to do in the producing and deſtroying the inks without it, I have elſewhere given you ſome occaſion and aſſiſtance to inquire: but I muſt not now ſtay to do ſo myſelf, only I ſhall take notice to you, that though it be taken for granted, that bodies will not be precipitated by alcalizate ſalts, that have not firſt been diſſolved in ſome acid menſtruums; yet I have found upon trials, which my conjectures led me to make on purpoſe, that divers vegetables, barely infuſed, or, but ſlightly decocted in common water, would, upon the affuſion of a ſtrong and clear lixivium of pot-aſhes, and much more of ſome other precipitating liquors that I ſometimes employ, afford good ſtore of a curdled matter, ſuch as I have had in the precipitations of vegetable ſubſtances, by the intervention of acid things; and that this matter was eaſily ſeparable from the reſt of the liquor, being left behind it in the filtre. And in making the firſt ink mentioned in this experiment, I found, that I could by filtration ſeparate pretty ſtore of a black pulverable ſubſtance, that remained in the filtre; and when the ink was made clear again by the oil of vitriol, the affuſion of diſſolved *ſal tartari* ſeemed but to precipitate, and thereby to unite and render conſpicuous the particles of the black mixture, that had before been diſperſed into very minute and ſingly inviſible particles by the inciſive and reſolving power of the highly corroſive oil of vitriol.

AND to manifeſt, *Pyrophilus,* that galls are not ſo requiſite as many ſuppoſe to the making atramentous liquors, we have ſometimes tried the following experiment: we took dried roſe-leaves, and decocted them for a while in fair water; into two or three ſpoonfuls of this decoction we ſhook a few drops of a ſtrong and well filtrated ſolution of vitriol (which perhaps, had it been green, would have done as well) and immediately the mixture did turn black, and when into this mixture, preſently after it was made, we ſhook a juſt proportion of aqua fortis, we turned it from a black ink to a deep red one, which by the affuſion of a little ſpirit of urine may be reduced immediately to an opacous and blackiſh colour. And in regard, *Pyrophilus,* that in the former experiments, both the infuſion of galls, and the decoction of roſes, and the ſolution of copperas, employed about them, are endowed each of them with its own colour, there may be a more noble experiment of the ſudden production of blackneſs made by the way mentioned in the ſecond ſection of the ſecond part of our eſſays; for though upon the confuſion of the liquors there mentioned, there do immediately emerge a very black mixture; yet both the infuſion of orpiment and the ſolution of minium were, before their being joined together, limpid and colourleſs.

EXPERIMENT III.

IF pieces of white hartſhorn be with a competent degree of fire diſtilled in a glaſs-retort, they will, after the avolation of the phlegm, ſpirit, volatile ſalt, and the looſer and the lighter parts of the oleaginous ſubſtance, remain behind of a coal-black colour. And even ivory itſelf being ſkilfully burnt (how I am wont to do it, I have elſewhere ſet down) affords painters one of the beſt and deepeſt blacks they have. And yet in the inſtance of diſtilled hartſhorn, the operation being made in glaſs-veſſels carefully cloſed, it appears there is no extraneous black ſubſtance, that inſinuates itſelf into white harſhorn, and thereby makes it turn black; but that the whiteneſs is deſtroyed, and the blackneſs generated, only by a change of texture, made in the burnt body, by the receſs of ſome parts, and the tranſpoſition of others. And though I remember not, that in many diſtillations of hartſhorn I ever found the *Cap. mort.* to paſs from black to a true whiteneſs, whilſt it continued in cloſed veſſels; yet having taken out the coal-black fragments, and calcined them in open veſſels, I could in few hours quite deſtroy that blackneſs, and without ſenſibly changing their bulk or figure, reduce them to great whiteneſs. So much do theſe two colours depend upon the diſpoſition of the little parts, that the bodies, wherein they are to be met with, do conſiſt of. And we find, that if white wine-tartar, or even the white cryſtals of ſuch tartar be burnt without being truly calcined, the *Cap. mortuum* (as the chymiſts call the more fixt part) will be black. But if you further continue the calcination, till you have perfectly incinerated the tartar, and kept it long enough in a ſtrong fire, the remaining calx will be white. And ſo we ſee, that not only other vegetable ſubſtances, but even white woods, as the hazel, will yield a black charcoal, and afterwards whitiſh aſhes; and ſo animal ſubſtances naturally white, as bones and egg ſhells, will grow black upon the being burnt, and white again, when they are perfectly calcined.

EXPERIMENT IV.

BUT yet I much queſtion, whether that rule delivered by divers, as well philoſophers as chymiſts, *aduſta nigra, ſed peruſta alba*, will hold as univerſally as is preſumed, ſince I have ſeveral examples to allege againſt it. For I have found, that by burning alabaſter, ſo as both to make it appear to boil almoſt like milk, and to reduce it to a very fine powder, it would not at all grow black, but retain its pure and native whiteneſs; and though by keeping it longer than is uſual in the fire, I produced but a faint yellow, even in that part of the powder, that lay neareſt the top of the crucible; yet having purpoſely inquired of an experienced ſtone-cutter, who is curious enough in trying concluſions in his own trade, he told me he had found, that if alabaſter or plaiſter of *Paris* be very long kept in a ſtrong fire, the whole heap of burnt powder would exchange its whiteneſs for a much deeper colour than the yellow I obſerved. Lead being calcined with a ſtrong fire turns (after having perhaps run through divers other colours) into minium, whoſe colour we know is a deep red; and if you urge this minium, as I have purpoſely done with a ſtrong fire, you may much eaſier find a glaſſy and brittle body darker than minium, than any white calx or glaſs. It is known among chymiſts, that the white calx of antimony, by the further and more vehement operation of the fire, may be melted into glaſs, which we have obtained of a red colour, which is far deeper than that of the calx of burnt antimony. And though common glaſs of antimony being uſually adulterated with borax, have its colour thereby diluted, oftentimes to a very pale yellow; yet not only ours made more ſincerely was, as we ſaid, of a colour leſs remote from black, than was the calx; but we obſerved, that by melting it once or twice more, and ſo expoſing it to the further operation of the fire, we had, as we expected, the colour heightened. To which we ſhall add but this one inſtance (which is worth the taking notice of, in reference to colours) that, if you take blue, but unſophiſticated, vitriol, and burn it very ſlowly, and with a gentle degree of heat, you may obſerve, that when it is burnt but a little, and yet ſo far as that you may rub it to powder betwixt your fingers, it will be of a white or whitiſh colour; but if you proſecute the calcination, this body, which by a light aduſtion was made white, will paſs through other colours, as gray, yellowiſh, and red; and if you further burn it with a long and vehement fire, by that time it comes to be *peruſtum*, it will be of a dark purple, nearer to black, not only than the firſt calx, but than the vitriol before it at all felt the fire. I might add, that *Crocus Martis (per ſe*, as they call it) made by the laſting violence of the reverberated flames is not ſo near a-kin to white, as the iron or ſteel that afforded it was before its calcinations; but that I ſuppoſe, theſe inſtances may ſuffice to ſatisfy you, that minerals are to be excepted out of the forementioned rule, which perhaps, though it ſeldom fail in ſubſtances belonging to the vegetable or animal kingdom, may yet be queſtioned even in ſome of theſe, if that be true, which the judicious traveller *Bellonius* affirms, that charcoals made out of the wood of oxycedar are white: and I could not find, that though in retorts hart's-horn and other white bodies will be denigrated by heat, yet camphire would not at all loſe its whiteneſs, though I have purpoſely kept it in ſuch a heat, as made it melt and boil.

EXPERIMENT V.

AND now I ſpeak of camphire, it puts me in mind of adding this experiment, that though, as I ſaid, in cloſed glaſſes I could not denigrate it by heat, but it would ſublime to the ſides and top of the glaſs, as it was before; yet not only it will, being ſet on fire in the free air, ſend forth a copious ſmoke, but having purpoſely upon ſome of it that was flaming, clapt a large glaſs, almoſt in the form of a hive (but more ſlender only) with a hole at the top (which I cauſed to be made to try experiments of fire and flame in) it continued ſo long burning, that it lined all the inſide of the glaſs with a ſoot as black as ink, and ſo copious, that, the cloſeneſs of the veſſel conſidered, almoſt all that part of the white camphire, that did take fire, ſeemed to have been changed into that deep black ſubſtance.

EXPERIMENT VI.

AND this alſo brings into my mind another experiment, that I made about the production of blackneſs, whereof, for reaſons too long to be here deduced, I expected and found a good ſucceſs; and it was this: I took rectified oil of vitriol (that I might have the liquor clean as well as ſtrong) and by degrees mixt with it a convenient proportion of the eſſential oil, as chymiſts call it, of wormwood, drawn over with ſtore of water in a limbec; and warily diſtilling the mixture in a retort, there remained a ſcarce credible quantity of dry matter, black as a coal. And becauſe the oil of wormwood, though a chymical oil drawn by a Virtuoſo, ſeemed to have ſomewhat in it of the colour of the plant, I ſubſtituted in its room the pure and ſubtile eſſential oil of winter-ſavory, and mixing little by little this liquor with (if I miſ-remember not) an equal weight of the formerly mentioned rectified oil of vitriol, and diſtilling them as before in a retort, beſides what there paſſed over into the receiver, even theſe two clear liquors left me a conſiderable proportion (though not ſo great as the two former) of a ſubſtance black as pitch, which I yet keep by me as a rarity.

EXPERIMENT VII.

A WAY of whiting wax cheaply and in great quantity may be a thing of good œconomical uſe; and we have elſewhere ſet down the practice of tradeſmen that blanch it; but here treating of whiteneſs only, in order to the philoſophy of colours, I ſhall not examine, which of the ſlow ways may be beſt employed, to free wax from the yellow melleous parts, but ſhall rather ſet down a quick way of making it white, though but in very ſmall quantities. Take then a little yellow wax, ſcraped or thinly ſliced, and putting it into a bolt's-head or ſome other convenient glaſs, pour to it a pretty deal of ſpirit of wine, and placing the veſſel in warm ſand, increaſe the heat by degrees, till the ſpirit of wine begin to ſimmer or to boil a little; and continuing that degree of fire, if you have put liquor enough, you will quickly have the wax diſſolved: then taking it off the fire, you may either ſuffer it to cool as haſtily as with ſafety to the glaſs you can, or pour it, whilſt it is yet hot, into a filtre of paper; and either in the glaſs where it cools, or in the filtre, you will ſoon find the wax and menſtruum together reduced into a white ſubſtance, almoſt like butter, which by letting the ſpirit exhale will ſhrink into a much leſſer bulk, but ſtill retaining its whiteneſs. And that, which is pretty in the working of this magiſtery of wax, is, that the yellowneſs vaniſhes, neither appearing in the ſpirit of wine, that paſſes limpid through the filtre, nor in the butter of wax, if I may ſo call it, that, as I ſaid, is white.

EXPERIMENT VIII.

THERE is an experiment, *Pyrophilus*, which though I do not ſo exactly remember; and though it be ſomewhat nice to make, yet I am willing to acquaint you with, becauſe the thing produced, though it be but a curioſity, is wont not a little to pleaſe the beholders; and it is a way of turning, by the help of a dry ſubſtance, an almoſt golden-coloured concrete into a white one. The ſeveral trials are not at preſent ſo freſh in my memory to enable me to tell you certainly, whether an equal only or a double weight of common ſublimate muſt be taken in reference to the tin-glaſs; but, if I miſtake not, there was in the experiment, that ſucceeded beſt, two parts of the former taken to one of the latter. Theſe ingredients being finely powdered and exactly mixed, we ſublimed together by degrees of fire (the due gradation of which is in this experiment a thing of main importance;) there aſcended a matter of a very peculiar texture; for it was for the moſt part made up of very thin, ſmooth, ſoft and ſlippery plates, almoſt like the fineſt ſort of ſcales of fiſhes, but of ſo lovely a white inclining to pearl-colour, and of ſo curious a colour and ſhining a gloſs, that they appeared in ſome reſpect little inferior to orient pearls, and in other regards, they ſeemed to ſurpaſs them, and were applauded for a ſort of the pretrieſt trifles, that we had ever prepared to amuſe the eye. I will not undertake, that though you will hardly miſs changing the colour of your ſhining tin-glaſs, yet you will the firſt or perhaps the ſecond time hit right upon the way of making the gliſtering ſublimate I have been mentioning.

EXPERIMENT IX.

WHEN we diſſolve in aqua fortis a mixture of gold and ſilver melted into one lump, it uſually happens, that the powder of gold, that falls to the bottom, as not being diſſoluble by that menſtruum, will not have its own yellow, but appear of a black colour, though neither the gold, nor the ſilver, nor the aqua fortis did before manifeſt any blackneſs. And divers alchymiſts, when they make ſolutions of minerals they would examine, are very glad, if they ſee a black powder precipitated to the bottom, taking it for a hopeful ſign, that thoſe particles are of a golden nature, which appear in a colour ſo ordinary to gold parted from other metals by aqua fortis, that it is a trouble to the refiner to reduce the precipitated calx to its native colour. For though (as we have tried) that may be quickly enough done by fire, which will make this gold look very glorioully (as indeed it is at leaſt one of the beſt ways, that is practiſed for the refining of gold) yet it requires both watchfulneſs and ſkill, to give it ſuch a degree of fire, as will ſerve to reſtore it to its luſtre, without giving it ſuch a one, as may bring it to fuſion, to which the minuteneſs of the corpuſcles it conſiſts of makes the powder very apt. And this brings into my mind, that having taken a flat and bright piece of gold, that was refined by a curious and ſkilful perſon on purpoſe to try to what height of purity gold could be brought by art, I found that this very piece, as glorious as it looked, being rubbed a little upon a piece of fine clean linen, did ſully it with a kind of black: and the like I have obſerved in refined ſilver, which I therefore mention, becauſe I formerly ſuſpected, that the impurity of the metal might have been the only cauſe of what I have divers times obſerved in wearing ſilver-hilted ſwords, namely, that where they rubbed upon my clothes, if they were of a light-coloured cloth, the affriction would quickly black them; and congruouſly hereunto I have found pens blacked almoſt all over, when I had a while carried them about me in a ſilver ink-caſe. To which I ſhall only add, that whereas in theſe ſeveral inſtances of denigration, the metals are worn off, or otherwiſe reduced into very minute parts, that circumſtance may prove not unworthy your notice.

EXPERIMENT X.

THAT a ſolution of ſilver does dye hair of a black colour, is a known experiment, which ſome perſons, more curious than dexterous, have ſo unluckily made upon themſelves, as to make their friends very merry. And I remember, that the other day I made myſelf ſome ſport by an improvement of this obſervation; for having diſſolved ſome pure ſilver in aqua fortis, and evaporated the *menſtruum ad ſiccitatem*, as they ſpeak, I cauſed a quantity of fair water to be poured upon the calx two or three ſeveral times, and to be at each evaporated, till the calx was very dry, and all the greeniſh blueneſs, that is wont to appear in common cryſtals of ſilver, was quite carried away. Then I made thoſe I meant to deceive, moiſten ſome part of their ſkin with their own ſpittle, and ſlightly rub the moiſtened parts with a little of this prepared ſilver; whereupon they admired to ſee, that a ſnow-white body laid upon the white ſkin ſhould preſently produce a deep blackneſs, as if the ſtains had been made with ink; eſpecially conſidering, that this blackneſs could not, like that produced by ordinary ink, be readily waſhed off, but required many hours, and part of it ſome days to its obliteration. And with the ſame white calx and a little fair water we likewiſe ſtained the white hafts of knives, with a laſting black in thoſe parts, where the calx was plentifully enough laid on; for where it was laid on but very thinly, the ſtain was not quite of ſo deep a colour.

EXPERIMENT XI.

THE cauſe of the blackneſs of thoſe many nations, which by one common name we are wont to call Negroes, has been long ſince diſputed of by learned men, who poſſibly had not done amiſs, if they had alſo taken into conſideration, why ſome whole races of other animals beſides men, as foxes and hares, are diſtinguiſhed by a blackneſs not familiar to the generality of animals of the ſame ſpecies. The general opinion (to be mentioned a little lower) has been rejected even by ſome of the antient geographers, and among the moderns *Ortelius* and divers other learned men have queſtioned it. But this is no place to mention what thoughts I have had to and fro about theſe matters: only as I ſhall freely acknowledge, that to me the inquiry ſeems more abſtruſe than it does to many others, and that becauſe conſulting with authors, and with books of voyages, and with travellers, to ſatisfy myſelf in matters of fact, I have met with ſome things among them, which ſeem not to agree very well with the notions of the moſt claſſic authors concerning theſe things; for it being my preſent work to deliver rather matters hiſtorical than theories, I ſhall annex ſome few of my collections, inſtead of a ſolemn diſputation. It is commonly preſumed, that the heat of the climates, wherein they live, is the reaſon, why ſo many inhabitants of the ſcorching regions of *Africa* are black; and there is this familiar obſervation to countenance this conjecture, that we plainly ſee that mowers, reapers, and other country-people, who ſpend the moſt part of the hot ſummer days expoſed to the ſun, have the ſkin of their hands and faces, which are the parts immediately expoſed to the ſun and air, made of a darker colour than before, and conſequently tending to blackneſs: and contrariwiſe we obſerve, that the Danes and ſome other people that inhabit cold climates, and even the Engliſh who feel not ſo rigorous a cold, have uſually whiter faces than the Spaniards, Portugals and other European inhabitants of hotter climates. But this argument I take to be far more ſpecious than convincing; for though the heat of the ſun may darken the colour of the ſkin by that operation, which we in Engliſh call ſun-burning; yet experience doth not evince, that I remember, that that heat alone can produce a diſcolouring, that ſhall amount to a true blackneſs, like that of Negroes; and we ſhall ſee by and by, that even the children of the Negroes not yet ten days old (perhaps not ſo much by three quarters of that time) will notwithſtanding their infancy be of the ſame hue with their parents. Beſides, there is a ſtrong argument to be alleged againſt the vulgar opinion, that in divers places in *Aſia* under the ſame parallel, or even of the ſame degree of latitude with the African regions inhabited by the Blacks, the people are at moſt but tawny; and in *Africa* itſelf divers nations in the empire of *Ethiopia* are not Negroes, though ſituated in the torrid zone, and as near the equinoctial, as other nations that are ſo (as the black inhabitants of *Zeylan* and *Malabar* are not in our globes placed ſo near the line as *Amara* the famouſeſt place in *Ethiopia*). Moreover (that which is of no ſmall moment in our preſent diſquiſition) I find not by the beſt navigators and travellers to the *Weſt-Indies*, whoſe books or themſelves I have conſulted on this ſubject, that excepting perhaps one place or two of ſmall extent, there are any Blacks originally natives of any part of *America* (for the Blacks now there have been by the Europeans long tranſplanted thither) though the new world contain in it ſo great a variety of climates, and particularly reaches quite croſs the torrid zone from one tropic to another. And though it be true, that the Danes be a whiter people than the Spaniards, yet that may proceed rather from other cauſes (not here to be enquired into) than from the coldneſs of the climate, ſince not only the Swedes and other inhabitants of thoſe cold countries, are not uſually ſo white as the Danes, nor whiter than other nations in proportion to their vicinity to the pole. [And ſince the writing of the former part of this eſſay, having an opportunity on a ſolemn occaſion to take notice of the numerous train of ſome extraordinary ambaſſadors ſent from the Ruſſian emperor to a great monarch, I obſerved, that (though it were then winter) the colour of their hair and ſkin was far leſs whitiſh than the Danes who inhabit a milder region is wont to be, but rather from the moſt part of a darkiſh brown; and the phyſician to the ambaſſador, with whom thoſe Ruſſes came, being aſked by me, whether in *Muſcovy* itſelf the generality of the people were more inclined to have dark-coloured hair than flaxen, he anſwered affirmatively; but ſeemed to ſuſpect, that the true and antient Ruſſians, a ſept of whom he told me he had met with in one of the provinces of that vaſt empire, were rather white like the Danes than any thing near ſo brown as the preſent Muſcovites, whom he gueſſes to be deſcended of the Tartars, and to have inherited their colour from them.] But to proſecute our former diſcourſe, I ſhall add for further proof the conjecture I was countenancing, that good authors inform us, that there are Negroes in *Africa* not far from the *Cape of Good Hope*, and conſequently beyond the ſouthern tropic, and without the torrid zone: much about the ſame nor-

Olearius Voyage de Moſco & de Perſe liv. 3.

thern latitude (or very little more) wherein there are divers American nations, that are not Negroes, and wherein the inhabitants of *Candia*, ſome parts of *Sicily*, and even of *Spain*, are not ſo much as Tawny-moors. But (which is a freſh and ſtrong argument againſt the common opinion) I find by our recent relations of *Greenland* (our accounts whereof we owe to the curioſity of that royal Virtuoſo the preſent King of *Denmark)* that the inhabitants are olive-coloured, or rather of a darker hue. But if the caſe were the ſame with men, and thoſe other kinds of animals I formerly named, I ſhould offer ſomething as a conſiderable proof, that cold may do much towards the making men white or black; and however I ſhall ſet down the obſervation as I have met with it, as worthy to come into the hiſtory of whiteneſs and blackneſs; and it is, that in ſome parts of *Ruſſia* and of *Livonia* it is affirmed by *Olaus Magnus* and others, that hares and foxes (ſome add partridges) which before were black, or red, or grey, do in the depth of winter become white by reaſon of the great cold; (for that it ſhould be, as ſome conceive, by looking upon the ſnow, ſeems improbable upon divers accounts): and I remember, that having purpoſely inquired of a Virtuoſo, who lately travelled through *Livonia* to *Moſco*, concerning the truth of this tradition, he both told me, he believed it, and added, that he ſaw divers of thoſe lately named animals either in *Ruſſia* or *Livonia* (for I do not very well remember whether of the two) which, though white, when he ſaw them in winter, they aſſured him had been black, or other colours, before the winter began, and would be ſo again when it was over. But for further ſatisfaction, I alſo conſulted one, that had for ſome years been an eminent phyſician in *Ruſſia*, who though he rejected ſome other traditions, that are generally enough believed concerning that country, told me nevertheleſs, that he ſaw no cauſe to doubt of this tradition of *Olaus Magnus*, as to foxes and hares; not only becauſe it is a common and uncontrouled aſſertion of the natives; but alſo becauſe he himſelf in the winter could never, that he remembered, ſee foxes and hares of any other colour than white. And I myſelf having ſeen a ſmall white fox, brought out of *Ruſſia* into *England*, towards the latter end of winter, foretold thoſe, that ſhewed him me, that he would change colour in ſummer; and accordingly coming to look upon him again in *July*, I found, that the back and ſides, together with the upper part of the head and tail, were already grown of a dark colour, the lower part of the head and belly containing as yet a whiteneſs. Let me add, that were it not for ſome ſcruple I have, I ſhould think more than what *Olaus* relates confirmed by the judicious *Olearius*, who was twice employed into thoſe parts as a public miniſter, who in his account of *Muſcovy* has this paſſage: *The hares there are grey; but in ſome provinces they grow white in the winter.* And within ſome few lines after; *It is not very difficult to find the cauſe of this change, which certainly proceeds only from the outward cold, ſince I know, that even in ſummer hares will change colour, if they be kept a competent time in a cellar.* I ſay, were it not for ſome ſcruple, becauſe I take notice, that in the ſame page the author affirms, that the like change of colour, that happens to hares in ſome provinces of *Muſcovy*, happens to them alſo in *Livonia*; and yet immediately ſubjoins, that in *Courland* the hares vary not their colour in winter; though theſe two laſt named countries be contiguous, that is, ſevered only by the river of *Dugna*. For it is ſcarce conceivable how cold alone ſhould have, in countries ſo near, ſo ſtrangely differing an operation, though no leſs ſtrange a thing is confeſſed by many, that aſcribe the complexion of Negroes to the heat of the ſun, when they would have the river of *Senega* ſo to bound the *Moors*, that though on the north-ſide they are but tawny, on the other ſide they are black.

There is another opinion concerning the complexion of Negroes, that is not only embraced by many of the more vulgar writers, but likewiſe by that ingenious traveller Mr. *Sandys*, and by a late moſt learned critic, beſides other men of note; and theſe would have the blackneſs of Negroes an effect of *Noah*'s curſe ratified by God's, upon *Cham*. But though I think, that even a Naturaliſt may without diſparagement believe all the miracles atteſted by the Holy Scriptures, yet in this caſe, to fly to a ſupernatural cauſe, will, I fear, look like ſhifting off the difficulty, inſtead of reſolving it; for we inquire not the firſt and univerſal, but the proper, immediate, and phyſical cauſe of the jetty colour of Negroes; and not only we do not find expreſſed in the ſcripture, that the curſe meant by *Noah* to *Cham* was the blackneſs of his poſterity, but we do find plainly enough there, that the curſe was quite another thing, namely, that he ſhould be a ſervant of ſervants, that is, by an Hebraiſm, a very abject ſervant to his brethren: which accordingly did in part come to paſs, when the Iſraelites of the poſterity of *Sem* ſubdued the Canaanites, that deſcended from *Cham*, and kept them in great ſubjection. Nor is it evident, that blackneſs is a curſe; for navigators tell us of black nations who think ſo much otherwiſe of their own condition, that they paint the devil white. Nor is blackneſs inconſiſtent with beauty, which even to our European eyes conſiſts not ſo much in colour, as an advantageous ſtature, a comely ſymmetry of the parts of the body, and good features in the face. So that I ſee not, why blackneſs ſhould be thought ſuch a curſe to the Negroes, unleſs perhaps it be, that being wont to go naked in thoſe hot climates, the colour of their ſkin does probably, according to the doctrine above delivered, make the ſun-beams more ſcorching to them, than they would prove to a people of a white complexion.

Greater probability there is, that the principal cauſe (for I would not exclude all concurrent ones) of the blackneſs of Negroes is ſome peculiar and ſeminal impreſſion: for not only we ſee, that Blackmoor boys, brought over into theſe colder climates, loſe not their colour; but good authors inform us, that the offspring of Negroes tranſplanted out of *Africa*, above a hundred years ago, retain ſtill the complexion of their progenitors, though poſſibly in tract of time it will decay; as, on the other ſide, the white people removing into very hot climates, have their ſkins by the heat of the ſun ſcorched into dark colours; yet neither they, nor their children have been obſerved, even in the countries of negroes, to deſcend to a colour amounting to that of the natives. Whereas I remember I have read in *Piſo*'s excellent account of *Braſil*, that betwixt the Americans and Negroes are generated a diſtinct ſort of men, which they call *Cabocles*; and betwixt Portugals and Æthiopian women, he tells us, he has ſometimes ſeen twins, whereof one had a white ſkin, the other a black: not to mention here ſome other inſtances he gives, that the productions of the mixtures of differing people, that is (indeed) the effects of ſeminal impreſſions, which they conſequently argue to have been their cauſes. And we ſhall not much ſcruple at this, if we conſider, that even organical parts may receive great differences from ſuch peculiar impreſſions, upon what account ſoever they came to be ſettled in the firſt individual perſons, from whom they are propagated to poſterity, as we ſee in the blobber-lips and flat-noſes of moſt nations of Negroes. And if we may credit what learned men deliver concerning the little feet of the Chineſes, the *Macrocephali* taken notice of by *Hippocrates* will not be the only inſtance we might apply to our preſent purpoſe. And on this occaſion it will not perchance be impertinent to add ſomething of what I have obſerved in other animals, as there are a ſort of hens, that want rumps; and that (not to mention, that in ſeveral places there is a ſort of crows or daws, that are not coal black as ours, but partly of a whitiſh colour) in ſpight of *Pophyry*'s examples

Piſo *Nat. & Med. Hiſt.* Braſil. lib. 1. *in fine.*

of inſeparable accidents, I have ſeen a perfectly white raven, as to bill as well as feathers, which I attentively conſidered, for fear of being impoſed upon. And this recalls into my memory, what a very ingenious phyſician has divers times related to me of a young lady, to whom being called, he found, that though ſhe much complained of want of health, yet there appeared ſo little cauſe either in her body, or her condition, to gueſs, that ſhe did any more than fancy herſelf ſick; that ſcrupling to give her phyſic, he perſuaded her friends rather to divert her mind by little journeys of pleaſure: in one of which going to viſit St. *Winifred*'s well, this lady, who was a catholic, and devout in her religion, and a pretty while in the water to perform ſome devotions, and had occaſion to fix her eyes very attentively upon the red pebble-ſtones, which in a ſcattered order made up a good part of thoſe that appeared through the water; and a while after growing big, ſhe was delivered of a child, whoſe white ſkin was copiouſly ſpeckled with ſpots of the colour and bigneſs of thoſe ſtones; and though now this child have already lived ſeveral years, yet ſhe ſtill retains them. I have but two things to add concerning the blackneſs of Negroes; the one is, that the ſeat of that colour ſeems to be but the thin epidermis, or outward ſkin; for I knew a young Negro, who having been lightly ſick of the ſmall-pox or meaſles (for it was doubted, which of the two was his diſeaſe) I found by inquiry of a perſon, that was concerned for him, that in thoſe places the little tumours had broke their paſſage through the ſkin, when they were gone, they left whitiſh ſpecks behind them; and the lately commended *Piſo* aſſures us, that having the opportunity in *Braſil* to diſſect many Negroes, he clearly found, that their blackneſs went no deeper than the very outward ſkin, which cuticula or epidermis being removed, the undermoſt ſkin or cutis appeared juſt as white as that of European bodies. And the like has been affirmed to me by a phyſician of our own, whom hearing he had diſſected a Negroe here in *England*, I conſulted about this particular. The other thing to be here taken notice of concerning Negroes is, that having inquired of an intelligent acquaintance of mine (who keeps in the *Indies* about three hundred of them, as well women as men, to work in his plantations) whether their children come black into the world; he anſwered, that they did not, but were brought forth of almoſt the like reddiſh colour with our European children: and having further enquired, how long it was before theſe infants appeared black, he replied, that it was not wont to be many days. And agreeable to this account I find that given us in a freſhly publiſhed French book, written by a Jeſuit, that had good opportunity of knowing the truth of what he delivers; for being one of the miſſionaries of his order into the ſouthern *America* upon the laudable deſign of converting infidels to Chriſtianity, he baptized ſeveral infants, which when newly born were much of the ſame colour with European babes, but within about a week began to appear of the hue of their parents. But more pregnant is the teſtimony of our countryman *Andrew Battel*, who being ſent priſoner by the Portugals to *Angola*, lived there, and in the adjoining regions, partly as a priſoner, partly as a pilot, and partly as a ſoldier, near eighteen years; and he mentioning the *African* kingdom of *Longo*, peopled with Blacks, has this paſſage: *The children in this country are born white, and change their colour in two days to a perfect black.* As for example, *The* Portugals, *which dwell in the kingdom of* Longo, *have ſometimes children by the Negroe-women; and many times the fathers are deceived, thinking, when the child is born, that it is theirs, and within two days it proves the ſon or daughter of a Negroe, which the Portugals greatly grieve at.* And the ſame perſon has elſewhere a relation, which, if he have made no uſe at all of the liberty of a traveller, is very well worth our notice; ſince this, together with that we have formerly mentioned

Purchas *Pilgrim, ſecond part, ſeventh Book, Chap.* 3. *Sect.* 5.

tioned of ſeminal impreſſions, ſhews a poſſibility, that a race of Negroes might be begun, though none of the ſons of *Adam* for many precedent generations were of that complexion. For I ſee not, why it ſhould not be at leaſt as poſſible, that white parents may ſometimes have black children, as that African Negroes ſhould ſometimes have laſtingly white ones; eſpecially ſince concurrent cauſes may eaſily more befriend the productions of the former kind, than under the ſcorching heat of *Africa* thoſe of the latter. And I remember on the occaſion of what he delivers, that of the white raven formerly mentioned, the poſſeſſor affirmed to me, that in the neſt, out of which he was taken white, they found with him but one other young one, and that he was of as jetty a black as any common raven. But let us hear our author himſelf: *Here are* (ſays he, ſpeaking of the formerly mentioned regions) *born in this country white children, which is very rare among them, for their parents are Negroes; and when any of them are born, they are preſented to the king, and are called* Dondos; *theſe are as white as any white men. Theſe are the king's witches, and are brought up in witchcraft, and always wait on the king: there is no man, that dares meddle with theſe* Dondos; *if they go to the market, they may take what they liſt, for all men ſtand in awe of them. The king of* Longo *hath four of them.* And yet this country in our globes is placed almoſt in the midſt of the torrid zone (four or five degrees ſouthward of the line.) And our author elſewhere tells us of the inhabitants, that they are ſo fond of their blackneſs, that they will not ſuffer any, that is not of that colour (as the Portugals that come to trade thither) to be ſo much as buried in their land; of which he annexes a particular example, that may be ſeen in his voyage preſerved by our induſtrious countryman Mr. *Purchas.* But it is high time for me to diſmiſs obſervations, and go on with experiments.

Purchas *ibid.*

Purchas *ibid. in fine.*

EXPERIMENT XII.

THE way, *Pyrophilus*, of producing whiteneſs by chymical precipitations is very well worth our obſerving; for thereby bodies, of very differing colours as well as natures, though diſſolved in ſeveral liquors, are all brought into calces or powders that are white. Thus we find, that not only crabs-eyes, that are of themſelves white, and pearls that are almoſt ſo, but coral and minium that are red, being diſſolved in ſpirit of vinegar, may be uniformly precipitated by oil of tartar into white powders. Thus ſilver and tin ſeparately diſſolved in aqua fortis will the one precipitate itſelf, and the other be precipitated by common ſalt-water into a white calx, and ſo will crude lead and quickſilver firſt diſſolved likewiſe in aqua fortis. The like calx will be afforded, as I have tried, by a ſolution of that ſhining mineral tinglaſs diſſolved in aqua fortis, and precipitated out of it; and divers of theſe calces may be made at leaſt as fair and white, if not better coloured, if inſtead of oil of tartar they were precipitated with oil of vitriol, or with another liquor I could name. Nay, that black mineral antimony itſelf, being reduced by and with the ſalts, that concur to the compoſition of common ſublimate, into that clear though unctuous liquor, that chymiſts commonly call rectified butter of antimony, will, by the bare affuſion of ſtore of fair water, be ſtruck down into that ſnow-white powder, which when the adhering ſaltneſs is well waſhed off, chymiſts are pleaſed to call *Mercurius Vitæ*; though the like powder may be made of antimony, without the addition of any mercury at all. And this lacteſcence, if I may ſo call it, does alſo commonly enſue, when ſpirit of wine, being impregnated with thoſe parts of gums or other vegetable concretions, that are ſuppoſed to abound with ſulphureous corpuſcles, fair water is ſuddenly poured upon the tincture or ſolution. And I remember, that very lately I did, for trial ſake, on a tincture of Benjamin drawn with ſpirit of wine, and brought to be as red as blood, pour ſome fair water; which preſently mingling with the liquor, immediately turned the whole mixture white. But if ſuch ſeeming milks be ſuffered to ſtand unſtirred for a convenient while, they are wont to let fall to the bottom a reſinous ſubſtance, which the ſpirit of wine diluted and weakened by the water poured into it is unable to ſupport any longer. And ſomething of kin to this change of colour in vegetables is that, which chymiſts are wont to obſerve upon the pouring of acid ſpirits upon the red ſolution of ſulphur, diſſolved in an infuſion of pot-aſhes, or in ſome other ſharp lixivium; the precipitated ſulphur, before it ſubſides, immediately turning the red liquor into a white one. And other examples might be added of this way of producing whiteneſs in bodies by precipitating them out of the liquors, wherein they have been diſſolved. But I think it may be more uſeful to admoniſh you, *Pyrophilus*, that this obſervation admits of reſtrictions, and is not ſo univerſal, as by this time perhaps you have begun to think it: for though moſt precipitated bodies are white, yet I know ſome that are not; for gold diſſolved in aqua regis, whether you precipitate it with oil of tartar, or with ſpirit of ſal armoniac, will not afford a white, but a yellow calx. Mercury alſo, though reduced into ſublimate, and precipitated with liquors abounding with volatile ſalts, as the ſpirits drawn from urine, hartſhorn, and other animal ſubſtances, yet will afford, as we noted in our firſt experiment about whiteneſs and blackneſs, a white precipitate; yet with the ſolution of pot-aſhes and other lixiviate ſalts, it will let fall an orange-tawny powder. And ſo will crude antimony, it being diſſolved in a ſtrong lye, if you pour (as far as I remember) any acid liquor upon the ſolution newly filtrated, whilſt it is yet warm. And if upon the filtrated ſolution of vitriol, you pour a ſolution of one of theſe fixed ſalts, there will ſubſide a copious ſubſtance, very far from having any whiteneſs, which the chymiſts are pleaſed to call (how properly I have elſewhere examined) the ſulphur of vitriol. So that moſt diſſolved bodies being by precipitation brought to white powders, and yet ſome affording precipitates of other colours, the reaſon of both the phænomena may deſerve to be enquired into.

EXPERIMENT XIII.

See Scaliger *Exercit.* 325. Sect. 9.

SOME learned modern writers are of opinion, that the account, upon which whiteneſs and blackneſs ought to be called, as they commonly are, the two extreme colours, is, that blackneſs (by which I preſume is meant the bodies endowed with it) receives no other colours; but whiteneſs very eaſily receives them all: whence ſome of them compare whiteneſs to the *Ariſtotelian Materia prima*, that being capable of any ſort of forms, as they ſuppoſe white bodies to be of every kind of colour. But not to diſpute about names or expreſſions, the thing itſelf that is affirmed as matter of fact, ſeems to be true enough in moſt caſes, not in all, or ſo as to hold univerſally. For though it be a common obſervation among dyers, that cloaths, which, have once been thoroughly imbued with black, cannot ſo well afterwards be dyed into lighter colours, the pre-exiſtent dark colour infecting the ingredients, that carry the lighter colour to be introduced, and making it degenerate into ſome more ſad one; yet the experiments lately mentioned may ſhew us, that where the change of colour in black bodies is attempted, not by mingling bodies of lighter colours with them, but by addition of ſuch things as are proper to alter the texture of thoſe corpuſcles that contain the black colour, it is no ſuch difficult matter, as the lately mentioned learned men imagine, to alter the colour of black bodies. For we ſaw, that inks of ſeveral kinds might in a trice be deprived of all their blackneſs; and thoſe made with logwood and red-roſes might alſo be changed, the one into a red, the other into a reddiſh liquor; and with oil of vitriol I have ſometimes turned black pieces of ſilk into a kind of yellow; and though the taffaty were thereby made rotten, yet the ſpoiling of that does no way prejudice the experiment, the change of black ſilk into yellow being nevertheleſs true, becauſe the yellow ſilk is the leſs good. And as for whiteneſs I think the general affirmation of its being ſo eaſily deſtroyed, or tranſmuted by any other colour, ought not to be received without ſome cautions and reſtrictions. For whereas, according to what I formerly noted, lead is by calcination turned into that red powder we call minium, and tin by calcination reduced to a white calx; the common putty, that is ſold and uſed ſo much in ſhops, inſtead of being, as it is pretended and ought to be, only the calx of tin, is, by the artificers that make it, to ſave the charge of tin, made (as ſome of themſelves have confeſſed, and as I long ſuſpected by the cheap rate it may be bought for) but of half tin and half lead, if not far more lead than tin; and yet the putty, in ſpite of ſo much lead, is a very white powder, without diſcloſing any mixture of minium. And ſo if you take two parts of copper, which is a high coloured metal, to but one of tin, you may by fuſion bring them into one maſs, wherein the whiteneſs of the tin is much more conſpicuous and predominant than the reddiſhneſs of the copper. And on this occaſion it may not be impertinent to mention an experiment, which I relate upon the credit of a very honeſt man, whom I purpoſely inquired of about it, being myſelf not very fond of making trials with arſenic: the experiment is this; that if you colliquate arſenic and copper in a due proportion, the arſenic will blanch the copper both within and without, which is an experiment well enough known. But when I inquired, whether or no this white mixture being ſkilfully kept a while upon the cupel would not let go its arſenic, which made whiteneſs its predominant colour, and return to the reddiſhneſs of copper, I was aſſured of the affirmative. So that among mineral bodies, ſome of thoſe that are white, may be far more capable, than thoſe I am reaſoning with ſeem to have known, of eclipſing others, and of making their colour predominant in mixtures. In further confirmation of which may be added, that I remember, that I alſo took a lump of ſilver and gold melted together, wherein, by the eſtimate of a very experienced refiner, there might be about a fourth or third part of gold; and yet the yellow colour of the gold was ſo hid by the white of the ſilver, that the whole maſs appeared to be but ſilver; and when it was rubbed upon the touch-ſtone, an ordinary beholder could ſcarce have diſtinguiſhed it from the touch of common ſilver; though if I put a little aqua fortis upon any part of the white ſurface it had given the touch-ſtone, the ſilver in the moiſtened part being immediately taken up and concealed by the liquor, the golden particles would preſently diſcloſe that native yellow, and look rather as if gold, than if the above-mentioned mixture had been rubbed upon the ſtone.

EXPERIMENT XIV.

I TOOK a piece of black horn (poliſhed as being part of a comb) this with a piece of broken glaſs I ſcraped into many thin and curled flakes, ſome ſhorter and ſome longer; and having laid a pretty quantity of theſe ſcrapings together, I found, as I looked for, that the heap they compoſed was white; and though, if I laid it upon a clean piece of white-paper, its colour ſeemed ſomewhat eclipſed by the greater whiteneſs of the body it was compared with, looking ſomewhat like linen, that had been ſullied by a little wearing; yet if I laid it upon a very black body, as upon a beaver hat, it then appeared to be of a good white. Which experiment, that you may in a trice make when you pleaſe, ſeems very much to diſfavour both their doctrine, that would have colours to flow from the ſubſtantial forms of bodies; and that of the chymiſts alſo, who aſcribe them to one or other of their three hypoſtatical principles: for though in our caſe there was ſo great a change made, that the ſame body, without being ſubſtantially either increaſed or leſſened, paſſes immediately from one extreme colour to another (and that too from black to white) yet this ſo great and ſudden change is effected by a ſlight mechanical tranſpoſition of parts, there being no ſalt or ſulphur or mercury, that can be pretended to be added or taken away, nor yet any ſubſtantial form, that can reaſonably be ſuppoſed to be generated and deſtroyed, the effect proceeding only from a local motion of the parts, which ſo varied their poſition, as to multiply their diſtinct ſurfaces, and to qualify them to reflect far more light to the eye, than they could before they were ſcraped off from the entire piece of black horn.

EXPERIMENT XV.

AND now, *Pyrophilus*, it will not be improper for us to take ſome notice of an opinion touching the cauſe of blackneſs, which I judged it not ſo ſeaſonable to queſtion, till I had ſet down ſome of the experiments, that might juſtify my diſſent from it. You know, that of late divers learned men, having adopted the three hypoſtatical principles, beſides other notions of the chymiſts, are very inclinable to reduce all qualities of bodies to one or other of thoſe three principles; and particularly aſſign for the cauſe of blackneſs the ſooty ſteam of aduſt or torrified ſulphur. But I hope, that what we have delivered above to countenance the opinion we have propoſed about the cauſe of blackneſs, will ſo eaſily ſupply you with ſeveral particulars, that may be made uſe of againſt this opinion, that I ſhall now repreſent to you but two things concerning it.

AND firſt, it ſeems, that the favourers of the chymical theories might have pitched upon ſome more proper term, to expreſs the efficient of blackneſs than ſulphur aduſt; for we know, that common ſulphur, not only when melted, but even when ſublimed. does not grow black by ſuffering the action of the fire, but continues and aſcends yellow, and rather more than leſs white, than it was before its being expoſed to the fire. And if it be ſet on fire, as when we make that acid liquor, that chymiſts call *Oleum Sulphuris per campanam*, it affords very little ſoot: and indeed the flame yields ſo little, that it will ſcarce in any degree black a ſheet of white paper, held a pretty while over the flame and ſmoke of it, which is obſerved rather to whiten than infect linen, and which does plainly make red roſes grow very pale, but not at all black, as far as the ſmoke is permitted to reach the leaves. And I can ſhew you a ſort of fixt ſulphur made by an induſtrious laborant of your acquaintance, who aſſured me, that he was wont to keep it for divers weeks together night and day in a naked and violent fire, almoſt like that of the glaſs-houſe; and when, to ſatisfy my curioſity, I made him take out a lump of it, though it were glowing hot (and yet not melted) it did not, when I had ſuffered it to cool, appear black, the true colour of it being a true red. I know it may be ſaid, that chymiſts in the opinion above recited mean the principle of ſulphur, and not common ſulphur, which receives its name, not from its being all perfectly of a ſulphureous nature, but for that plenty and predominancy of the ſulphureous principle in it. But allowing this, it is eaſy to reply, that according to this very reaſon, torrified ſulphur ſhould afford more blackneſs than moſt other concretes, wherein that principle is confeſſed to be far leſs copious. Alſo when I have expoſed camphire to the fire in cloſe veſſels, as inflammable, and conſequently (according to the chymiſts) as ſulphureous a body as it is, I could not by ſuch a degree of heat as brought it to fuſion, and made it to boil in the glaſs, impreſs any thing of blackneſs, or of any other colour, than its own pure white, upon this vegetable concrete. But what ſhall we ſay to ſpirit of wine, which being made by a chymical analyſis of the liquor that affords it, and being totally inflammable, ſeems to have a full right to the title they give it of *Sulphur Vegetabile?* and yet this fluid ſulphur not only contracts not any degree of blackneſs by being often ſo heated, as to be made to boil, but when it burns away with an actual flame, I have not found, that it would diſcolour a piece of white paper held over it, with any diſcernable ſoot. Tin alſo, that wants not, according to the chymiſts, a *Sulphur Joviale*, when thoroughly burned by the fire into a calx, is not black, but eminently white. And I lately noted to you out of *Bellonius*, that the charcoals of oxy-cedar are not of the former of theſe two colours, but of the latter. And the ſmoke of our *Tinby* coals here in *England* has been uſually obſerved rather to blanch linen than to black it. To all which other particulars of the like nature might be added; but I rather chuſe to put you in mind of the third experiment, about making black liquors, or ink, of bodies, that were none of them black before. For how can it be ſaid, that when thoſe liquors are put together actually cold, and continue ſo after their mixture, there intervenes any new aduſtion of ſulphur to produce the emergent blackneſs? (and the ſame queſtion will be applicable to the blackneſs produced upon the blade of a knife, that has cut lemons and ſome kind of ſour apples, if the juice, though both actually and potentially cold, be not quickly wiped off.) And when by the inſtilling either of a few drops of oil of vitriol, as in the ſecond experiment, or of a little of the liquor mentioned in the paſſage pointed at in the fourth experiment (where I teach at once to deſtroy one black ink, and make another) the blackneſs produced by thoſe experiments is preſently deſtroyed; if the colour proceeded only from the plenty of ſulphureous parts, torrified in the black bodies, I demand what becomes of them, when the colour ſo ſuddenly diſappears? For it cannot reaſonably be ſaid, that all thoſe, that ſufficed to make ſo great a quantity of black matter, ſhould reſort to ſo very ſmall a proportion of the clarifying liquor (if I may ſo call it) as to be diluted by it, without at all denigrating it. And if it be ſaid, that the inſtilled liquor diſperſed thoſe black corpuſcles, I demand, how that diſperſion comes to deſtroy their blackneſs, but by making ſuch a local motion of their parts, as deſtroys their former texture? Which may be a matter of ſuch moment in caſes like ours, that I remember, that I have in few hours, without addition, from ſoot itſelf, attained pretty ſtore of cryſtalline ſalt, and good ſtore of tranſparent liquor; and (which I have on another occaſion noted as remarkable) this ſo black ſubſtance had its colour ſo altered, by the change of texture it received from the fire, wherewith it was diſtilled, that it did for a great while afford ſuch plenty of very white exhalations, that the receiver, though large, ſeemed to be almoſt filled with milk.

Secondly, But were it granted, as it is in ſome caſes not improbable, that divers bodies may receive a blackneſs from a ſooty exhalation, occaſioned by the aduſtion of their ſulphur, which (for the reaſons lately mentioned) I ſhould rather call their oily parts; yet ſtill this account is applicable but to ſome particular bodies, and will afford us no general theory of blackneſs. For if, for example, white hartſhorn being, in veſſels well luted to each other, expoſed to the fire, be ſaid to turn black by the infection of its own ſmoke, I think I may juſtly demand, what it is that makes the ſmoke or ſoot itſelf black, ſince no ſuch colour, but its contrary, appeared before in the hartſhorn? And with the ſame reaſon, when we are told, that torrified ſulphur makes bodies black, I deſire to be told alſo, why torrefaction makes ſulphur itſelf black? Nor will there be any ſatisfactory reaſon aſſigned of theſe queries, without taking in thoſe fertile as well as intelligible mechanical principles of the poſition and texture of the minute parts of the body in reference to the light and the eye; and theſe applicable principles may ſerve the turn in many caſes, where the aduſtion of ſulphur cannot be pretended; as in the appearing blackneſs of an open window looked upon at a ſomewhat remote diſtance from the houſe; as alſo in the blackneſs men think they ſee in the holes, that happen to be in white linen, or paper of the like colour; and in the increaſing blackneſs immediately produced barely by ſo rubbing velvet, whoſe piles were inclined before, as to reduce them to a more erect poſture; in which and in many other caſes formerly alleged, there appears nothing requiſite to the production of the blackneſs, but the hindering of the incident beams of light from rebounding plentifully enough to the eye. To be ſhort, thoſe I reaſon with, do concerning blackneſs what the chymiſts are wont alſo to do concerning other qualities; namely, to content themſelves to tell us, in what ingredient of a mixt body, the quality enquired after does reſide, inſtead of explicating the nature of it, which (to borrow a compariſon from their own laboratories) is much as if in an inquiry after the cauſe of ſalivation, they ſhould think it enough to tell us, that the ſeveral kinds of precipitates of gold and mercury, as likewiſe of quickſilver and ſilver (for I know the make and uſe of ſuch precipitates alſo) do ſalivate upon the account of the mercury, which though diſguiſed abounds in them; whereas the difficulty is as much to know upon what account mercury itſelf, rather than other bodies, has that power of working by ſalivation. Which I ſay not, as though it were not ſomething (and too often the moſt we can arrive at) to diſcover in which of the ingredients of a compounded body the quality, whoſe nature is ſought, reſides; but becauſe, though this diſcovery itſelf may paſs for ſomething, and is oftentimes more than what is taught us about the ſame ſubjects in the ſchools, yet we ought not to think it enough, when more clear and particular accounts are to be had.

The Experimental Hiſtory of COLOURS begun.

PART III.

Containing promiſcuous Experiments about COLOURS.

EXPERIMENT I.

BECAUSE that, according to the conjectures I have above propoſed, one of the moſt general cauſes of the diverſity of colours in opacous bodies, is, that ſome reflect the light mingled with more, others with leſs of ſhade (either as to quantity, or as to interruption ;) I hold it not unfit to mention, in the firſt place, the experiments, that I thought upon to examine this conjecture. And though coming to tranſcribe them out of ſome phyſiological *Adverſaria* I had written in looſe papers, I cannot find one of the chief records I had of my trials of this nature, yet the papers, that eſcaped miſcarrying, will, I preſume, ſuffice to manifeſt the main thing, for which I now allege them. I find then among my *Adverſaria* the following narrative.

October the 11th, About ten in the morning in ſun-ſhiny weather (but not without fleeting clouds) we took ſeveral ſorts of paper ſtained, ſome of one colour, and ſome of another; and in a darkened room, whoſe window looked ſouthward, we caſt the beams, that came in at a hole about three inches and a half in diameter, upon a white wall, that was placed on one ſide, about five foot diſtance from them.

THE white gave much the brighteſt reflection.

THE green, red, and blue being compared together, the red gave much the ſtrongeſt reflection, and manifeſtly enough alſo threw its colour upon the wall: the green and blue were ſcarce diſcernable by their colours, and ſeemed to reflect an almoſt equal light.

THE yellow, compared with the two laſt named, reflected ſomewhat more light.

THE red and purple being compared together, the former manifeſtly reflected a good deal more light.

THE blue and purple compared together, the former ſeemed to reflect a little more light, though the purple colour were more manifeſtly ſeen.

A SHEET of very well ſleeked marbled paper being applied as the others, did not caſt any of its diſtinct colours upon the wall, nor throw its light upon it with an equal diffuſion; but threw the beams unſtained and bright to this and that part of the wall, as if its poliſh had given it the nature of a ſpecular body. But comparing it with a ſheet of white paper, we found the reflection of the latter to be much ſtronger, it diffuſing almoſt as much light to a good extent as the marble paper did to one part of the wall.

THE green and purple left us ſomewhat in ſuſpenſe, which reflected the moſt light; only the purple ſeemed to have ſome little advantage over the green, which was dark in its kind.

THUS much I find in our above-mentioned collections; among which there are alſo ſome notes concerning the production of compounded colours, by reflection from bodies differingly coloured. And theſe notes we intended ſhould ſupply us with what we ſhould mention as our ſecond experiment: but having loſt the paper, that contained the particulars, and remembering only in general, that if the objects, which reflected the light, were not ſtrongly coloured and ſomewhat gloſſy, the reflected beams would not manifeſtly make a compounded colour upon the wall, and even then but very faintly; we ſhall now ſay no more of that matter, only reſerving ourſelves to mention hereafter the compoſition of a green, which we ſtill retain in memory.

EXPERIMENT II.

WE may add, *Pyrophilus*, on this occaſion, that though a darkened room be generally thought requiſite to make the colour of a body appear by reflection from another body, that is not one of thoſe, that are commonly agreed upon to be ſpecular (as poliſhed metal, quickſilver, glaſs, water, *&c.*) yet I have often obſerved, that when I wore doublets lined with ſome ſilken ſtuff, that was very gloſſy and vividly coloured, eſpecially red, I could in an enlightened room plainly enough diſcern the colour upon the pure white linen, that came out at my ſleeve, and reached to my cuffs; as if that fine white body were more ſpecular than coloured and unpoliſhed bodies are thought capable of being.

EXPERIMENT III.

WHILST we were making the newly mentioned experiments, we thought fit to try alſo what compoſition of colours might be made by altering the light in its paſſage to the eye, by the interpoſition not of perfectly diaphanous bodies (that having been already tried by others as well as by us, as we ſhall ſoon have occaſion to take notice) but of ſemi-opacous bodies, and thoſe ſuch as looked upon in an ordinary light, and not held betwixt it and the eye, are not wont to be diſcriminated from the reſt of opacous bodies. Of this trial our mentioned *Adverſaria* preſent us the following account:

HOLDING theſe ſheets, ſometimes one, ſometimes the other of them, before the hole betwixt the ſun and the eye, with the coloured ſides obverted to the ſun; we found them ſingle to be ſomewhat tranſparent, and appear of the ſame colour as before, only a little altered by the great light they were placed in: but laying two of them one over another, and applying them ſo to the hole, the colours were compounded as follows.

THE blue and yellow ſcarce exhibited any thing but a darker yellow, which we aſcribed to the coarſeneſs of the blue paper, and its darkneſs in its kind. For applying the blue parts of the marbled paper with the yellow paper after the ſame manner, they exhibited a good green.

THE yellow and red looked upon together gave us but a dark red, ſomewhat (and but a little) inclining to an orange colour.

THE purple and red looked on together appeared more ſcarlet.

THE purple and yellow made an orange.

THE green and red made a dark orange-tawny.

THE green and purple made the purple appear more dirty.

THE blue and purple made the purple more lovely, and far more deep.

THE red parts of the marbled paper, looked upon with the yellow, appeared of a red far more like ſcarlet than without it.

BUT the fineneſs or coarſeneſs of the papers, their being carefully or ſlightly coloured, and divers other circumſtances, may ſo vary the events of ſuch experiments as theſe, that if, *Pyrophilus*, you would build much on them, you muſt carefully repeat them.

EXPERIMENT IV.

THE triangular priſmatical glaſs being the inſtrument, upon whoſe effects we may the moſt commodiouſly ſpeculate the nature of emphatical colours (and perhaps that of others too;) we thought it might be uſeful to obſerve the ſeveral reflections and refractions, which the incident beams of light ſuffer in rebounding from it, and paſſing through it. And this we thought might be beſt done, not (as is uſual) in an ordinary enlightened room, where (by reaſon of the difficulty of doing otherwiſe) even the curious have left particulars unheeded, which may in a convenient place be eaſily taken notice of; but in a darkened room, where by placing the glaſs in a convenient poſture, the various reflections and refractions may be diſtinctly obſerved;

The Explication of the Scheme

FP. As Equ[illegible]

[illegible]

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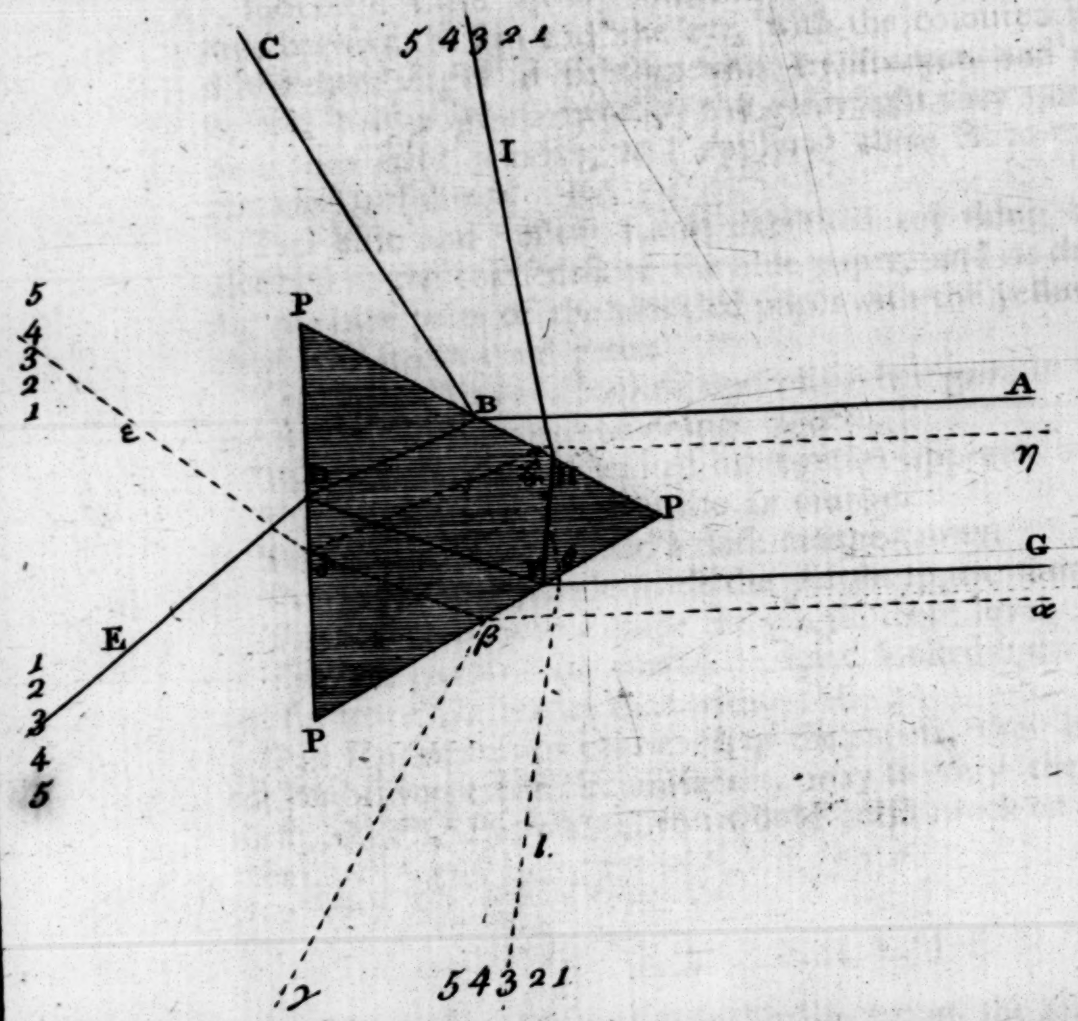

The Explication of the Scheme

PPP. *An Æquilaterotriangular Cryſtalline Pri*
one of whoſe edges **P.** *is placed directly to -*
:wards the Sun

AB *& αβ Two rays from the Sun falling on th*
Priſm at **B** *β. and thence partly reflected t*
:wards **C** *& γ. and partly refracted towards* **D**
BC *& βγ: Those reflected Rays*

BD *& βδ: Those refracted Rays which are par*
refracted towards **E** *& ε. and there paint a*
Iris 12345. denoting the five consecutio
of colours Red, Yellow, Green, Blew, an
Purple; & are partly reflected towards **F** *&*

DF *& δζ. Those Reflected Rays which are pa*
:ly refracted towards **G** *& η. colourleſs, an*
partly reflected, towards **H** *& θ.*

FH *& ζθ Those Reflected Rays which are*
refracted towards **I** *& ι and there pain*
an other fainter Iris, the colours of
which are contrary to the former 543
21. signifying Purple, Blew Green
Yellow, Red, so that the Priſm in this
poſture exhibits four Rainbows.

and where it may appear, what beams are untinged, and which they are, that, upon the bodies, that terminate them, do paint either the primary or ſecondary iris. In purſuance of this we did, in the above-mentioned darkened room, make obſervation of no leſs than four reflections, and three refractions, that were afforded us by the ſame priſm; and thought, that, notwithſtanding what was taught us by the rules of catoptricks and dioptricks, it would not be amiſs to find alſo, by hiding ſometimes one part of the priſm, and ſometimes another, and obſerving where the light or colour vaniſhed thereupon, by which reflection and by which refraction each of the ſeveral places whereon the light rebounding from, or paſſing through, the priſm, appeared either ſincere or tincted, was produced. But becauſe it would be tedious, and not ſo intelligible to deliver this in words, I have thought fit to refer you to the annexed ſcheme, where the newly mentioned particulars may be at one view taken notice of.

EXPERIMENT V.

I Know not whether you will think it inconſiderable to annex to this experiment, that we obſerved in a room not darkened, that the priſmatical iris (if I may ſo call it) might be reflected without loſing any of its ſeveral colours (for we now conſider not their order) not only from a plain looking-glaſs and from the calm ſurface of fair water, but alſo from a concave looking-glaſs; and that refraction did as little deſtroy thoſe colours as reflection. For by the help of a large (double convex) burning-glaſs, through which we refracted the ſun-beams, we found, that one part of the iris might be made to appear either beyond, or on this ſide of the other parts of the ſame iris; but yet the ſame vivid colours would appear in the diſplaced part (if I may ſo term it) as in the other. To which I ſhall add, that having, by hiding the ſide of the priſm, obverted to the ſun with an opacous body, wherein only one ſmall hole was left for the light to paſs though, reduced the priſmatical iris (caſt upon white paper) into a very narrow compaſs, and looked upon it through a microſcope; the colours appeared the ſame as to kind, that they did to the naked eye.

EXPERIMENT VI.

IT may afford matter of ſpeculation to the inquiſitive, ſuch as you, *Pyrophilus*, that as the colours of outward objects brought into a darkened room do ſo much depend for their viſibility upon the dimneſs of the light they are there held by, that the ordinary light of the day being freely let in upon them, they immediately diſappear; ſo our trials have informed us, that as to the priſmatical iris painted on the floor by the beams of the ſun trajected thorough a triangular glaſs, though the colours of it appear very vivid even at noon-day, and in ſun-ſhiny weather, yet by a more powerful light they may be made to diſappear. For having ſometimes, (in proſecution of ſome conjectures of mine not now to be inſiſted on) taken a large metalline concave ſpeculum, and with it caſt the converging beams of the ſun upon a priſmatical iris, which I had cauſed to be projected upon the floor, I found, that the over-powerful light made the colours of the iris diſappear. And if I ſo reflected the light, as that it croſſed but the middle of the iris, in that part only the colours vaniſhed or were made inviſible; thoſe parts of the iris, that were on the right and left hand of the reflected light (which ſeemed to divide them, and cut the iris aſunder) continuing to exhibit the ſame colours as before. But upon this we muſt not now ſtay to ſpeculate.

EXPERIMENT VII.

I HAVE ſometimes thought it worth while to take notice, whether or no the colours of opacous bodies might not appear to the eye ſomewhat diverſified, not only by the diſpoſition of the ſuperficial parts of the bodies themſelves, and by the poſition of the eye in reference to the object and the light, (for theſe things are notorious enough) but according alſo to the nature of the lucid body, that ſhines upon them. And I remember, that in proſecution of this curioſity, I obſerved a manifeſt difference in ſome kinds of coloured bodies looked on by day-light, and afterwards by the light of the moon, either directly falling on them, or reflected upon them from a concave looking-glaſs. But not finding at preſent, in my collections about colours, any thing ſet down of this kind, I ſhall, till I have opportunity to repeat them, content myſelf to add what I find regiſtered concerning colours looked on by candle-light, in regard that not only the experiment is more eaſy to be repeated, but the objects being the ſame ſorts of coloured paper laſtly mentioned, the collation of the two experiments may help to make the conjectures they will ſuggeſt ſomewhat the leſs uncertain.

WITHIN a few days of the time above-mentioned, divers ſheets of coloured paper, that had been looked upon before in the ſun-ſhine, were looked upon at night by the light of a pretty big candle (ſnuffed) and the changes that were obſerved were theſe:

THE yellow ſeemed much fainter than in the day, and inclinable to a pale ſtraw-colour.

THE red ſeemed little changed; but ſeemed to reflect light more ſtrongly than any other colour (for white was none of them).

A FAIR deep green looked upon by itſelf, ſeemed to be a dark blue: but being looked upon together with a dark blue, appeared greeniſh; and beheld together with a yellow, appeared more blue than at firſt.

THE blue looked more like a deep purple or murray, than it had done in the day-light.

THE purple ſeemed very little altered.

THE red looked upon with the yellow made the yellow look almoſt like brown cap-paper.

N. B. THE caution ſubjoined to the third experiment is alſo applicable to this.

EXPERIMENT VIII.

BUT here I muſt not omit to ſubjoin, that to ſatisfy ourſelves, whether or no the light of a candle were not made unſincere, and as it were tinged with a yellow colour, by the admixtion of the corpuſcles it aſſumes from its fuel; we did not content ourſelves with what appears to the naked eye, but taking a pretty thick rod or cylinder (for thin pieces would not ſerve the turn) of deep blue glaſs, and looking upon the candle's flame at a convenient diſtance through it, we perceived, as we expected, the flame to look green: which, as we often note, is the colour wont to emerge from the compoſition of opacous bodies, which were apart one of them blue, and the other yellow. And this perchance may be the main reaſon of that, which ſome obſerve, that a ſheet of very white paper being looked upon by candle-light, it is not eaſy at firſt to diſcern it from a light yellow or lemon colour; white bodies (as we have elſewhere obſerved) having more than thoſe, that are otherwiſe coloured, of a ſpecular nature; in regard that though they exhibit not (unleſs they be poliſhed) the ſhape of the luminary, that ſhines on them, yet they reflect its light more ſincere and untroubled, by either ſhades or refractions, than bodies of other colours, (as blue, or green, or yellow, or the like.)

EXPERIMENT IX.

WE took a leaf of ſuch foliated gold, as apothecaries are wont to gild their pills with; and with the edge of a knife, (lightly moiſtened by drawing it over the ſurface of the tongue, and afterwards) laid upon the edge of the gold leaf, we ſo faſtened it to the knife, that being held againſt the light, it continued extended like a little flag. This leaf being held very near the eye, and obverted to the light, appeared ſo full of pores, that it ſeemed to have ſuch a kind of tranſparency, as that of a ſieve, or a piece of cypreſs, or a love-hood; but the light that paſſed by theſe pores was in its paſſages ſo tempered with ſhadow, and modified, that the eye diſcerned no more a golden colour, but a greeniſh blue. And for others ſatisfaction, we did in the night look upon a candle through ſuch a leaf of gold; and by trying the effect of ſeveral proportions of diſtance betwixt the leaf, the eye and the light, we quickly hit upon ſuch a poſition for the leaf of gold, as that the flame looked on through it, appeared of a greeniſh blue, as we have ſeen in the day-time. The like experiment tried with a leaf of ſilver ſucceeded not well.

EXPERIMENT X.

WE have ſometimes found in the ſhops of our druggiſts a certain wood, which is there called *Lignum Nephriticum*, becauſe the inhabitants of the country, where it grows, are wont to uſe the infuſion of it made in fair water againſt the ſtone of the kidneys. And indeed an eminent phyſician of our acquaintance, who has very particularly inquired into that diſeaſe, aſſures me, that he has found ſuch an infuſion one of the moſt effectual remedies, which he has ever tried againſt that formidable diſeaſe. The ancienteſt account I have met with of this ſimple, is given us by the experienced *Monardes* in theſe words: *Nobis*, ſays he, *Nova Hiſpana mittit quoddam ligni genus craſſum & enode, cujus uſus jam diu receptus fuit in his regionibus ad renum vitia, & urinæ difficultates ac arenulas pellendas. Fit autem hac ratione*; *lignum aſſulatim & minutim conciſum in limpidiſſima aqua fontana maceratur, inque ea relinquitur, donec aqua à bibentibus abſumpta ſit*; *dimidia hora poſt injectum lignum aqua cœruleum colorem contrahit, qui ſenſim intenditur pro temporis diuturnitate, tametſi lignum candidum ſit.* This wood, *Pyrophilus*, may afford us an experiment, which, beſides the ſingularity of it, may give no ſmall aſſiſtance to an attentive conſiderer towards the detection of the nature of colours. The experiment, as we made it, is this: Take *Lignum Nephriticum*, and with a knife cut it into thin ſlices; put about a handful of theſe ſlices into two, three or four pounds of the pureſt ſpring-water; let them infuſe there a night; but if you be in haſte, a much ſhorter time may ſuffice. Decant this impregnated water into a clear glaſs phial; and if you hold it directly between the light and your eye, you ſhall ſee it wholly tincted, (excepting the very top of the liquor, wherein you will ſometimes diſcern a ſky-coloured circle) with an almoſt golden colour, unleſs your infuſion have been made too ſtrong of the wood; for in that caſe it will againſt the light appear ſomewhat dark and reddiſh, and requires to be diluted by the addition of a convenient quantity of water. But if you hold this phial from the

Nicolaus Monardes lib. *ſimplic. ex India allator.* cap. 27.

the light, ſo that your eye be placed betwixt the window and the phial, the liquor will appear of a deep and lovely ceruleous colour, of which alſo the drops, if any be lying on the outſide of the glaſs, will ſeem to be very perfectly. And thus far we have tried the experiment, and found it to ſucceed even by the light of candles of the larger ſize. If you ſo hold the phial over againſt your eyes, that it may have a window on one ſide of it, and a dark part of the room both before it and on the other ſide, you ſhall ſee the liquor partly of a bluiſh and partly of a golden colour. If turning your back to the window, you pour out ſome of the liquor towards the light and towards your eyes, it will ſeem at the coming out of the glaſs to be perfectly ceruleous; but when it is fallen down a little way, the drops may ſeem particoloured, according as the beams of light do more or leſs fully penetrate and illuſtrate them. If you take a baſon about half full of water, and having placed it ſo in the ſun-beams ſhining into a room, that one part of the water may be freely illuſtrated by the beams of light, and the other part of it darkened by the ſhadow of the brim of the baſon; if then, I ſay, you drop of our tincture, made ſomewhat ſtrong, both into the ſhade and illuminated parts of the water, you may by looking upon it from ſeveral places, and by a little agitation of the water, obſerve divers pleaſing phænomena, which were tedious to particularize. If you pour a little of this tincture upon a ſheet of white paper, ſo as the liquor may remain of ſome depth upon it, you may perceive the neighbouring drops to be partly of one colour, and partly of the other, according to the poſition of your eye in reference to the light when it looks upon them; but if you pour off the liquor, the paper will ſeem dyed of an almoſt yellow colour. And if a ſheet of paper with ſome of this liquor in it be placed in a window where the ſun-beams may ſhine freely on it, then if you turn your back to the ſun and take a pen or ſome ſuch ſlender body, and hold it overthwart betwixt the ſun and the liquor, you may perceive, that the ſhadow projected by the pen upon the liquor will not all of it be a vulgar and dark, but in part a curiouſly coloured ſhadow; that edge of it, which is next the body that makes it, being almoſt of a lively golden colour, and the remoter verge of a ceruleous one.

THESE and other phænomena, which I have obſerved in this delightful experiment, divers of my friends have looked upon not without ſome wonder; and I remember an excellent oculiſt, finding by accident in a friend's chamber a fine phial full of this liquor, which I had given that friend, and having never heard any thing of the experiment, nor having any body near him, that could tell him what this ſtrange liquor might be, was a great while apprehenſive, as he preſently after told me, that ſome ſtrange new diſtemper was invading his eyes. And I confeſs, that the unuſualneſs of the phænomena made me very ſolicitous to find out the cauſe of this experiment; and though I am far from pretending to have found it, yet my inquiries have, I ſuppoſe, enabled me to give ſuch hints, as may lead your greater ſagacity to the diſcovery of the cauſe of this wonder. And firſt finding that this tincture, if it were too copious in the water, kept the colours from being ſo lively, and their change from being ſo diſcernable, and finding alſo that the impregnating virtue of this wood did by its being frequently infuſed in new water by degrees decay; I conjectured that the tincture afforded by the wood muſt proceed from ſome ſubtiler parts of it drawn forth by the water, which ſwimming to and fro in it, did ſo modify the light, as to exhibit ſuch and ſuch colours: and becauſe theſe ſubtile parts were ſo eaſily ſoluble even in cold water, I concluded that they muſt abound with ſalts, and perhaps contain much of the eſſential ſalt, as the chymiſts call it, of the wood. And to try whether theſe ſubtile parts were volatile enough to be diſtilled, without the diſſolution of their texture, I carefully diſtilled ſome of the tincted liquor in very low veſſels, and the gentle heat of a lamp furnace; but found all that came over to be as limpid and colourleſs as rock-water, and the liquor remaining in the veſſel to be ſo deeply ceruleous, that it required to be oppoſed to a very ſtrong light to appear of any other colour. I took likewiſe a phial with ſpirit of wine, and a little ſalt of hartſhorn, and found that there was a certain proportion to be met with betwixt the liquor and the ſalt, which made the mixture fit to exhibit ſome little variety of colours not obſervable in ordinary liquors, as it was variouſly directed in reference to the light and the eye; but this change of colour was very far ſhort from that which we had admired in our tincture. But, however, I ſuſpected that the tinging particles did abound with ſuch ſalts, whoſe texture, and the colour ſpringing from it, would probably be altered by piercing acid ſalts, which would in likelihood either make ſome diſſipation of their parts, or aſſociate themſelves to the like bodies, and either way alter the colour exhibited by them; whereupon pouring into a ſmall phial, full of impregnated water, a very little ſpirit of vinegar, I found that, according to my expectation, the ceruleous colour immediately vaniſhed, but was deceived in the expectation I had, that the golden colour would do ſo too; for, which way ſoever I turned the phial, either to or from the light, I found the liquor to appear always of a yellowiſh colour and no other. Upon this I imagined that the acid ſalts of the vinegar having been able to deprive the liquor of its ceruleous colour, a ſulphureous ſalt being of a contrary nature, would be able to mortify the ſaline particles of vinegar, and deſtroy their effects; and accordingly having placed myſelf betwixt the window, and the phial, and into the ſame liquor dropt a few drops of oil of tartar *per deliquium*, (as chymiſts call it) I obſerved with pleaſure, that immediately upon the diffuſion of this liquor, the impregnated water was reſtored to its former ceruleous colour; and this liquor of tartar being very ponderous, and falling at firſt to the bottom of the phial, it was eaſy to obſerve that for a little while the lower part of the liquor appeared deeply ceruleous, whilſt all the upper part retained its former yellowneſs, which it immediately loſt as ſoon as either agitation or time had made a competent diffuſion of the liquor of tartar through the body of the former tincture; and this reſtored liquor did, as it was looked upon againſt or from the light, exhibit the ſame phænomena as the tincted water did, before either of the adventitious liquors was poured into it.

HAVING made, *Pyrophilus*, divers trials upon this nephritick wood, we found mention made of it by the induſtrious Jeſuit *Kircherus*, who having received a cup turned of it from the Mexican procurator of his ſociety, has probably received alſo from him the information he gives us concerning that exotick plant; and therefore partly for that reaſon, and partly becauſe what he writes concerning it, does not perfectly agree with what we have delivered, we ſhall not ſcruple to acquaint you in his own words, with as much of what he writes concerning our wood, as is requiſite to our preſent purpoſe. *Hoc loco* (ſays he) *neutiquam omittendum duximus quoddam ligni candidi Mexicani genus, quod indigenæ Coalle & Tlapazatli vocant, quod etſi experientia hucuſque non niſi cæruleo aquam colore tingere docuerit, nos tamen continua experientia invenimus id aquam in omne colorum genus transformare, quod merito cuipiam paradoxum videri poſſet; ligni frutex grandis, ut aiunt, non rarò in molem arboris excreſcit, truncus illius eſt craſſus, enodis, inſtar piri arboris, folia ciceris foliis, aut rutæ haud abſimilia, flores exigui, ablongi, lutei & ſpicatim digeſti; eſt frigida & humida planta, licet parum recedat à medio temperamento. Hujus itaque deſcriptæ arboris lignum in poculum efformatum, aquam eidem infuſam primo in aquam intenſe cæruleam, colore floris bugloſſæ, tingit, &*

Kircher. Art. Mag. lucis & umbræ, lib. 1. part 3.

quo diutius in eo ſteterit, tanto intenſiorem colorem acquirit. Hanc igitur aquam ſi vitriæ ſphæræ infuderis lucique expoſueris, ne ullum quidem cærulei coloris veſtigium apparebit, ſed inſtar aquæ puræ putæ fontanæ limpidam claramque aſpicientibus ſe præbebit. Porro ſi hanc phialam vitream verſus locum magis umbroſum direxeris, totus humor gratiſſimum virorem referet; ſi adhuc umbroſioribus locis, ſubrubrum, & ſic pro rerum objectarum conditione, mirum dictu, colorem mutabit; in tenebris verò vel in vaſe opaco poſita cæruleum colorem ſuum reſumet.

In this paſſage we may take notice of the following particulars. And firſt, he calls it a white Mexican wood, whereas (not to mention that *Monardes* informs us that it is brought out of *Nova Hiſpania)* the wood that we have met with in ſeveral places, and employed as *Lignum Nephriticum*, was not white, but for the moſt part of a much darker colour, not unlike that of the ſadder coloured wood of juniper. It is true, that *Monardes* himſelf alſo ſays, that the wood is white; and it is affirmed, that the wood which is of a ſadder colour is adulterated by being imbued with the tincture of a vegetable, in whoſe decoction it is ſteeped. But having purpoſely inquired of the eminenteſt of our Engliſh druggiſts, he peremptorily denied it. And indeed, having conſidered ſome of the faireſt round pieces of this wood that I could meet with in theſe parts, I had opportunity to take notice that in one or two of them it was the external part of the wood that was white, and the more inward part that was of the other colour; the contrary of which would probably have appeared, if the wood had been adulterated after the aforementioned manner. And I have at preſent by me a piece of ſuch wood, which for about an inch next the bark is white, and then, as it were, abruptly paſſes to the abovementioned colour; and yet this wood, by the tincture it afforded us in water, appears to have its coloured part genuine enough: for as for the white part, it appears, upon trial of both at once, much leſs enriched with the tingent property.

Next, whereas our author tells us, that the infuſion of this wood expoſed in a phial to the light, looks like ſpring-water, in which, he afterwards adds, that there is no tincture to be ſeen in it; our obſervation and his agree not: for the liquor which oppoſed to the darker part of a room exhibits a ſky-colour, did conſtantly, when held againſt the light, appear yellowiſh or reddiſh, according as its tincture was more dilute or deep; and then, whereas it has been already ſaid, that the ceruleous colour was by acid ſalts aboliſhed, this yellowiſh one ſurvived without any conſiderable alteration, ſo that unleſs our author's words be taken in a very limited ſenſe, we muſt conclude, that either his memory miſinformed him, or that his white nephritick wood, and the ſadder coloured one which we employed, were not altogether of the ſame nature. What he mentions of the cup made of *Lignum Nephriticum*, we have not had opportunity to try, not having been able to procure pieces of that wood great enough, and otherwiſe fit to be turned into cups; but as for what he ſays in the title of his experiment, that this wood tinges the water with all ſorts of colours, that is much more than any of thoſe pieces of nephritick wood that we have hitherto employed, was able to make good; the change of colours diſcernable in a phial full of water, impregnated by any of them, as it is directed towards a place more lightſome or obſcure, being far from affording a variety anſwerable to ſo promiſing a title. And as for what he tells us, that in the dark the infuſion of our wood will reſume a ceruleous colour, I wiſh he had informed us how he tried it.

But this brings into my mind, that having ſometimes, for curioſity ſake, brought a round phial with a long neck filled with the tincture of *Lignum Nephriticum* into the darkened room already often mentioned, and holding it ſometimes in, ſometimes near the ſun-beams that entered at the hole, and ſometimes partly in them, and partly out of them, the glaſs being held in ſeveral poſtures, and looked upon from ſeveral neighbouring parts of the room, diſcloſed a much greater variety of colours than in ordinary enlightened rooms it is wont to do; exhibiting, beſides the uſual colours, a red in ſome parts, and a green in others, beſides intermediate colours produced by the differing degrees, and odd mixtures of light and ſhade.

By all this you may ſee, *Pyrophilus*, the reaſonableneſs of what we elſewhere had occaſion to mention, when we have divers times told you, that it is uſeful to have new experiments tried over again, though they were, at firſt, made by knowing and candid men; ſuch reiterations of experiments commonly exhibiting ſome new phænomena, detecting ſome miſtake, or hinting ſome truth, in reference to them, that was not formerly taken notice of. And ſome of our friends have been pleaſed to think, that we have made no unuſual addition to this experiment, by ſhewing a way, how in a moment our liquor may be deprived of its blueneſs, and reſtored to it again by the affuſion of a few drops of liquors, which have neither of them any colour at all of their own. And that which deſerves ſome particular wonder, is, that the ceruleous tincture of our wood is ſubject by the former method to be deſtroyed or reſtored, the yellowiſh or reddiſh tincture continuing what it was. And that you may ſee, that ſalts are of a conſiderable uſe in the ſtriking of colours, let me add to the many experiments which may be afforded us to this purpoſe by the dyers trade, this obſervation; that as far as we have hitherto tried, thoſe liquors in general that are ſtrong of acid ſalts have the power of deſtroying the blueneſs of the infuſion of our wood, and thoſe liquors indiſcriminately that abound with ſulphureous ſalts (under which I comprehend the urinous and volatile ſalts of animal ſubſtances, and the alcalizate or fixed ſalts that are made by incineration) have the virtue of reſtoring it.

A Corollary of the TENTH EXPERIMENT.

THAT this experiment, *Pyrophilus*, may be as well uſeful as delightful to you, I muſt mind you, *Pyrophilus*, that in the newly mentioned obſervation, I have hinted to you a new and eaſy way of diſcovering in many liquors (for I dare not ſay in all) whether it be an acid or ſulphureous ſalt, that is predominant; and that ſuch a diſcovery is oftentimes of great difficulty, and may frequently be of great uſe, he that is not a ſtranger to the various properties and effects of ſalts, and of how great moment it is to be able to diſtinguiſh their tribes, may readily conceive. But to proceed to the way of trying other liquors by an infuſion of our wood, take it briefly thus. Suppoſe I have a mind to try whether I conjecture aright, when I imagine that allom, though it be plainly a mixt body, does abound rather with acid than ſulphureous ſalt: to ſatisfy myſelf herein, I turn my back to the light, and holding a ſmall phial full of the tincture of *Lignum Nephriticum*, which, looked upon in that poſition, appears ceruleous, I drop into it a little of a ſtrong ſolution of allom made in fair water; and finding upon the affuſion and ſhaking of this new liquor, that the blueneſs formerly conſpicuous on our tincture does preſently vaniſh, I am thereby incited to ſuppoſe, that the ſalt predominant in allom belongs to the family of ſour ſalts. But if on the other ſide I have a mind to examine whether or no I rightly conceive that ſalt of urine, or of hartſhorn is rather of a ſaline ſulphureous (if I may ſo ſpeak) than of an acid nature, I drop a little of the ſaline ſpirit of either into the nephritic tincture, and finding that the ceruleous colour is rather thereby deepened than deſtroyed,

ſtroyed, I collect that the ſalts, which conſtitute theſe ſpirits, are rather ſulphureous than acid. And to ſatisfy myſelf yet farther in this particular, I take a ſmall phial of freſh tincture, and placing both it and myſelf in reference to the light as formerly, I drop into the infuſion juſt as much diſtilled vinegar, or other acid liquor as will ſerve to deprive it of its blueneſs (which a few drops, if the ſour liquor be ſtrong, and phial ſmall, will ſuffice to do;) then without changing my poſture, I drop and ſhake into the ſame phial a ſmall proportion of ſpirit of hartſhorn or urine, and finding that upon this affuſion the tincture immediately recovers its ceruleous colour, I am thereby confirmed in my former opinion, of the ſulphureous nature of theſe ſalts. And ſo, whereas it is much doubted by ſome modern chymiſts to what ſort of ſalt, that which is predominant in quick-lime belongs, we have been perſuaded to refer it rather to lixiviate than acid ſalts; and having obſerved, that though an evaporated infuſion of it will ſcarce yield ſuch a ſalt, as aſhes and other alcalizate bodies are wont to do, yet if we deprive our nephritic tincture of its blueneſs by juſt ſo much diſtilled vinegar as is requiſite to make that colour vaniſh, the lixivium of quick-lime will immediately upon its affuſion recall the baniſhed colour, but not ſo powerfully as either of the ſulphureous liquors formerly mentioned. And therefore I allow myſelf to gueſs at the ſtrength of the liquors examined by this experiment, by the quantity of them which is ſufficient to deſtroy or reſtore the ceruleous colour of our tincture. But whether concerning liquors, wherein neither acid nor alcalizate ſalts are eminently predominant, our tincture will enable us to conjecture any thing more than that ſuch ſalts are not predominant in them, I take not upon me to determine here, but leave to further trial; for I find not that ſpirit of wine, ſpirit of tartar freed from acidity, or chymical oil of turpentine (although liquors which muſt be conceived very ſaline, if chymiſts have, which is here no place to diſpute, rightly aſcribed taſtes to the ſaline principle of bodies) have any remarkable power either to deprive our tincture of its ceruleous colour, or reſtore it, when upon the affuſion of ſpirit of vinegar it has diſappeared.

EXPERIMENT XI.

AND here I muſt not omit, *Pyrophilus*, to inform you, that we can ſhew you even in a mineral body ſomething that may ſeem very near of kin to the changeable quality of the tincture of *Lignum Nephriticum*; for we have ſeveral flat pieces of glaſs, of the thickneſs of ordinary panes for windows, one of which being interpoſed betwixt the eye and a clear light, appears of a golden colour, not much unlike that of the moderate tincture of our wood; but being ſo looked upon as that the beams of light are not ſo much trajected through it as reflected from it to the eye, that yellow ſeems to degenerate into a pale blue, ſomewhat like that of a turquoiſe. And that which may alſo appear ſtrange, is this, that if in a certain poſture you hold one of theſe plates perpendicular to the horizon, ſo that the ſun-beams ſhine upon half of it, the other half being ſhaded, you may ſee that the part ſhined upon will be of a much diluter yellow than the ſhaded part, which will appear more richly coloured; and if you alter the poſture of the glaſs, ſo that it be not held perpendicular, but parallel in reference to the horizon, you may ſee (which perhaps you will admire) the ſhaded part look of a golden colour, but the other that the ſun ſhines freely on, will appear conſiderably blue, and as you remove any part of the glaſs thus held horizontally into the ſun-beams or ſhade, it will in the twinkling of an eye ſeem to paſs from one of the above mentioned colours to the other; the ſun-beams trajected through it upon a ſheet of white paper held near it, do colour it with yellow, ſomewhat bordering upon a red, but yet the glaſs may be ſo oppoſed to the ſun, that it may upon paper project a mixed colour here and there more inclined to yellow, and here and there more to blue. The other phænomena of this odd glaſs, I fear it would be ſcarce worth while to record; and therefore I ſhall rather advertiſe you, firſt, that in the trying of theſe experiments with it, you muſt take notice that one of the ſides has either alone, or at leaſt principally, its ſuperficial parts diſpoſed to the reflection of the blue colour above named, and that therefore you muſt have a care to keep that ſide neareſt to the eye. And next, that we have ourſelves made glaſſes not unfit to exhibit an experiment not unlike that I have been ſpeaking of, by laying upon pieces of glaſs ſome very finely foliated ſilver, and giving it by degrees a much ſtronger fire than is requiſite or uſual for the tinging of glaſſes of other colours. And this experiment, not to mention that it was made without a furnace, in which artificers that paint glaſs are wont to be very curious, is the more conſiderable, becauſe, that though a ſkilful painter could not deny to me that it was with ſilver he coloured his glaſſes yellow; yet he told me, that when to burn them (as they ſpeak) he lays on the plates of glaſs nothing but a calx of ſilver calcined without corroſive liquors, and tempered with fair water, the plates are tinged of a fine yellow that looks of a golden colour, which part ſoever of it you turn to or from the light; whereas (whether it be what an artificer would call over-doing, or burning, or elſe the employing the ſilver crude that makes the difference) we have found more than once, that ſome pieces of glaſs prepared as we have related, though held againſt the light they appeared of a tranſparent yellow, yet looked on with one's back turned to the light, they exhibited an untranſparent blue.

EXPERIMENT XII.

IF you will allow me, *Pyrophilus*, for the avoiding of ambiguity, to employ the word pigments, to ſignify ſuch prepared materials (as cochineal, vermilion, orpiment) as painters, dyers, and other artificers make uſe of to impart or imitate particular colours; I ſhall be the better underſtood in divers paſſages of the following papers, and particularly when I tell you, that the mixing of pigments being no inconſiderable part of the painters art, it may ſeem an incroachment in me to meddle with it. But I think I may eaſily be excuſed (though I do not altogether paſs it by) if I reſtrain myſelf to the making of a tranſient mention of ſome few of their practices about this matter; and that only ſo far forth, as may warrant me to obſerve to you, that there are but few ſimple and primary colours (if I may ſo call them) from whoſe various compoſitions all the reſt do as it were reſult. For though painters can imitate the hues (though not always the ſplendor) of thoſe almoſt numberleſs differing colours that are to be met with in the works of nature, and of art, I have not yet found, that to exhibit this ſtrange variety they need imploy any more than white, and black, and red, and blue, and yellow; theſe five, variouſly compounded, and (if I may ſo ſpeak) decompounded, being ſufficient to exhibit a variety and number of colours, ſuch as thoſe that are altogether ſtrangers to the painters pallets can hardly imagine.

Thus (for inſtance) black and white differingly mixed, make a vaſt company of lighter and darker greys.

Blue and yellow make a huge variety of greens.

Red and yellow make orange-tawny.

RED with a little white makes a carnation.

RED with an eye of blue, makes a purple; and by theſe ſimple compoſitions again compounded among themſelves, the ſkilful painter can produce what kind of colour he pleaſes, and a great many more than we have yet names for. But, as I intimated above, it is not my deſign to proſecute this ſubject, though I thought it not unfit to take ſome notice of it, becauſe we may hereafter have occaſion to make uſe of what has been now delivered, to illuſtrate the generation of intermediate colours; concerning which we muſt yet ſubjoin this caution, that to make the rules about the emergency of colours fit to be relied upon, the corpuſcles whereof the pigments conſiſt muſt be ſuch as do not deſtroy one another's texture; for in caſe they do, the produced colour may be very different from that which would reſult from the mixture of other harmleſs pigments of the ſame colours, as I ſhall have occaſion to ſhew ere long.

EXPERIMENT XIII.

IT may alſo give much light to an inquirer into the nature of colours, to know that not only in green, but in many (if not all) other colours, the light of the ſun paſſing through diaphanous bodies of differing hues may be tinged of the ſame compound colour, as if it came from ſome painters colours of the ſame denomination, though this latter be exhibited by reflection, and be (as the former experiment declares) manifeſtly compounded of material pigments. Wherefore to try the compoſition of colours by trajection, we provided ſeveral plates of tinged glaſs, which being laid two at a time, one on the top of another, the object looked upon through them both, appeared of a compounded colour, which agrees well with what we have obſerved in the ſecond experiment, of looking againſt the light through differingly coloured papers. But we thought the experiment would be more ſatisfactory, if we procured the ſun-beams to be ſo tinged in their paſſage through plates of glaſs, as to exhibit the compounded colour upon a ſheet of white paper. And though by reaſon of the thickneſs of the glaſſes, the effect was but faint, even when the ſun was high and ſhined forth clear, yet, we eaſily remedied that by contracting the beams we caſt on them by means of a convex burning-glaſs, which, where it made the beams much converge, increaſed the light enough to make the compounded colour very manifeſt upon the paper. By this means we obſerved, that the beams trajected through blue and yellow compoſed a green; that an intenſe and moderate red did with yellow make differing degrees of ſaffron, and orange-tawny colours; that green and blue made a colour partaking of both, ſuch as that which ſome Latin writers call *Pavonaceus*; that red and blue made a purple; to which we might add other colours, that we produced by the combinations of glaſſes differingly tinged, but that I want proper words to expreſs them in our language, and had not, when we made the trials, the opportunity of conſulting with a painter, who perchance might have ſupplied me with ſome of the terms I wanted.

I KNOW not whether it will be requiſite to ſubjoin on this occaſion, what I tried concerning reflections from coloured glaſſes, and other tranſparent bodies; namely, that having expoſed four or five ſorts of them to the ſun, and caſt the reflected beams upon white paper held near at hand, the light appeared not manifeſtly tinged, but as if it had been reflected from the impervious parts of a colourleſs glaſs; only that reflected from the yellow was here and there ſtained with the ſame colour, as if thoſe beams were not all reflected from the ſuperficial, but ſome from the internal parts of the glaſs; upon which occaſion you may take notice, that a ſkilful tradeſman, who makes ſuch coloured glaſs, told me, that whereas the red pigment was but ſuperficial, the yellow penetrated to the very midſt of the plate. But for further ſatisfaction, not having the opportunity to foliate thoſe plates, and ſo turn them into looking-glaſſes, we foliated a plate of *Muſcovy* glaſs, and then laying on it a little tranſparent varniſh of a gold colour, we expoſed it to the ſun-beams, ſo as to caſt them upon a body fit to receive them; on which the reflected light appearing, as we expected, yellow, manifeſted that rebounding from the ſpecular part of the ſelenitis, it was tinged in its return with the colour of the tranſparent varniſh through which it paſſed.

EXPERIMENT XIV.

AFTER what we have ſaid of the compoſition of colours, it will now be ſeaſonable to annex ſome experiments that we made in favour of thoſe colours, that are taught in the ſchools not to be real, but only apparent and fantaſtical; for we found by trials, that theſe colours might be compounded, both with true and ſtable colours, and with one another, as well as unqueſtionably genuine and laſting colours, and that the colours reſulting from ſuch compoſitions, would reſpectively deſerve the ſame denominations.

For firſt, having by the trajection of the ſun-beams through a glaſs priſm thrown an iris on the floor, I found that by placing a blue glaſs at a convenient diſtance betwixt the priſm and the iris, that part of the iris that was before yellow, might be made to appear green, though not of a graſs green, but of one more dilute and yellowiſh. And it ſeems not improbable, that the narrow greeniſh liſt (if I may ſo call it) that is wont to be ſeen between the yellow and blue parts of the iris, is made by the confuſion of thoſe two bordering colours.

Next, I found, that though the want of a ſufficient livelineſs in either of the compounding colours, or a light error in the manner of making the following trials, was enough to render ſome of them unſucceſsful, yet, when all neceſſary circumſtances were duly obſerved, the event was anſwerable to our expectation and deſire.

And (as I formerly noted) that red and blue compound a purple, ſo I could produce this laſt named colour, by caſting at ſome diſtance from the glaſs the blue part of the priſmatical iris (as I think it may be called for diſtinction ſake) upon a lively red (or elſe the experiment ſucceeds not ſo well.) And I remember, that ſometimes when I tried this upon a piece of red cloth, that part of the iris which would have been blue, (as I tried by covering that part of the cloth with a piece of white paper) and compounded with the red, wherewith the cloth was imbued before, appeared of a fair purple, did, when I came to view it near at hand, look very oddly, as if there were ſome ſtrange reflection or refraction, or both, made in the hairs of which that cloth was compoſed.

Casting likewiſe the priſmatical iris upon a very vivid blue, I found that part of it, which would elſe have been the yellow, appear green. (Another ſomewhat differing trial, and yet fit to confirm this, you will find in the fifteenth experiment.)

But it may ſeem ſomewhat more ſtrange, that though the priſmatical iris being made by the refraction of light through a body that has no colour at all, muſt according to the doctrine of the ſchools, conſiſt of as purely emphatical colours as may be, yet even theſe may be compounded with one another, as well as real colours in the groſſeſt pigments. For I took at once two triangular glaſſes, and one of them being kept fixt in the ſame poſture, that the iris it projected on the floor might not waver, I caſt on the ſame floor another iris with the other priſm, and moving it to and fro to bring what part of the ſecond iris I pleaſed, to fall upon what part of the firſt I thought fit, we did ſometimes (for a ſmall error ſuffices to hinder the ſucceſs) obtain by this means a green colour in that part of the more ſtable iris, that before was yellow, or blue; and frequently by caſting thoſe beams, that in one of the iriſes made the blue upon the red parts of the other iris, we were able to produce a lovely purple, which we can deſtroy or recompoſe at pleaſure, by ſevering and re-approaching the edges of the two iriſes.

EXPERIMENT XV.

ON this occaſion, *Pyrophilus*, I ſhall add, that finding the glaſs priſm to be the uſefulleſt inſtrument men have yet employed about the contemplation of colours, and conſidering that priſms, hitherto in uſe, are made of glaſs tranſparent and colourleſs, I thought it would not be amiſs to try, what change the ſuperinduction of a colour, without the deſtruction of the diaphaneity, would produce in the colours exhibited by the priſm. But being unable to procure one to be made of coloured glaſs, and fearing alſo that if it were not carefully made, the thickneſs of it would render it too opacous, I endeavoured to ſubſtitute one made of clarified roſin, or of turpentine brought (as I elſewhere teach) to the conſiſtence of a tranſparent gum. But though theſe endeavours were not wholly loſt, yet we found it ſo difficult to give theſe materials their true ſhape, that we choſe rather to varniſh over an ordinary priſm with ſome of thoſe few pigments that are to be had tranſparent; as accordingly we did firſt with yellow, and then with red, or rather crimſon, made with lake tempered with a convenient oil; and the event was, that for want of good tranſparent colours (of which you know there are but very few) both the yellow and the red made the glaſs ſo opacous (though the pigment were laid on but upon two ſides of the glaſs, no more being abſolutely neceſſary) that unleſs I looked upon an enlightened window, or the flame of a candle, or ſome other luminous or very vivid object, I could ſcarce diſcern any colours at all, eſpecially when the glaſs was covered with red. But when I did look on ſuch objects, it appeared (as I expected) that the colour of the pigment had vitiated or drowned ſome of thoſe which the priſm would, according to its wont, have exhibited, and mingling with others, altered them: as I remember, that both to my eyes, and others to whom I ſhewed it, when the priſm was covered with yellow, it made thoſe parts of bright objects, where the blue would elſe have been conſpicuous, appear of a light green. But, *Pyrophilus*, both the nature of the colours, and the degree of tranſparency, or of darkneſs in the pigment, beſides divers other circumſtances, did ſo vary the phænomena of theſe trials, that till I can procure ſmall coloured priſms, or hollow ones that may be filled with tincted liquor, or obtain ſome better pigments than thoſe I was reduced to employ, I ſhall forbear to build any thing upon what has been delivered, and ſhall make no other uſe of it, than to invite you to proſecute the enquiry further.

EXPERIMENT XVI.

AND here, *Pyrophilus*, ſince we are treating of emphatical colours, we ſhall add what we think not unworthy your obſervation, and not unfit to afford ſome exerciſe to the ſpeculative. For there are ſome liquors which, though colourleſs themſelves, when they come to be elevated, and diſperſed into exhalations, exhibit a conſpicuous colour, which they loſe again, when they come to be reconjoined into a liquor; as good ſpirit of nitre, or upon its account ſtrong aqua fortis, though devoid of all appearance of redneſs whilſt they continue in the form of a liquor, if a little heat chance to turn the minute parts of them into vapours, the ſteam will appear of a reddiſh or deep yellow colour, which will vaniſh when thoſe exhalations come to reſume the form of a liquor.

And not only if you look upon a glaſs half full of aqua fortis, or ſpirit of nitre, and half full of nitrous ſteams proceeding from it, you will ſee the upper part of the glaſs of the colour freſhly mentioned, if through it you look upon the light: but which is much more conſiderable, I have tried, that putting aqua fortis in a long clear glaſs, and adding a little copper or ſome ſuch open metal to it, to excite heat and fumes, the light trajected through thoſe fumes, and caſt upon a ſheet of white paper, did upon that appear of the colour that the fumes did, when directly looked upon, as if the light were as well tinged in its paſſage through theſe fumes, as it would have been by paſſing through ſome glaſs or liquor in which the ſame colour was inherent.

To which I ſhall further add, that having ſometimes had the curioſity to obſerve whether the beams of the ſun near the horizon, trajected through a very red ſky, would not (though ſuch redneſſes are taken to be but emphatical colours) exhibit the like colour; I found that the beams falling within a room upon a very white object, placed directly oppoſite to the ſun, diſcloſed a manifeſt redneſs, as if they had paſſed through a coloured medium.

EXPERIMENT XVII.

THE emergency, *Pyrophilus*, of colours upon the coalition of the particles of ſuch bodies as were neither of them of the colour of that mixture whereof they are the ingredients, is very well worth our attentive obſervation, as being of good uſe both ſpeculative and practical: for much of the mechanical uſe of colours among painters and dyers doth depend upon the knowledge of what colours may be produced by the mixtures of pigments ſo and ſo coloured. And (as we lately intimated) it is of advantage to the contemplative Naturaliſt, to know how many and which colours are primitive (if I may ſo call them) and ſimple, becauſe it both eaſes his labour by confining his moſt ſolicitous enquiry to a ſmall number of colours upon which the reſt depend, and aſſiſts him to judge of the nature of particular compounded colours, by ſhewing him, from the mixture of what more ſimple ones, and of what proportions of them to one another, the particular colour to be conſidered does reſult. But becauſe, to inſiſt on the proportions, the manner and the effects of ſuch mixtures, would oblige me to conſider a greater part of the painter's art and dyer's trade, than I am well acquainted with, I confined myſelf to make trial of ſeveral ways to produce green, by the compoſition of blue and yellow: and ſhall in this place both recapitulate moſt of the things I have diſperſedly delivered already concerning that ſubject, and recruit them.

And firſt, whereas painters (as I noted above) are wont to make green by tempering blue and yellow, both of them made into a ſoft conſiſtence, with either water or oil, or ſome liquor of kin to one of thoſe two, according as the picture is to be drawn with thoſe they call water-colours, or thoſe they term oil-colours; I found, that by chuſing fit ingredients, and mixing them in the form of dry powders, I could do, what I could not if the ingredients were tempered up with a liquor: but the blue and yellow powders muſt not only be finely ground, but ſuch as that the corpuſcles of the one may not be too unequal to thoſe of the other, leſt by the diſproportionate minuteneſs the ſmaller cover and hide the greater. We uſed with good ſucceſs a ſlight mixture of the fine powder of biſe, with that of orpiment, or that of good yellow oker; I ſay, a ſlight mixture, becauſe we found that an exquiſite mixture did not do ſo well: but by lightly mingling the two pigments in ſeveral little parcels, thoſe of them in which the proportion and manner of mixture was more lucky, afforded us a good green.

2. WE alſo learned in the dye-houſes, that cloth being dyed blue with woad, is afterwards by the yellow decoction of woud-wax or wood-wax dyed into a green colour.

3. YOU may alſo remember what we above related, where we intimated, that having in a darkened room taken two bodies, a blue and a yellow, and caſt the light reflected from the one upon the other, we likewiſe obtained a green.

4. AND you may remember, that we obſerved a green to be produced, when in the ſame darkened room we looked at the hole at which alone the light entered, through the green and yellow parts of a ſheet of marbled paper laid over one another.

5. WE found too, that the beams of the ſun being trajected through two pieces of glaſs, the one blue and the other yellow, laid over one another, did upon a ſheet of white paper, on which they were made to fall, exhibit a lovely green.

6. I HOPE alſo, that you have not already forgot, what was ſo lately delivered, concerning the compoſition of a green, with a blue and yellow: of which moſt authors would call the one a real, and the other an emphatical.

7. AND I preſume, you may have yet freſh in your memory, what the fourteenth experiment informs you, concerning the exhibiting of a green, by the help of a blue and yellow, that were both of them emphatical.

8. WHEREFORE we will proceed to take notice, that we alſo deviſed a way of trying whether or no metalline ſolutions, though one of them at leaſt had its colour adventitious, by the mixture of the menſtruum employed to diſſolve it, might not be made to compound a green after the manner of other bodies. And though this ſeemed not eaſy to be performed by reaſon of the difficulty of finding metalline ſolutions of the colour requiſite, that would mix without precipitating each other; yet after a while having conſidered the matter, the firſt trial afforded me the following experiment. I took a high yellow ſolution of good gold in aqua regis (made of aqua fortis, and as I remember half its weight of ſpirit of ſalt); to this I put a due proportion of a deep and lovely blue ſolution of crude copper (which I have elſewhere taught to be readily diſſolvable in ſtrong ſpirit of urine.) And theſe two liquors, though at firſt they ſeemed a little to curdle one another, yet being thoroughly mingled by ſhaking, they preſently, as had been conjectured, united into a tranſparent green liquor, which continued ſo for divers days, that I kept it in a ſmall glaſs wherein it was made, only letting fall a little blackiſh powder to the bottom. The other phænomena of this experiment belong not to this place, where it may ſuffice to take notice of the production of a green, and that the experiment was more than once repeated with ſucceſs.

9. AND laſtly, to try whether this way of compounding colours would hold even in ingredients actually melted by the violence of the fire, provided their texture were capable of ſafely enduring fuſion, we cauſed ſome blue and yellow ammel to be long and well wrought together in the flame of a lamp, which being ſtrongly and inceſſantly blown on them, kept them in ſome degree of fuſion, and at length (for the experiment requires ſome patience as well as ſkill) we obtained the expected ammel of a green colour.

I KNOW not, *Pyrophilus*, whether it be worth while to acquaint you with the ways that came into my thoughts, whereby in ſome meaſure to explicate the firſt of the mentioned ways of making a green; for I have ſometimes conjectured, that the mixture of the biſe and the orpiment produced a green by ſo altering the ſuperficial aſperity, which each of thoſe ingredients had apart, that the light incident on the mixture was reflected with differing ſhades, as to quantity, or order, or both, from thoſe of either of the ingredients, and ſuch as the light is wont to be modified with, when it reflects from graſs, or leaves, or ſome of thoſe other bodies that we are wont to call green. And ſometimes too I have doubted, whether the produced green might not be partly at leaſt derived from this, that the beams that rebound from the corpuſcles of the orpiment, giving one kind of ſtroke upon the retina, whoſe perception we call yellow, and the beams reflected from the corpuſcles of the biſe giving another ſtroke upon the ſame retina, like to objects that are blue; the contiguity and minuteneſs of theſe corpuſcles may make the appulſe of the reflected light fall upon the retina within ſo narrow a compaſs, that the part they beat upon being as it were a phyſical point, they may give a compounded ſtroke, which may conſequently exhibit a compounded and new kind of ſenſation: as we ſee that two ſtrings of a muſical inſtrument being ſtruck together, making two noiſes that arrive at the ear at the ſame time as to ſenſe, yield a ſound differing from either of them, and as it were compounded of both; inſomuch that if they be diſcordantly tuned, though each of them ſtruck apart would yield a pleaſing ſound, yet being ſtruck together they make but a harſh and troubleſome noiſe. But this not being ſo fit a place to proſecute ſpeculations, I ſhall not inſiſt, neither upon theſe conjectures nor any others, which the experiment we have been mentioning may have ſuggeſted to me. And I ſhall leave it to you, *Pyrophilus*, to derive what inſtruction you can from comparing together the various ways whereby a yellow and a blue can be made to compound a green: that which I now pretend to, being only to ſhew that the firſt of thoſe mentioned ways (not to take at preſent notice of the reſt) does far better agree with our conjectures about colours, than either with the doctrine of the ſchools, or with that of the chymiſts, both which ſeem to be very much diſfavoured by it.

FOR firſt, ſince in the mixture of the two mentioned powders I could by the help of a very excellent microſcope (for ordinary ones will ſcarce ſerve the turn) diſcover that which ſeemed to the naked eye a green body, to be but a heap of diſtinct, though very ſmall grains of yellow orpiment and blue biſe confuſedly enough blended together, it appears that the coloured corpuſcles of either kind did each retain its own nature and colour; by which it may be gueſſed, what meer tranſpoſition and juxtapoſition of minute and ſingly unchanged particles of matter can do to produce a new colour. For that this local motion and new diſpoſition of the ſmall parts of the orpiment did intervene, is much more manifeſt than it is eaſy to explicate how they ſhould produce this new green, otherwiſe than by the new manner of their being put together, and conſequently by their new diſpoſition to modify the incident light, by reflecting it otherwiſe than they did before they were mingled together.

SECONDLY, The green thus made, being (if I may ſo ſpeak) mechanically produced, there is no pretence to derive it from I know not what incomprehenſible ſubſtantial form, from which yet many would have us believe that colours muſt flow; nor does this green, though a real and permanent, not a phantaſtical and vanid colour, ſeem to be ſuch an inherent quality as they would have it, ſince not only each part of the mixture remains unaltered in colour, and conſequently of a differing colour from the heap they compoſe; but if the eye be aſſiſted by a microſcope to diſcern

cern things better and more diſtinctly than before it could, it ſees not a green body, but a heap of blue and yellow corpuſcles.

AND in the third place, I demand what either ſulphur, or ſalt, or mercury has to do in the production of this green; for neither the biſe nor the orpiment were endued with that colour before; and the bare juxtapoſition of the corpuſcles of the two powders that work not upon each other, but might, if we had convenient inſtruments, be ſeparated, unaltered, cannot, with any probability, be imagined either to increaſe or diminiſh any of the three hypoſtatical principles (to which of them ſoever the chymiſts are pleaſed to aſcribe colours); nor does there here intervene ſo much as heat to afford them any colour to pretend, that at leaſt there is made an extraverſion (as the Helmontians ſpeak) of the ſulphur, or of any of the two other ſuppoſed principles. But upon this experiment we have already reflected enough, if not more than enough for once.

EXPERIMENT XVIII.

BUT here, *Pyrophilus*, I muſt advertiſe you, that it is not every yellow and every blue that, being mingled, will afford a green; for in caſe one of the ingredients do not act only as endowed with ſuch a colour, but as having a power to alter the texture of the corpuſcles of the other, ſo as to indiſpoſe them to reflect the light, as corpuſcles that exhibit a blue or a yellow are wont to reflect it; the emergent colour may be not green, but ſuch as the change of texture in the corpuſcles of one or both of the ingredients qualifies them to ſhew forth: as for inſtance, if you let fall a few drops of ſyrup of violets upon a piece of white paper, though the ſyrup being ſpread will appear blue, yet mingling with it two or three drops of the lately mentioned ſolution of gold, I obtained not a green but a reddiſh mixture, which I expected from the remaining power of the acid ſalts abounding in the ſolution, ſuch ſalts or ſaline ſpirits being wont, as we ſhall ſee anon, though weakened, ſo to work upon that ſyrup as to change it into a red or reddiſh colour. And to confirm that for which I allege the former experiment, I ſhall add this other, that having made a very ſtrong and high-coloured ſolution of filings of copper with ſpirit of urine, though the menſtruum ſeemed glutted with the metal, becauſe I put in ſo much filings, that many of them remained for divers days undiſſolved at the bottom; yet having put three or four drops of ſyrup of violets upon white paper, I found that the deep blue ſolution proportionably mingled with this other blue liquor, did not make a blue mixture, but, as I expected, a fair green, upon the account of the urinous ſalt that was in the menſtruum.

EXPERIMENT XIX.

TO ſhew the chymiſts, that colours may be made to appear or vaniſh, where there intervenes no acceſſion or change either of the ſulphureous, or the ſaline, or the mercurial principle (as they ſpeak) of bodies; I ſhall not make uſe of the iris afforded by the glaſs-priſm, nor of the colours to be ſeen in a fair morning in thoſe drops of dew that do in a convenient manner reflect and refract the beams of light to the eye: but I will rather mind them of what they may obſerve in their own laboratories, namely, that divers, if not all, chymical eſſential oils, as alſo good ſpirit of wine, being ſhaken till they have good ſtore of bubbles, thoſe bubbles will (if attentively conſidered) appear adorned with various and lovely colours, which all immediately vaniſh, upon the relapſing of the liquor that affords thoſe bubbles their ſkins, into the reſt of the oil, or ſpirit of wine; ſo that a colourleſs liquor may be made in a trice to exhibit variety of colours, and may loſe them in a moment without the acceſſion or diminution of any of its hypoſtatical principles. And, by the way, it is not unworthy our notice, that ſome bodies, as well colourleſs as coloured, by being brought to a great thinneſs of parts, acquire colours though they had none before, or colours differing from them they were before endued with: for, not to inſiſt on the variety of colours, that water, made ſomewhat glutinous by ſoap, acquires when it is blown into ſuch ſpherical bubbles as boys are wont to make and play with; turpentine (though it have a colour deep enough of its own) may (by being blown into after a certain manner) be brought to afford bubbles adorned with variety of orient colours, which though they vaniſh ſome while upon the breaking of the bubbles, yet they would in all likelihood always exhibit colours upon their ſuperficies (though not always the ſame in the ſame parts of them, but varied according to the incidence of the ſight, and the poſition of the eye) if their texture were durable enough. For I have ſeen one that was ſkilled at faſhioning glaſſes by the help of a lamp, blowing ſome of them ſo ſtrongly as to burſt them; whereupon it was found, that the tenacity of the metal was ſuch, that before it broke, it ſuffered itſelf to be reduced into films ſo extremely thin, that being kept clean they conſtantly ſhewed on their ſurfaces (but after the manner newly mentioned) the varying colours of the rainbow, which were exceedingly vivid, as I had often opportunity to obſerve in ſome, that I cauſed purpoſely to be made, to keep by me.

But leſt it ſhould be objected, that the above-mentioned inſtances are drawn from tranſparent liquors, it may poſſibly appear not impertinent to add, what I have ſometimes thought upon, and ſeveral times tried, when I was conſidering the opinions of the chymiſts about colours. I took then a feather of a convenient bigneſs and ſhape, and holding it at a fit diſtance betwixt my eye and the ſun when he was near the horizon, methought there appeared to me a variety of little rainbows, with differing and very vivid colours, of which none was conſtantly to be ſeen in the feather; the like phænomenon I have at other times (though not with altogether ſo good ſucceſs) produced, by interpoſing at a due diſtance a piece of black ribband betwixt the almoſt ſetting ſun and my eye; not to mention the trials I have made to the ſame purpoſe, with other bodies.

EXPERIMENT XX.

Take good ſyrup of violets, impregnated with the tincture of the flowers, drop a little of it upon a white paper (for by that means the change of colour will be more conſpicuous, and the experiment may be practiſed in ſmaller quantities) and on this liquor let fall two or three drops of ſpirit either of ſalt or vinegar, or almoſt any other eminently acid liquor, and upon the mixture of theſe you ſhall find the ſyrup immediately turned red: and the way of effecting ſuch a change has not been unknown to divers perſons, who have produced the like, by ſpirit of vitriol, or juice of lemons, but have groundleſsly aſcribed the effect to ſome peculiar quality of thoſe two liquors, whereas (as we have already intimated) almoſt any acid ſalt will turn ſyrup of violets red. But to improve the experiment, let me add what has not (that I know of) been hitherto obſerved, and has, when we firſt ſhewed it them, appeared ſomething ſtrange, even to thoſe that have been inquiſitive into the nature of colours; namely, that if inſtead of ſpirit of ſalt, or that of vinegar, you drop upon the ſyrup of violets a little oil of tartar *per deliquium*, or the like quantity of ſolution of pot-aſhes, and rub them together with your finger, you ſhall find the blue colour of the ſyrup turned in a moment into a perfect green; and the like may be performed by divers other liquors, as we may have occaſion elſewhere to inform you.

Annotation upon the Twentieth EXPERIMENT.

THE uſe of what we lately delivered concerning the way of turning ſyrup of violets red or green, may be this; that, though it be a far more common and procurable liquor than the infuſion of *lignum nephriticum*, it may yet be eaſily ſubſtituted in its room, when we have a mind to examine, whether or no the ſalt predominant in a liquor or other body, wherein it is looſe and abundant, belong to the tribe of acid ſalts or not. For if ſuch a body turn the ſyrup of a red or reddiſh purple colour, it does for the moſt part argue the body (eſpecially if it be a diſtilled liquor) to abound with acid ſalt. But if the ſyrup be made green, that argues the predominant ſalt to be of a nature repugnant to that of the tribe of acids. For, as I find that either ſpirit of ſalt, or oil of vitriol, or aqua fortis, or ſpirit of vinegar, or juice of lemons, or any of the acid liquors I have yet had occaſion to try, will turn ſyrup of violets of a red, or at leaſt of a reddiſh colour; ſo I have found, that not only the volatile ſalts of all animal ſubſtances I have uſed, as ſpirit of hartſhorn, of urine, of ſal armoniac, of blood, *&c.* but alſo all the alcalizate ſalts I have employed, as the ſolution of ſalt of tartar, of pot-aſhes, of common wood-aſhes, lime-water, *&c.* will immediately change the blue ſyrup into a perfect green. And by the ſame way (to hint that upon the by) I elſewhere ſhow you, both the changes that nature and time produce, in the more ſaline parts of ſome bodies, may be diſcovered, and alſo how even ſuch chymically prepared bodies, as belong not either to the animal kingdom, or to the tribe of alcalies, may have their new and ſuperinduced nature ſucceſsfully examined. In this place I ſhall only add, that not alone the changing the colour of the ſyrup requires, that the changing body be more ſtrong of the acid, or other ſort of ſalt, that is predominant in it, than is requiſite for the working upon the tincture of *lignum nephriticum*; but that in this alſo, the operation of the formerly mentioned ſalts upon our ſyrup, differs from their operation upon our tinctures; that in this liquor, if the ceruleous colour be deſtroyed by an acid ſalt, it may be reſtored by one that is either volatile, or lixiviate; whereas in ſyrup of violets, though one of theſe contrary ſalts will deſtroy the action of the other, yet neither of them will reſtore the ſyrup to its native blue; but each of them will change it into the colour which itſelf doth (if I may ſo ſpeak) affect, as we ſhall have occaſion to ſhew in the notes on the twenty-fifth experiment.

EXPERIMENT XXI.

THERE is a weed, more known to plowmen than beloved by them, whoſe flowers from their colour are commonly called blue-bottles, and corn-weed from their growing among corn. Theſe flowers, ſome ladies do, upon the account of their lovely colour, think worth the being candied, which when they are, they will long retain ſo fair a colour, as makes them a very fine ſallad in the winter. But I have tried, that when they are freſhly gathered, they will afford a juice, which when newly expreſſed (for in ſome caſes it will ſoon enough degenerate) affords a very deep and pleaſant blue. Now (to draw this to our preſent ſcope) by dropping on this freſh

Herbariſts *are wont to call this plant* Cyanus vulgaris minor.

juice a little ſpirit of ſalt (that being the acid ſpirit I had then at hand) it immediately turned (as I predicted) into a red. And if, inſtead of the ſour ſpirit, I mingled with it a little ſtrong ſolution of an alcalizate ſalt, it did preſently diſcloſe a lovely green; the ſame changes being, by thoſe differing ſorts of ſaline liquors, producible in this natural juice, that we lately mentioned to have happened to that factitious mixture, the ſyrup of violets. And I remember, that finding this blue liquor, when freſhly made, to be capable of ſerving in a pen for an ink of that colour, I attempted by moiſtening one part of a piece of white paper with the ſpirit of ſalt I have been mentioning, and another with ſome alcalizate or volatile liquor, to draw a line on the leiſurely dried paper, that ſhould even before the ink was dry appear partly blue, partly red, and partly green: but though the latter part of the experiment ſucceeded not well (whether becauſe volatile ſalts are too fugitive to be retained in the paper, and alcalizate ones are too unctuous, or ſo apt to draw moiſture from the air, that they keep the paper from drying well) yet the former part ſucceeded well enough; the blue and the red being conſpicuous enough to afford a ſurprizing ſpectacle to thoſe, I acquaint not with (what I willingly allow you to call) the trick.

Annotation upon the one and twentieth EXPERIMENT.

BUT leſt you ſhould be tempted to think *(Pyrophilus)* that volatile or alcalizate ſalts change blues into green, rather upon the ſcore of the eaſy tranſition of the former colour into the latter, than upon the account of the texture, wherein moſt vegetables, that afford a blue, ſeem, though otherwiſe differing, to be allied; I will add, that when I purpoſely diſſolved blue vitriol in fair water, and thereby imbued ſufficiently that liquor with that colour, a lixiviate liquor, and a urinous ſalt being copiouſly poured upon diſtinct parcels of it, did each of them, though perhaps with ſome difference, turn the liquor not green, but of a deep yellowiſh colour, almoſt like that of yellow oker; which colour, the precipitated corpuſcles retained, when they had leiſurely ſubſided in the bottom. What this precipitated ſubſtance is, it is not needful now to inquire in this place, and in another I have ſhewn you, that notwithſtanding its colour, and its being obtainable from an acid menſtruum by the help of ſalt of tartar, it is yet far enough from being the true ſulphur of vitriol.

EXPERIMENT XXII.

OUR next experiment *(Pyrophilus)* will perhaps ſeem to be of a contrary nature to the two former, made upon ſyrup of violets and juice of blue-bottles. For as in them, by affuſion of oil of tartar, a blueiſh liquor is made green, ſo in this, by the ſole mixture of the ſame oil, a greeniſh liquor becomes blue. The hint of this experiment was given us by the practice of ſome Italian painters, who being wont to counterfeit *Ultra-marine Azure* (as they call it) by grinding verdigreaſe with ſal-armoniac, and ſome other ſaline ingredients, and letting them rot (as they imagine) for a good while together in a dunghill, we ſuppoſed that the change of colour wrought in the verdigreaſe by this way of preparation muſt proceed from the action of certain volatile and alcalizate ſalts, abounding in ſome of the mingled concretes, and brought to make a further diſſolution of the copper abounding in the verdigreaſe; and therefore we conjectured, that if both the verdigreaſe, and ſuch ſalts were diſſolved in fair water, the ſmall parts of both being therein more ſubdivided and ſet at liberty, would have better acceſs to each other, and thereby incorporate much the more ſuddenly.

And accordingly we found, that if upon a ſtrong ſolution of good French verdigreaſe (for it is that we are wont to employ, as the beſt) you pour a juſt quantity of oil of tartar, and ſhake them well together, you ſhall immediately ſee a notable change of colour, and the mixture will grow thick, and not tranſparent; but if you ſtay a while, till the groſſer part be precipitated to, and ſettled in the bottom, you may obtain a clear liquor of a very lovely colour, and exceeding delightful to the eye. But, you muſt have a care to drop in a competent quantity of oil of tartar, for elſe the colour will not be ſo deep and rich; and if inſtead of this oil you employ a clear lixivium of pot-aſhes, you may have an azure ſomewhat lighter or paler than, and therefore differing from, the former. And if inſtead of either of theſe liquors, you make uſe of ſpirit of urine, or of hartſhorn, you may, according to the quantity and quality of the ſpirit you pour in, obtain ſome further variety (though ſcarce conſiderable) of ceruleous liquors. And yet lately by the help of this urinous ſpirit we made a blue liquor, which not a few ingenious perſons, and among them, ſome, whoſe profeſſion makes them very converſant with colours, have looked upon with ſome wonder. But theſe azure-coloured liquors ſhould be freed from the ſubſiding matter, which the ſalts of tartar or urine precipitate out of them, rather by being decanted, than by filtration. For by the latter of theſe ways, we have ſometimes found the colour of them very much impaired, and little ſuperior to that of the groſſer ſubſtance, that is left in the filtre.

EXPERIMENT XXIII.

THAT roſes held over the fume of ſulphur, may quickly by it be deprived of their colour, and have as much of their leaves, as the fume works upon, burned pale, is an experiment, that divers others have tried, as well as I. But (*Pyrophilus*) it may ſeem ſomewhat ſtrange to one that has never conſidered the compounded nature of brimſtone, that whereas the fume of ſulphur will, as we have ſaid, whiten the leaves of roſes; that liquor, which is commonly called, oil of ſulphur *per campanam*, becauſe it is ſuppoſed to be made by the condenſation of theſe fumes, in glaſſes ſhaped like bells, into a liquor, does powerfully heighten the tincture of red roſes, and make it more red and vivid, as we have eaſily tried by putting ſome red-roſe leaves, that had been long dried (and ſo had loſt much of their colour) into a phial of fair water. For a while after the affuſion of a convenient quantity of the liquor we are ſpeaking of, both the leaves themſelves, and the water they were ſteeped in, diſcovered a very freſh and lovely colour.

EXPERIMENT XXIV.

IT may (*Pyrophilus*) ſomewhat ſerve to illuſtrate, not only the doctrine of pigments, and of colours, but divers other parts of the corpuſcular philoſophy, as that explicates odours, and many other things, not as the ſchools by airy qualities, but by real, though extremely minute bodies; to examine, how much of the colourleſs liquor a very ſmall parcel of a pigment may imbue with a diſcernable colour. And though there be ſcarce any thing of preciſeneſs to be expected from ſuch trials, yet I preſumed, that (at leaſt) I ſhould be able to ſhow a much further ſubdiviſion of the parts of matter into viſible particles, than I have hitherto found taken notice of, and than moſt men would imagine; nobody, that I know of, having yet attempted to reduce this matter to any meaſure.

The bodies, the moſt promiſing for ſuch a purpoſe, might ſeem to be the metals, eſpecially gold, becauſe of the multitude and minuteneſs of its parts, which might be argued from the incomparable cloſeneſs of its texture: but though we tried a ſolution of gold made in aqua regia firſt, and then in fair water, yet in regard we were to determine the pigment we employed, not by bulk, but weight, and becauſe alſo, that the yellow colour of gold is but a faint one in compariſon of the deep colour of cochineal, we rather choſe this to make our trials with. But among divers of theſe it would ſuffice to ſet down one, which was carefully made in veſſels conveniently ſhaped, (and that in the preſence of a witneſs, and an aſſiſtant) the ſum whereof I find among my *Adverſaria*, regiſtered in the following words. To which I ſhall only premiſe, (to leſſen the wonder of ſo ſtrange a diffuſion of the pigment) that cochineal will be better diſſolved, and have its colour far more heightened by ſpirit of urine, than (I ſay not by common water, but) by rectified ſpirit of wine itſelf.

The note I ſpoke of is this: [One grain of cochineal diſſolved in a pretty quantity of ſpirit of urine, and then diſſolved further by degrees in fair water, imparted a diſcernable, though but a very faint colour, to about ſix glaſs-fulls of water, each of them containing about forty three ounces and a half, which amounts to above a hundred twenty five thouſand times its own weight.]

EXPERIMENT XXV.

It may afford a conſiderable hint *(Pyrophilus)* to him, that would improve the art of dying, to know what change of colours may be produced by the three ſeveral ſorts of ſalts already often mentioned, (ſome or other of which may be procured in quantity at reaſonable rates) in the juices, decoctions, infuſions, and (in a word) the more ſoluble parts of vegetables. And, though the deſign of this diſcourſe be the improvement of knowledge, not of trades; yet thus much I ſhall not ſcruple to intimate here, that the blue liquors, mentioned in the twentieth and one and twentieth experiments, are far from being the only vegetable ſubſtances upon which acid, urinous, and alcalizate ſalts have the like operations to thoſe recited in thoſe two experiments. For ripe privet berries (for inſtance) being cruſhed upon white paper, though they ſtain it with a purpliſh colour, yet if we let fall on ſome part of it two or three drops of ſpirit of ſalt, and on the other part a little more of the ſtrong ſolution of pot-aſhes, the former liquor immediately turned that part of the thick juice or pulp, on which it fell, into a lovely red, and the latter turned the other part of it into a delightful green. Though I will not undertake, that thoſe colours in that ſubſtance ſhall not be much more orient than laſting; and though *(Pyrophilus)* this experiment may ſeem to be almoſt the ſame with thoſe already delivered concerning ſyrup of violets, and the juice of blue-bottles, yet I think it not amiſs to take occaſion to inform you, that this experiment reaches much farther than perhaps you yet imagine, and may be of good uſe to thoſe, whom it concerns to know how dying ſtuffs may be wrought upon by ſaline liquors. For, I have found this experiment to ſucceed in ſo many various berries, flowers, bloſſoms, and other finer parts of vegetables, that neither my memory, nor my leiſure ſerves me to enumerate them. And it is ſomewhat ſurprizing to ſee, by how differingly-coloured flowers, or bloſſoms, (for example) the paper being ſtained, will by an acid ſpirit be immediately turned red, and by any alcaly or any urinous ſpirit turned green; inſomuch that even the cruſhed bloſſoms of meſerion, (which I gathered in winter and froſty weather) and thoſe of peas, cruſhed upon white paper, how remote ſoever their colours be from green, would in a moment paſs into a deep degree of that colour, upon the touch of an alcalizate liquor. To which let us add, that either of thoſe new pigments (if I may ſo call them) may, by the affuſion of enough of a contrary liquor, be preſently changed from red into green, and from green into red: which obſervation will hold alſo in ſyrup of violets, juices of blue-bottles, *&c.*

ANNOTATION.

AFTER what I have formerly delivered to evince that there are many inſtances, wherein new colours are produced or acquired by bodies, which chymiſts are wont to think deſtitute of ſalt, or to whoſe change of colours no new acceſſion of ſaline particles does appear to contribute; I think we may ſafely enough acknowledge, that we have taken notice of ſo many changes made by the intervention of ſalts in the colours of mixed bodies, that it has leſſened our wonder, that though many chymiſts are wont to aſcribe the colours of ſuch bodies to their ſulphureous, and the reſt to their mercurial principle; yet *Paracelſus* himſelf directs us in the indagation of colours, to have an eye principally upon ſalts, as we find in that paſſage of his, wherein he takes upon him to oblige his readers much by inſtructing them, of what things they are to expect the knowledge from each of the three diſtinct principles of bodies. *Alias* (ſays he) *colorum ſimilis ratio eſt: de quibus brevem inſtitutionem hanc attendite, quod ſcilicet colores omnes ex ſale prodeant. Sal enim dat colorem, dat balſamum.* And a little beneath; *Jam natura ipſa colores protrahit ex ſale, cuique ſpeciei dans illum, qui ipſi competit, &c.* After which he concludes; *Itaque qui rerum omnium corpora cognoſcere, vult, huic opus eſt, ut ante omnia cognoſcat ſulphur; ab hoc, qui deſiderat noviſſe colores, is ſcientiam iſtorum petat à ſale; qui ſcire vult virtutes, is ſcrutetur arcana Mercurii. Sic nimirum fundamentum hauſerit myſteriorum, in quolibet creſcenti indagandorum, prout natura cuilibet ſpecie ea ingeſſit.* But though *Paracelſus* aſcribes to each of his beloved hypoſtatical principles much more than I fear will be found to belong to it; yet if we pleaſe to conſider colours, not as philoſophers, but as dyers, the concurrence of ſalts to the ſtriking and change of colours, and their efficacy, will, I ſuppoſe, appear ſo conſiderable, that we ſhall not need to quarrel much with *Paracelſus*, for aſcribing in this place (for I dare not affirm that he uſes to be ſtill of one mind) the colours of bodies to their ſalts, if by ſalts he here underſtood not only elementary ſalts, but ſuch alſo as are commonly taken for ſalts, as allom, cryſtals of tartar, vitriol, *&c.* becauſe the ſaline principle does chiefly abound in them, though indeed they be, as we elſewhere declare, mixed bodies, and have moſt of them, beſides what is ſaline, both ſulphureous, aqueous, and groſs or earthy parts.

Paracelſus *de Mineral. tract.* 1. *pag. m.* 342.

BUT though (*Pyrophilus*) I have obſerved a red and green to be produced, the former, by acid ſalts, the latter by ſalts not acid, in the expreſt juices of ſo many differing vegetable ſubſtances, that the obſervation, if purſued, may prove (as I ſaid) of good uſe: yet to ſhow you how much even theſe effects depend upon the particular texture of bodies, I muſt ſubjoin ſome caſes wherein I (who am ſomewhat backwards to admit obſervations for univerſal) had the curioſity to diſcover, that the experiments would not uniformly ſucceed; and of theſe exceptions, the chief that I now remember, are reducible to the following three.

EXPERIMENT XXVI.

AND, (firſt) I thought fit to try the operation of acid ſalts upon vegetable ſubſtances, that are already and by their own nature red. And accordingly I made trial upon ſyrup of clove-julyflowers, the clear expreſſed juice of the ſucculent berries of *Spina Cervina*, or buckthorn, (which I had long kept by me for the ſake of its deep colour) upon red roſes, infuſion of Brazil, and divers other vegetable ſubſtances, on ſome of which cruſhed (as is often mentioned) upon white paper (which is alſo to be underſtood in moſt of theſe experiments, if no circumſtance of them argue otherwiſe) ſpirit of ſalt either made no conſiderable change, or altered the colour but from a darker to a lighter red. How it will ſucceed in many other vegetable juices, and infuſions of the ſame colour, I have at preſent ſo few at hand, that I muſt leave you to find it out yourſelf. But as for the operation of the other ſort of ſalts upon theſe red ſubſtances, I found it not very uniform, ſome red, or reddiſh infuſions, as of roſes, being turned thereby into a dirty colour, but yet inclining to green. Nor was the ſyrup of clove-julyflowers turned by the ſolution of potaſhes to a much better, though ſomewhat a greener colour. Another ſort of red infuſions was by an alcaly not turned into a green, but advanced into a crimſon, as I ſhall have occaſion to note ere long. But there were other ſorts, as particularly the lovely coloured juice of buckthorn berries, that readily paſſed into a lovely green.

EXPERIMENT XXVII.

AMONG other vegetables, which we thought likely to afford exceptions to the general obſervation about the differing changes of colours produced by acid and ſulphureous ſalts, we thought fit to make trial upon the flowers of jaſmin, they being both white as to colour, and eſteemed to be of a more oily nature than other flowers. Whereupon having taken the white parts only of the flowers, and rubbed them ſomewhat hard with my finger, upon a piece of clean paper, it appeared very little diſcoloured. Nor had ſpirit of ſalt, wherewith I moiſtened one part of it, any conſiderable operation upon it. But ſpirit of urine, and ſomewhat more effectually a ſtrong alcalizate ſolution, did immediately turn the almoſt colourleſs paper moiſtened by the juice of the jaſmin, not as thoſe liquors are wont to do, when put upon the juices of other flowers, of a good green, but of a deep, though ſomewhat greeniſh yellow; which experiment I did afterwards at ſeveral times repeat with the like ſucceſs. But it ſeems not that a great degree of unctuouſneſs is neceſſary to the production of the like effects, for when we tried the experiment with the leaves of thoſe purely white flowers that appear about the end of winter, commonly called ſnow-drops, the event was not much unlike that, which we have been newly mentioning.

EXPERIMENT XXVIII.

ANOTHER ſort of inſtances to ſhow how much changes of colours, effected by ſalts, depend upon the particular texture of the coloured bodies, has been afforded me by ſeveral yellow flowers, and other vegetables, as mary-gold leaves, early primroſes, freſh madder, &c. For being rubbed upon white paper, till they imbued it with their colour, I found not, that by the addition of alcalizate liquors, nor yet by that of an urinous ſpirit, they would be turned either green or red: nor did ſo acid a ſpirit as that of ſalt conſiderably alter their colour, ſave that it ſeemed a little to dilute it: only in ſome early primroſes it deſtroyed the greateſt part of the colour, and made the paper almoſt white again. And madder alſo afforded ſomething peculiar, and very differing from what we have newly mentioned: for having gathered ſome roots of it, and (whilſt they were recent) expreſſed upon white paper, the yellow juice, an alcalizate ſolution dropt upon it did not turn it either green or white, but red. And the bruiſed madder itſelf being drenched with the like alcalizate ſolution, exchanged alſo its yellowneſs for a redneſs.

An admonition touching the four preceding EXPERIMENTS.

HAVING thus *(Pyrophilus)* given you divers inſtances, to countenance the general obſervation delivered in the twenty-fifth experiment, and divers exceptions whereby it ought to be limited; I muſt leave the further inquiry into theſe matters to your own induſtry. For not remembering at preſent many of thoſe other trials, long ſince made to ſatisfy myſelf about particulars, and not having now the opportunity to repeat them, I muſt content myſelf to have given you the hint, and the ways of proſecuting the ſearch yourſelf; and only declare to you in general, that, as I have made many trials, unmentioned in this treatiſe, whoſe events were agreeable to thoſe mentioned in the twenty-fifth experiment, ſo (to name now no other inſtances) what I have tried with acid and ſulphureous ſalts upon the pulp of juniper berries, rubbed upon white paper, inclines me to think, that among that vaſt multitude, and ſtrange variety of plants that adorn the face of the earth, perhaps many other vegetables may be found, on which ſuch menſtruums may not have ſuch operations, as upon the juice of violets, peas-bloſſoms, *&c.* no nor upon any of thoſe three other ſorts of vegetables, that I have taken notice of in the three foregoing experiments: it ſufficiently appearing even by theſe, that the effects of a ſalt upon the juices of particular vegetables do very much depend upon their particular textures.

EXPERIMENT XXIX.

IT may be of ſome uſe towards the diſcovery of the nature of theſe changes, which the alimental juice receives in ſome vegetables, according to the differing degrees of their maturity, and according to the differing kinds of plants of the ſame denomination, to obſerve what operation acid, urinous, and alcalizate ſalts will have upon the juices of the ſeveral ſorts of the vegetable ſubſtances I have been mentioning.

To declare my meaning by an example; I took from the ſame cluſter one blackberry full ripe, and another that had not yet gone beyond a redneſs; and rubbing a piece of white paper with the former, I obſerved that the juice adhering to it was of a dark reddiſh colour, full of little black ſpecks, and that this juice, by a drop of a ſtrong lixivium, was immediately turned into a greeniſh colour deep enough; by as much urinous ſpirit, into a colour much of kin to the former, though ſomewhat differing, and fainter; and by a drop of ſpirit of ſalt, into a fine and lightſome red: whereas the red berry being in like manner rubbed upon paper, left on it a red colour, which was very little altered by the acid ſpirit newly named, and by the urinous and lixiviate ſalts received changes of colour, differing from thoſe that had been juſt before produced in the dark juice of the ripe black berry.

I REMEMBER alſo, that though the infuſion of damaſk roſes would as well, though not ſo much, as that of red, be heightened by acid ſpirits to an intenſe degree of redneſs,

neſs, and by lixiviate ſalts be brought to a darkiſh green; yet having for trial's ſake taken a roſe, whoſe leaves, which were large and numerous, like thoſe of a *Provence* roſe, were perfectly yellow, though in a ſolution of ſalt of tartar, they afforded a green blueiſh tincture, yet I did not by an acid liquor obtain a red one; all that the ſaline ſpirit I employed performed, being (if I much miſ-remember not) to dilute ſomewhat the yellowneſs of the leaves. I would alſo have tried the tincture of yellow violets, but could procure none. And if I were in thoſe iſlands of *Banda*, which are made famous as well as rich, by being the almoſt only place where cloves will proſper, I ſhould think it worth my curioſity to try, what operation the three differing kinds of ſalts, I have ſo often mentioned, would have upon the juice of this ſpice (expreſſed at the ſeveral ſeaſons of it) as it grows upon the tree. Since good authors inform us (of what is remarkable) that theſe, whether fruits, or rudiments of fruits, are at firſt white, afterward green, and then reddiſh, before they be beaten off the tree; after which being dried before they are put up, they grow blackiſh, as we ſee them. And one of the receteſt Herbariſts informs us, that the flower grows upon the top of the clove itſelf, conſiſting of four ſmall leaves, like a cherry-bloſſom, but of an excellent blue. But (*Pyrophilus*) to return to our own obſervations, I ſhall add, that I the rather chuſe to mention to you an example drawn from roſes, becauſe that though I am apt to think, as I elſewhere advertiſe, that ſomething may be gueſſed at about ſome of the qualities of the juices of vegetables, by the reſemblance or diſparity that we meet with in the changes made of their colours, by the operation of the ſame kinds of ſalts; yet that thoſe conjectures ſhould be very warily made, may appear, among other things, by the inſtance I have choſen to give in roſes. For though (as I formerly told you) the dried leaves, both of the damaſk, and of red ones, give a red tincture to water ſharpened with acid ſalts, yet the one ſort of leaves is known to have a purgative faculty, and the other are often, and divers ways employed for binding.

See Parkinſon, *Th. Botan. Trib.* [illegible]. *cap.* 26.

And I alſo chuſe (*Pyrophilus*) to ſubjoin this twenty-ninth experiment to thoſe that precede it, about the change of the colours of vegetables by ſalts, for theſe two reaſons: the firſt, that you may not eaſily entertain ſuſpicions, if in the trials of an experiment of ſome of the kinds formerly mentioned, you ſhould meet with an event ſomewhat differing from what my relations have made you expect. And the ſecond, that you may hereby be invited to diſcern, that it may not be amiſs to take notice of the particular ſeaſons wherein you gather the vegetables which in nicer experiments you make uſe of. For, if I were not hindered both by haſte and ſome juſtifiable conſiderations, I could perhaps add conſiderable inſtances, to thoſe lately delivered, for the making out of this obſervation; but for certain reaſons I ſhall at preſent ſubſtitute a remarkable paſſage to be met with in that laborious Herbariſt Mr. *Parkinſon*, where treating of the virtues of the (already divers times mentioned) buckthorn berries, he ſubjoins the following account of ſeveral pigments that are made of them, not only according to the ſeveral ways of handling them, but according to the differing ſeaſons of maturity, at which they are gathered. *Of theſe berries* (ſays he) *are made three ſeveral ſorts of colours as they ſhall be gathered, that is, being gathered while they are green, and kept dry, are called ſapberries, which being ſteeped into ſome allom-water, or freſh bruiſed into allom-water, they give a reaſonable fair yellow colour which painters uſe for their work, and book-binders to colour the edges of books, and leather-dreſſers to colour leather; as they uſe alſo to make a green colour, called ſap-green, taken from the berries when they are black, being bruiſed and put into a braſs or copper kettle or pan, and there ſuffered to abide three or four days, or a little heated upon the fire, and ſome beaten allum* *allom put unto them, and afterwards preſſed forth; the juice or liquor is uſually put into great bladders tied with ſtrong thread at the head and hung up until it be dry, which is diſſolved in water or wine, but ſack* (he affirms) *is the beſt to preſerve the colour from ſtarving (as they call it) that is, from decaying, and make it hold freſh the longer. The third colour (whereof none,* ſays he, *that I can find have made mention but only* Tragus) *is a purpliſh colour, which is made of the berries ſuffered to grow upon the buſhes until the middle or end of* November, *that they are ready to drop from the trees.*

And, I remember *(Pyrophilus)* that I tried, with a ſucceſs that pleaſed me well enough, to make ſuch a kind of pigment, as the painters call ſap-green, by a way not unlike that delivered here by our author, but I cannot now find any thing relating to that matter among my looſe papers. And my trials were made ſo many years ago, that I dare not truſt my memory for circumſtances, but will rather tell you, that in a noted colour-ſhop I brought them by queſtions to confeſs to me, that they made their ſap-green much after the ways by our Botaniſt here mentioned. And on this occaſion I ſhall add an obſervation, which though it does not ſtrictly belong to this place, may well enough be mentioned here; namely, that I find by an account given us by the learned *Cluſius*, of alaternus, that even the groſſer parts of the ſame plant are ſome of them one colour, and ſome another: for ſpeaking of that plant, he tells us, that the Portugals uſe the bark to dye their nets into a red colour, and with the chips of the wood, which are whitiſh, they dye a blackiſh blue.

EXPERIMENT XXX.

AMONG the experiments that tend to ſhew that the change of colours in bodies may proceed from the varied texture of their parts, and the conſequent change of their diſpoſition to reflect or refract the light, that ſort of experiments muſt not be left unmentioned, which is afforded us by chymical digeſtions. For, if chymiſts will believe ſeveral famous writers about what they call the philoſopher's ſtone, they muſt acknowledge that the ſame matter, ſealed up hermetically in a philoſophical egg, will, by the continuance of digeſtion, or if they will have it ſo (for it is not material in our caſe which of the two it be) of decoction, run through a great variety of differing colours, before it come to that of the nobleſt elixir; whether that be ſcarlet, or purple, or whatever other kind of red. But without building any thing on ſo abſtruſe and queſtionable an operation (which yet may be pertinently repreſented to thoſe that believe the thing) we may obſerve, that divers bodies digeſted in carefully cloſed veſſels, will in tract of time change their colour: as I have elſewhere mentioned my having obſerved even in rectified ſpirit of hartſhorn, and as is evident in the precipitations of amalgams of gold and mercury, without addition, where, by the continuance of a due heat, the ſilver-coloured amalgam is reduced into a ſhining red powder. Further inſtances of this kind you may find here and there in divers places of my other eſſays. And indeed it has been a thing, that has much contributed to deceive many chymiſts, that there are more bodies than one, which by digeſtion will be brought to exhibit that variety and ſucceſſion of colours, which they imagine to be peculiar to what they call the *true matter of the philoſophers*. But concerning this, I ſhall refer you to what you may elſewhere find in the diſcourſe written touching the paſſive deceptions of chymiſts, and more about the production of colours by digeſtion you will meet with preſently. Wherefore I ſhall now make only this obſervation from what has been delivered, that in theſe operations there appears not any cauſe to attribute the new colours emergent to the action of a new ſubſtantial form, nor to any increaſe or decrement of either the ſalt, ſulphur, or mercury of the matter that acquires new colours: for the veſſels are cloſed, and theſe principles, according to the chymiſts, are ingenerable and incorruptible; ſo that the effect ſeems to proceed from hence, that the heat agitating and ſhuffling the corpuſcles of the body expoſed to it, does in proceſs of time ſo change its texture, as that the tranſpoſed parts do modify the incident light otherwiſe than they did when the matter appeared of another colour.

EXPERIMENT XXXI.

AMONG the ſeveral changes of colour, which bodies acquire or diſcloſe by digeſtion, it is very remarkable, that chymiſts find a redneſs rather than any other colour in moſt of the tinctures they draw, and even in the more groſs ſolutions they make of almoſt all concretes, that abound either with mineral or vegetable ſulphur, though the menſtruum employed about theſe ſolutions or tinctures be ever ſo limpid or colourleſs.

THIS we have obſerved in I know not how many tinctures drawn with ſpirit of wine from jalap, guaiacum, and ſeveral other vegetables; and not only in the ſolutions of amber, benzoin, and divers other concretes made with the ſame menſtruum, but alſo in divers mineral tinctures. And, not to urge that familiar inſtance of the ruby of ſulphur, as chymiſts upon the ſcore of its colour call the ſolution of flowers of brimſtone, made with the ſpirit of turpentine, nor to take notice of other more known examples of the aptneſs of chymical oils to produce a red colour with the ſulphur they extract, or diſſolve; not to inſiſt (I ſay) upon inſtances of this nature, I ſhall further repreſent to you, as a thing remarkable, that both acid and alcalizate ſalts, though in moſt other caſes of ſuch contrary operations, in reference to colours, will, with many bodies that abound with ſulphureous, or with oily parts, produce a red; as is manifeſt partly in the more vulgar inſtances of the tinctures, or ſolutions of ſulphur made with lixiviums, either of calcinated tartar or pot-aſhes, and other obvious examples, partly by this, that the true glaſs of antimony extracted with ſome acid ſpirits, with or without wine, will yield a red tincture, and that I know an acid liquor, which in a moment will turn oil of turpentine into a deep red. But among the many inſtances I could give you of the eaſy production of redneſs by the operation of ſaline ſpirit, as well as of ſpirit of wine; I remember two or three of thoſe I have tried, which ſeem remarkable enough to deſerve to be mentioned to you apart.

EXPERIMENT XXXII.

BUT before we ſet them down, it will not perhaps appear impertinent to premiſe,

THAT there ſeems to be a manifeſt diſparity betwixt red liquors, ſo that ſome of them may be ſaid to have a genuine redneſs in compariſon of others, that have a yellowiſh redneſs: for if you take (for example) a good tincture of cochineal, dilute it ever ſo much with fair water, you will not (as far as I can judge by what I have tried) be able to make it a yellow liquor. Inſomuch that a ſingle drop of a rich ſolution of cochineal in ſpirit of urine, being diluted with above an ounce of fair water, exhibited no yellowiſhneſs at all, but a fair (though ſomewhat faint) pink or carnation; and even when cochineal was by degrees diluted much beyond the newly mentioned colour, by the way formerly related to you in the twenty fourth experiment, I remember not, that there appeared in the whole trial any yellow. But if you take balſam of ſulphur (for inſtance) though it may appear in a glaſs, where it has a good thickneſs, to be of a deep red; yet if you ſhake the glaſs, or pour a few drops on a ſheet of white paper, ſpreading them on it with your finger, the balſam that falls back along the ſides of the glaſs, and that which ſtains the paper, will appear yellow, not red. And there are divers tinctures, ſuch as that of amber made with ſpirit of wine (to name now no more) that will appear either yellow or red, according as the veſſels that they fill are ſlender or broad.

EXPERIMENT XXXIII.

BUT to proceed to the experiments I was about to deliver: firſt, oil or ſpirit of turpentine, though clear as fair water, being digeſted upon the purely white ſugar of lead, has, in a ſhort time, afforded us a high red tincture, that ſome artiſts are pleaſed to call the balſam of *Saturn*, which they very much (and probably not altogether without cauſe) extol as an excellent medicine in divers outward affections.

EXPERIMENT XXXIV.

NEXT, take of common brimſtone finely powdered five ounces, of ſal-armoniac likewiſe pulverized an equal weight, of beaten quicklime ſix ounces, mix theſe powders exquiſitely, and diſtil them through a retort placed in ſand by degrees of fire, giving at length as intenſe a heat as you well can in ſand; there will come over (if you have wrought well) a volatile tincture of ſulphur, which may probably prove an excellent medicine, and ſhould have been mentioned among the other preparations of ſulphur, which we have elſewhere imparted to you, but that it is very pertinent to our preſent ſubject, the change of colours. For though none of the ingredients be red, the diſtilled liquor will be ſo: and this liquor, if it be well drawn, will, upon a little agitation of the phial firſt unſtopped (eſpecially if it be held in a warmer hand) ſend forth a copious fume, not red, like that of nitre, but white; and ſometimes this liquor may be ſo drawn, that I remember, not long ſince, I took pleaſure to obſerve in a parcel of it, that ingredients not red, did not only yield by diſtillation a volatile ſpirit that was red, but though that liquor did upon the bare opening of the bottle it was kept in, drive us away with the plenty and ſulphureous ſcent of a white ſteam which it ſent forth, yet the liquor itſelf being touched by our fingers, did immediately dye them black.

EXPERIMENT XXXV.

THE third and laſt experiment I ſhall now mention, to ſhew how prone bodies abounding in ſulphureous parts are to afford a red colour, is one, wherein by the operation of a ſaline ſpirit upon a white or whitiſh body, which according to the chymiſts ſhould be altogether ſulphureous, a redneſs may be produced, not (as in the former experiments) ſlowly, but in the twinkling of an eye. We took then of the eſſential oil of aniſeeds, which has this peculiarity, that in cold weather it loſes its fluidity and the greateſt part of its tranſparency, and looks like a white or whitiſh ointment, and near at hand ſeems to conſiſt of a multitude of little ſoft ſcales: of this coagulated ſtuff was ſpread a little with a knife upon a piece of white paper, and letting fall on it, and mixing with it, a drop or two of oil of vitriol, immediately (as we foreſaw) there emerged together with ſome heat and ſmoke, a blood-red colour which therefore was in a trice produced by two bodies, whereof the one had but a whitiſh colour, and the other (if carefully rectified) had no colour at all.

EXPERIMENT XXXVI.

BUT on this occaſion (*Pyrophilus*) we muſt add once for all, that in many of the above recited experiments, though the changes of colour happened as we have mentioned them; yet the emergent or produced colour is oft-times very ſubject to degenerate, both quickly and much. Notwithſtanding which, ſince the changes, we have ſet down, do happen preſently upon the operation of the bodies upon each other, or at the times by us ſpecified, that is ſufficient both to juſtify our veracity, and to ſhew what we intend; it not being eſſential to the genuineneſs of a colour to be durable. For a fading leaf, that is ready to rot, and moulder into duſt, may have as true a yellow, as a wedge of gold, which ſo obſtinately reſiſts both time and fire. And the reaſon why I take occaſion from the former experiment to ſubjoin this general advertiſement, is, that I have ſeveral times obſerved, that the mixture reſulting from the oils of vitriol, and of aniſeeds, though it acquire a thicker conſiſtence than either of the ingredients had, has quickly loſt its colour, turning in a very ſhort time into a dirty grey, at leaſt in the ſuperficial parts, where it is expoſed to the air: which laſt circumſtance I therefore mention, becauſe that, though it ſeem probable, that this degeneration of colours may oft-times and in divers caſes proceed from the further action of the ſaline corpuſcles, and the other ingredients upon one another, yet in many caſes much of the quick change of colours ſeems aſcribeable to the air, as may be made probable by ſeveral reaſons; the firſt whereof may be fetched from the newly recited example of the two oils; the next may be, that we have ſometimes obſerved long window-curtains of light colours to have that part of them, which was expoſed to the air, when the window was open of one colour, and the lower part, that was ſheltered from the air by the wall, of another colour: and the third argument may be fetched from divers obſervations, both of others, and our own; for of that pigment ſo well known in painters ſhops, by the name of Turnſol, our induſtrious *Parkinſon*, in the particular account he gives of the plant that bears it, tells us alſo, That *the berries, when they are at their full maturity, have within them between the outer ſkin and the inward kernel or ſeed, a certain juice or moiſture, which being rubbed upon paper or cloth, at the firſt appears of a freſh and lovely green colour, but preſently changeth into a kind of blueiſh purple, upon the cloth or paper; and the ſame cloth afterwards wet in water, and wrung forth, will colour the water into a claret-wine colour; and theſe* (concludes he) *are thoſe rags of cloth, which are uſually called* turnſol *in the druggiſts or grocers ſhops.* And to this obſervation of our Botaniſt we will add an experiment of our own (made before we met with that) which, though in many circumſtances very differing, ſerves to prove the ſame thing. For having taken of the deeply red juice of buckthorn berries, which I bought of the man that uſes to ſell it to the apothecaries, to make their ſyrup *de ſpina cervina*, I let ſome of it drop upon a piece of white paper, and having left it there for many hours, till the paper was grown dry again, I found what I was inclined to ſuſpect, namely, that this juice was degenerated from a deep red to a dirty kind of greyiſh colour, which, in a great part of the ſtained paper, ſeemed not to have ſo much as an eye of red: though a little

Parkinſon, *Thea.* Bot. *Trib.* cap. 4. 12.

ſpirit of ſalt or diſſolved alcali would turn this unpleaſant colour (as formerly I told you it would change the not yet altered juice) into a red or green. And to ſatisfy myſelf, that this degeneration of colour did not proceed from the paper, I dropped ſome of the deep red or crimſon juice upon a white glazed tile, and ſuffering it to dry on there, I found that even in that body, on which it could not ſoak, and by which it could not be wrought, it nevertheleſs loſt its colour. And theſe inſtances *(Pyrophilus)* I am the more careful to mention to you, that you may not be much ſurpriſed or diſcouraged, if you ſhould ſometimes miſs of performing punctually what I affirm myſelf to have done in point of changing colours; ſince in theſe experiments the over-ſight or neglect of ſuch little circumſtances, as in many others would not be perhaps conſiderable, may occaſion the miſcarrying of a trial. And I was willing alſo to take this occaſion of advertiſing you in the repeating of the experiments mentioned in the treatiſe, to make uſe of the juices of vegetables, and other things prepared for your trials, as ſoon as ever they are ready, leſt one or other of them grow leſs fit, if not quite unfit by delay; and to eſtimate the event of trials by the change, that is produced preſently upon the due and ſufficient application of actives to paſſives (as they ſpeak) becauſe in many caſes the effects of ſuch mixtures may not be laſting, and the newly produced colour may in a little time degenerate. But *(Pyrophilus)* I forgot to add to the former obſervations lately made about vegetables, a third of the ſame import, made in mineral ſubſtances, by telling you, that the better to ſatisfy a friend or two in this particular, I ſometimes made, according to ſome conjectures of mine, this experiment; that having diſſolved good ſilver in aqua fortis, and precipitated it with ſpirit of ſalt, upon the firſt decanting of the liquor, the remaining matter would be purely white; but after it had lain a while uncovered, that part of it that was contiguous to the air, would not only loſe its whiteneſs, but appear of a very dark and almoſt blackiſh colour; I ſay, that part that was contiguous to the air, becauſe if that were gently taken off, the ſubjacent part of the ſame maſs would appear very white, till that alſo, having continued a while expoſed to the air, would likewiſe degenerate. Now whether the air performs theſe things by the means of a ſubtile ſalt, which we elſewhere ſhew it not to be deſtitute of, or by a piercing moiſture, that is apt eaſily to inſinuate itſelf into the pores of ſome bodies, and thereby change their texture, and ſo their colour; or by ſoliciting the avolation of certain parts of the bodies, to which it is contiguous; or by ſome other way (which poſſibly I may elſewhere propoſe and conſider) I have not now the leiſure to diſcourſe. And for the ſame reaſon, though I could add many other inſtances, of what I formerly noted touching the emergency of redneſs upon the digeſtion of many bodies, inſomuch that I have often ſeen upon the borders of *France* (and probably we may have the like in *England)* a ſort of pears, which digeſted for ſome time with a little wine, in a veſſel exactly cloſed, will in not many hours appear throughout of a deep red colour (as alſo that of the juice, wherein they are ſtewed, becomes) but even on pure and white ſalt of tartar, pure ſpirit of wine, as clear as rock-water, will (as we elſewhere declare) by long digeſtion acquire a redneſs: though I ſay ſuch inſtances might be multiplied, and though there be ſome other obvious changes of colours, which happen ſo frequently, that they cannot but be as well conſiderable as notorious; ſuch as is the blackneſs of almoſt all bodies burned in the open air; yet our haſte invites us to reſign you the exerciſe of inquiring into the cauſes of theſe changes. And certainly, the reaſon both why the ſoots of ſuch differing bodies are almoſt all of them all black, why ſo much the greater part of vegetables ſhould be rather green than of any other colour, and particularly (which more directly concerns the place) why gentle heats do ſo frequently in chymical operations produce rather a redneſs than any other colour in digeſted menſtruums, not only ſulphureous, as ſpirit of wine, but ſaline, as ſpirit of vinegar, may be very well worth a ſerious enquiry; which I ſhall therefore recommend to *Pyrophilus* and his ingenious friends.

EXPERIMENT XXXVII.

IT may ſeem ſomewhat ſtrange, that if you take the crimſon ſolution of cochineal, or the juice of black cherries, and of ſome other vegetables that afford the like colour (which becauſe many take but for a deep red, we do with them ſometimes call it ſo) and let ſome of it fall upon a piece of paper, a drop or two of an acid ſpirit, ſuch as ſpirit of ſalt, or aqua fortis, will immediately turn it into a fair red. Whereas, if you make an infuſion of brazil in fair water, and drop a little ſpirit of ſalt or aqua fortis into it, that will deſtroy its redneſs, and leave the liquor of a yellow (ſometimes pale) I might perhaps plauſibly enough ſay on this occaſion, that if we conſider the caſe a little more attentively, we may take notice, that the action of the acid ſpirit ſeems in both caſes but to weaken the colour of the liquor on which it falls. And ſo though it deſtroy redneſs in the tincture of brazil, as well as produce red in the tincture of cochineal, its operations may be uniform enough, ſince as crimſon ſeems to be little elſe than a very deep red, with (perhaps) an eye of blue, ſo ſome kinds of red ſeem (as I have lately noted) to be little elſe than heightened yellow. And conſequently in ſuch bodies, the yellow ſeems to be but a diluted red. And accordingly alcalizate ſolutions and urinous ſpirits, which ſeem diſpoſed to deepen the colours of the juices and liquors of moſt vegetables, will not only reſtore the ſolution of cochineal and the infuſion of brazil to crimſon, whence the ſpirit of ſalt had changed them into a truer red; but will alſo (as I lately told you) not only heighten the yellow juice of madder into red, but advance the red infuſion of brazil to a crimſon. But I know not whether it will not be much ſafer to derive theſe changes from varied textures, than certain kinds of bodies; and you will perhaps think it worth while, that I ſhould add on this occaſion, that it may deſerve ſome ſpeculation, why, notwithſtanding what we have been obſerving, though blue and purple ſeem to be deeper colours than red, and therefore the juices of plants of either of the two former colours may (congruouſly enough to what has been juſt now noted) be turned red by ſpirit of ſalt or aqua fortis, yet blue ſyrup of violets and ſome purples ſhould both by oil of tartar and ſpirit of urine be changed into green, which ſeems to be not a deeper, but a more diluted colour than blue, if not alſo than purple.

EXPERIMENT XXXVIII.

IT would much contribute to the hiſtory of colours, if chymiſts would in their laboratories take a heedful notice, and give us a faithful account of the colours obſerved in the ſteams of bodies either ſublimed or diſtilled, and of the colours of thoſe productions of the fire, that are made up by the coalition of thoſe ſteams. As (for inſtance) we obſerve in the diſtilling of pure ſalt-petre, that at a certain ſeaſon of the operation, the body, though it ſeem either cryſtalline, or white, affords very red fumes: whereas though vitriol be green or blue, the ſpirit of it is obſerved to come over in whitiſh fumes. The like colour I have taken notice of in the fumes of ſeveral other concretes of differing colours, and natures, eſpecially when diſtilled with ſtrong fires. And we elſewhere note, that even ſoot, as black as it is, has filled our receivers with ſuch copious white fumes, that they ſeemed to have had their inſides waſhed with milk. And no leſs obſervable may be the diſtilled liquors, into which ſuch fumes convene (for though we will not deny, that by ſkill and care a reddiſh liquor may be obtained from nitre) yet the common ſpirit of it, in the making even of which, ſtore of theſe red fumes are wont to paſs over into the receiver, appears not to be at all red. And beſides, that neither the ſpirit of vitriol, nor that of ſoot is any thing white; and, beſides alſo, that as far as I have obſerved, moſt (for I ſay not all) of the empyreumatical oils of wood, and other concretes, are either of a deep red, or of a colour between red and black; beſides this, I ſay, it is very remarkable, that notwithſtanding that great variety of colours to be met with in the herbs, flowers, and other bodies wont to be diſtilled *in balneo*; yet (as far at leaſt as our common diſtillers' experience reacheth) all the waters and ſpirits that firſt come over by that way of diſtillation, leave the colours of their concretes behind them, though indeed there be one or two vegetables not commonly taken notice of, whoſe diſtilled liquors I elſewhere obſerve to carry over the tincture of the concrete with them. And as in diſtillations, ſo in ſublimations, it were worth while to take notice of what comes up, in reference to our preſent ſcope, but purpoſely performing them (as I have in ſome caſes done) in conveniently ſhaped glaſſes, that the colour of the aſcending fumes may be diſcerned; for it may afford a Naturaliſt good information to obſerve the congruities or the differences betwixt the colours of the aſcending fumes, and thoſe of the flowers they compoſe by their convention. For it is evident, that theſe flowers do many of them, in point of colour, much differ, not only from one another, but ofttimes from the concretes that afforded them. Thus (not here to repeat what I formerly noted of the black ſoots of very differingly coloured bodies) though camphire and brimſtone afford flowers much of their own colour, ſave that thoſe of brimſtone are wont to be a little paler, than the lumps that yielded them; yet even of red benzoin, that ſublimed ſubſtance, which chymiſts call its flowers, is wont to be white or whitiſh. And to omit other inſtances, even one and the ſame black mineral, antimony, may be made to afford flowers, ſome of them red, and ſome grey, and, which is more ſtrange, ſome of them purely white. And it is the preſcription of ſome glaſs-men by exquiſitely mingling a convenient proportion of brimſtone, ſal armoniac, and quickſilver, and ſubliming them together, to make a ſublimate of an excellent blue; and though having cauſed the experiment to be made, we found the produced ſublimate to be far from being of a lovely colour (as was promiſed) that here and there it ſeemed blueiſh, and at leaſt was of a colour differing enough from either of the ingredients, which is ſufficient for our preſent purpoſe. But a much finer colour is promiſed by ſome of the empirics, that pretend to ſecrets, who tell us, that orpiment being ſublimed, will afford among the parts of it that fly upward, ſome little maſſes, which, though the mineral itſelf be of a good yellow, will be red enough to emulate rubies, both in colour and tranſlucency. And this experiment may, for aught I know, ſometimes ſucceed; for I remember, that having in a ſmall bolt-head purpoſely ſublimed ſome powdered orpiment, we could in the lower part of the ſublimate diſcern here and there ſome reddiſh lines, though much of the upper part of the ſublimate conſiſted of a matter, which was not alone purely yellow, but tranſparent almoſt like a powder. And we have alſo this way obtained a ſublimate, the lower part whereof though it conſiſted not of rubies, yet the ſmall pieces of it, which were numerous enough, were of a pleaſant reddiſh colour, and gliſtered very prettily. But to inſiſt on ſuch kind of trials and obſervations (where the aſcending fumes of bodies differ in colour from the bodies themſelves) though it might indeed enrich the hiſtory of colours, would rob me of too much of the little time I have to diſpatch what I have further to tell you concerning them.

EXPERIMENT XXXIX.

TAKE the dried buds (or bloſſoms) of the pomegranate tree (which are commonly called in the ſhops *Balauſtiums* ;) pull off the reddiſh leaves, and by a gentle ebullition of them in fair water, or by a competent infuſion of them in like water well heated, extract a faint reddiſh tincture; which, if the liquor be turbid, you may clarify it by filtrating it. Into this, if you pour a little good ſpirit of urine, or ſome other ſpirit abounding in the like ſort of volatile ſalts, the mixture will preſently turn of a darkiſh green colour; but if inſtead of the forementioned liquor, you drop into the ſimple infuſion a little rectified ſpirit of ſea-ſalt, the pale and almoſt colourleſs liquor will immediately not only grow more tranſparent, but acquire a high redneſs, like that of rich claret wine; which ſo ſuddenly acquired colour may as quickly be deſtroyed, and turned into a dirty blueiſh green, by the affuſion of a competent quantity of the abovementioned ſpirit of urine.

ANNOTATION.

THIS experiment may bring ſome light to, and receive ſome from a couple of other experiments, that I remember I have met with in the ingenious *Gaſſendus*'s animadverſions upon *Epicurus*'s philoſophy, whilſt I was turning over the leaves of thoſe learned commentaries, (my eyes being too weak to let me read ſuch voluminous books quite through) and I the leſs ſcruple (nowithſtanding my contrary cuſtom in this treatiſe) to ſet down theſe experiments of another, becauſe I ſhall a little improve the latter of them, and becauſe by comparing therewith that which I have laſt cited, we may be aſſiſted to conjecture upon what account it is, that oil of vitriol heightens the tincture of red-roſe leaves, ſince ſpirit of ſalt, which is a highly acid menſtruum, but otherwiſe differing enough from oil of vitriol, does the ſame thing. Our author's experiments then, as we made them, are theſe: We took about a glaſs-full of lukewarm water, and in it immerged a quantity of the leaves of ſenna, and preſently upon the immerſion there did not appear any redneſs in the water, but dropping into it a little oil of tartar, the liquor ſoon diſcovered a redneſs to the watchful eye; whereas by a little of that acid liquor of vitriol, which is like the former undeſervedly called oil, ſuch a colour would not be extracted from the infuſed ſenna. On the other ſide, we took ſome red-roſe leaves dried, and having ſhaken them into a glaſs of fair water, they imparted to it no redneſs, but upon the affuſion of a little oil of vitriol the water was immediately turned red, which it would not have been, if inſtead of oil of vitriol, we had employed oil of tartar to produce that colour. That theſe were *Gaſſendus* his experiments, I partly remember, and was aſſured by a friend, who lately tranſcribed them out of *Gaſſendus* his book, which, I therefore add, becauſe I have not now that book at hand. And the deſign of *Gaſſendus* in theſe experiments our friend affirms to be, to prove, that of things not red a redneſs may be made only by mixture, and the varied poſition of parts, wherein the doctrine of that ſubtile philoſopher doth not a little authorize what we have formerly delivered concerning the emergency and change of colours. But the inſtances, that we have out of him ſet down, ſeem not to be the moſt eminent, that may be produced of this truth: for our next experiment will ſhew the production of ſeveral colours out of liquors, which have not any of them any ſuch colour, nor indeed any diſcernable one at all. And whereas though our author tells us, that there was no redneſs either in the water, or the leaves of ſenna, or the oil of tartar; and though it be true, that the predominant colour of the leaves of ſenna be another than red, yet we have tried, that by ſteeping that plant a night even in cold water, it would afford a very deep yellow or reddiſh tincture without the help of the oil of tartar, which ſeems to do little more than aſſiſt the water to extract more nimbly a plenty of that red tincture, wherewith the leaves of ſenna do of themſelves abound, and having taken off the tincture of ſenna, made only with fair water, before it grew to be reddiſh, and decanted it from the leaves, we could not perceive, that by dropping ſome oil of tartar into it, that colour was conſiderable, though it were a little heightened into a redneſs, which might have been expected, if the particles of the oil did eminently co-operate, otherwiſe than we have expreſſed, to the production of this redneſs.

And as for the experiment with red-roſe leaves, the ſame thing may be alledged; for we found that ſuch leaves, by bare infuſion for a night and day in fair water, did afford us a tincture bordering at leaſt upon redneſs; and that colour being conſpicuous in the leaves themſelves, would not by ſome ſeem ſo much to be produced as to be extracted by the affuſion of oil of vitriol. And the experiment tried with the dry leaves of damaſk roſes ſucceeded but imperfectly, but that is indeed obſervable to our author's pupoſe, that oil of tartar will not perform in this experiment what oil of vitriol doth: but becauſe this laſt named liquor is not ſo eaſily to be had, give me leave to advertiſe you, that the experiment will ſucceed, if inſtead of it you employ aqua fortis. And though ſome trials of our own formerly made, and others eaſily deducible from what we have already delivered, about the different families and operations of ſalt, might enable us to preſent you an experiment upon red-roſe leaves, more accommodated to our author's purpoſe than that which he hath given us; yet our reverence to ſo candid a philoſopher, invites us rather to improve his experiment, than ſubſtitute another in its place. Take therefore of the tincture of red-roſe leaves, (for with damaſk-roſe leaves the experiment ſucceedeth not well) made as before hath been taught with a little oil of vitriol, and a good quantity of fair water; pour off this liquor into a clear phial half filled with limpid water, till the water held againſt the light have acquired a competent redneſs, without loſing its tranſparency; into this tincture drop leiſurely a little good ſpirit of urine, and ſhaking the phial, which you muſt ſtill hold againſt the light, you ſhall ſee the red liquor immediately turned into a fine greeniſh blue, which colour was not to be found in any of the bodies, upon whoſe mixture it emerged: and this change is the more obſervable, becauſe in many bodies the degenerating of blue into red is uſual enough, but the turning of red into blue is very unfrequent. If at every drop of ſpirit of urine you ſhake the vial containing the red tincture, you may delightfully obſerve a pretty variety of colours in the paſſage of that tincture from a red to a blue, and ſometimes we have this way hit upon ſuch a liquor, as being looked upon againſt and from the light, did ſeem faintly to emulate the abovementioned tincture of *Lignum Nephriticum*. And if you make the tincture of red-roſes very high, and, without diluting it with fair water, pour on the ſpirit of urine, you may have a blue ſo deep as to make the liquor opacous; but being dropt upon white paper, the colour will ſoon diſcloſe itſelf. Alſo having made the red, and conſequently the blue tincture very tranſparent, and ſuffered it to reſt in a ſmall open phial for a day or two, we found, according to our conjecture, that not only the blue but the red colour alſo vaniſhed; the clear liquor being of a bright amber colour, at the bottom of which ſubſided a light, but copious feculency of almoſt the ſame colour, which ſeems to be nothing but the tincted parts of the roſe-leaves drawn out by the acid ſpirits of the oil of vitriol, and precipitated by the volatile ſalt of the ſpirit of urine: which makes it the more probable, that the redneſs drawn by the oil of vitriol, was at leaſt as well an extraction of the tinging parts of the roſes, as a production of redneſs. And laſtly, if you be deſtitute of ſpirit of urine, you may change the colour of the tincture of roſes with many other ſulphureous ſalts, as a ſtrong ſolution of pot-aſhes, oil of tartar, *&c.* which yet are ſeldom ſo free from feculency, as the ſpirituous parts of urine become by repeated diſtillation.

ANNOTATION.

On this occaſion, I call to mind, that I found a way of producing, though not the ſame kind of blue, as I have been mentioning, yet a colour near of kin to it, namely, a fair purple, by employing a liquor not made red by art, inſtead of the tincture of red-roſes made with an acid ſpirit: and my way was, only to take log-wood (a wood very well known to dyers) having by infuſing the powder of it a while in fair water made that liquor red, I dropt into it a tantillum of an urinous ſpirit, as that of ſal armoniac (and I have done the ſame thing with an alcali) by which the colour was in a moment turned into a rich, and lovely purple. But care muſt be had, that you let not fall into a ſpoonful above two or three drops, leſt the colour become ſo deep, as to make the liquor too opacous. And (to anſwer the other part of *Gaſſendus* his experiment) if, inſtead of fair water, I infuſed the logwood in water made ſomewhat ſour by the acid ſpirit of ſalt, I ſhould obtain neither a purple liquor, nor a red, but only a yellow one.

EXPERIMENT XL.

The experiment I am now to mention to you, *Pyrophilus*, is that which both you, and all the other Virtuoſi that have ſeen it, have been pleaſed to think very ſtrange; and indeed of all the experiments of colours I have yet met with, it ſeems to be the fitteſt to recommend the doctrine propoſed in this treatiſe, and to ſhew that we need not ſuppoſe, that all colours muſt neceſſarily be inherent qualities, flowing from the ſubſtantial forms of the bodies they are ſaid to belong to, ſince by a bare mechanical change of texture in the minute parts of bodies, two colours may in a moment be generated quite *de novo*, and utterly deſtroyed. For there is this difference betwixt the following experiment, and moſt of the others delivered in theſe papers, that in this, the colour that the body already had, is not changed into another, but betwixt two bodies, each of them apart devoid of colour, there is in a moment generated a very deep colour, and which, if it were let alone, would be permanent; and yet by a very ſmall parcel of a third body, that has no colour of its own (leſt ſome may pretend I know not what antipathy betwixt colours) this otherwiſe permanent colour will be in another trice ſo quite deſtroyed, that there will remain no footſteps either of it or of any other colour in the whole mixture.

The experiment is very eaſy, and it is thus performed: take good common ſublimate, and fully ſatiate with it what quantity of water you pleaſe, filtre the ſolution carefully through clean and cloſe paper, that it may drop down as clear and colourleſs as fountain water. Then, when you'll ſhew the experiment, put of it about a ſpoonful into a ſmall wine-glaſs, or any other convenient veſſel made of clear glaſs, and dropping in three or four drops of good oil of tartar *per deliquium*, well filtred, that it may likewiſe be without colour: theſe two limpid liquors will in the twinkling of an eye turn into an opacous mixture of a deep orange colour, which by keeping the glaſs continually ſhaking in your hand, you muſt preſerve from ſettling too ſoon to the bottom; and when the ſpectators have a little beheld this firſt change, then you muſt preſently drop in about four or five drops of the oil of vitriol, and continuing to ſhake the glaſs pretty ſtrongly, that it may the nimbler diffuſe itſelf, the whole colour, if you have gone ſkilfully to work, will immediately diſappear, and all the liquor in the glaſs will be clear and colourleſs as before, without ſo much as a ſediment at the bottom. But for the more graceful trial of this experiment, it will not be amiſs to obſerve, firſt, that there ſhould not be taken too much of the ſolution of ſublimate, nor too much of the oil of tartar dropped in, to avoid the neceſſity of putting in ſo much oil of vitriol as may make an ebullition, and perhaps run over the glaſs. Secondly, that it is convenient to keep the glaſs always a little ſhaking, both for the better mixing of the liquors, and to keep the yellow ſubſtance from ſubſiding, which elſe it would in a ſhort time do; though when it is ſubſided it will retain its colour, and alſo be capable of being deprived of it by the oil newly mentioned. Thirdly, that if any yellow matter ſtick at the ſides of the glaſs, it is but inclining the glaſs, till the clarified liquor can waſh along it, and the liquor will preſently imbibe it, and deprive it of its colour.

MANY have ſometimes wondered, how I came to light upon this experiment; but the notions or conjectures I have about the differing natures of the ſeveral tribes of ſalts, having led me to deviſe the experiment, it will not be difficult for me to give you the chymical reaſon, if I may ſo ſpeak, of the phænomenon. Having then obſerved, that mercury, being diſſolved in ſome menſtruums, would yield a dark yellow precipitate, and ſuppoſing that, as to this, common water, and the ſalts that ſtick to the mercury would be equivalent to thoſe acid menſtruums, which work upon the quickſilver, upon the account of their ſaline particles, I ſubſtituted a ſolution of ſublimate in fair water, inſtead of a ſolution of mercury in aqua fortis, or ſpirit of nitre, that ſimple ſolution being both clearer and free from that very offenſive ſmell, which accompanies the ſolutions of mercury made with thoſe other corroſive liquors. Then I conſidered, that that which makes the yellow colour, is indeed but a precipitate made by the means of the oil of tartar, which we drop in, and which, as the chymiſts know, does generally precipitate metalline bodies corroded by acid ſalts: ſo that the colour in our caſe reſults from the coalition of the mercurial particles with the ſaline ones, wherewith they were formerly aſſociated, and with the alcalizate particles of the ſalt of tartar that ſwim up and down in the oil. Wherefore conſidering alſo, that very many of the effects of lixiviate liquors, upon the ſolutions of other bodies, may be deſtroyed by acid menſtruums, as I elſewhere more particularly declare, I concluded, that if I choſe a very potently acid liquor, which by its inciſive power might undo the work of the oil of tartar, and diſperſe again thoſe particles, which the other had by precipitation aſſociated, into ſuch minute corpuſcles as were before ſingly inconſpicuous, they would become inconſpicuous again, and conſequently leave the liquor as colourleſs as before the precipitation was made.

THIS, as I ſaid, *Pyrophilus*, ſeems to be the chymical reaſon of this experiment; that is, ſuch a reaſon, as, ſuppoſing the truth of thoſe chymical notions I have elſewhere I hope evinced, may give ſuch an account of the phænomena as chymical notions can ſupply us with: but I both here and elſewhere make uſe of this way of ſpeaking, to intimate that I am ſufficiently aware of the difference betwixt a chymical explication of a phænomenon, and one that is truly philoſophical or mechanical; as in our preſent caſe, I tell you ſomething, when I tell you that the yellowneſs of the mercurial ſolution, and the oil of tartar, is produced by the precipitation occaſioned by the affuſion of the latter of thoſe liquors, and that the deſtruction of the colour proceeds from the diſſipation of that curdled matter, whoſe texture is deſtroyed, and which is diſſolved into minute and inviſible particles by the potently acid menſtruum: which is the reaſon, why there remains no ſediment in the bottom, becauſe the infuſed oil takes it up, and reſolves it into hidden or inviſible parts, as water does ſalt or ſugar. But when I have told you all this, I am far from thinking I have told all that ſuch an inquiſitive perſon as yourſelf would know: for I preſume you would deſire, as well as I, to learn (at leaſt) why the particles of the mercury, of the tartar, and of the acid ſalts convening together, ſhould make rather an orange colour than a red, or a blue, or a green. For it is not enough to ſay what I related a little before, that divers mercurial ſolutions, though otherwiſe made, would yield a yellow precipitate, becauſe the queſtion will recur concerning them; and to give it a ſatisfactory anſwer, is, I freely acknowledge, more than I dare as yet pretend to.

But to confirm my conjecture about the chymical reaſon of our experiment, I may add, that as I have (*viz.* pag. 11. of this treatiſe) elſewhere (on another occaſion) told you, with ſaline liquors of another kind and nature than ſalt of tartar (namely, with ſpirit of urine, and liquors of kin to that) I can make the mercury precipitate out of the firſt ſimple ſolution quite of another colour than that hitherto mentioned; nay, if inſtead of altering the precipitating liquor, I altered the texture of the ſublimate in ſuch a way as my notions about ſalt required, I could produce the ſame phænomenon. For having purpoſely ſublimed together equal parts (or thereabout) of ſal armoniac and ſublimate, firſt diligently mixed, the aſcending flowers being diſſolved in fair water, and filtred, gave a ſolution limpid and colourleſs, like that of the other ſublimates, and yet an alcaly dropped into this liquor did not turn it yellow but white. And upon the ſame grounds we may with quickſilver, without the help of common ſublimate, prepare another ſort of flowers diſſoluble in water without diſcolouring it, with which I could likewiſe do what I newly mentioned; to which I ſhall add (what poſſibly you will ſomewhat wonder at) that ſo much does the colour depend upon the texture reſulting from the convention of the ſeveral ſorts of corpuſcles, that though, in our experiment, oil of vitriol deſtroys the yellow colour, yet with quickſilver and fair water, by the help of oil of vitriol alone, we may eaſily make a kind of precipitate of a fair and permanent yellow, as you will ere long (in the forty-ſecond experiment of this third part) be taught. And I may further add, that I choſe oil of vitriol, not ſo much for any other or peculiar quality, as for its being, when it is well rectified (which it is ſomewhat hazardous to bring it to be) not only devoid of colour and ill ſmells, but extremely ſtrong and inciſive. For though common and undephlegmated aqua fortis will not perform the ſame thing well, yet that which is made exceeding ſtrong, by being carefully dephlegmed, will do it pretty well, though not ſo well as oil of vitriol; which is ſo ſtrong, that even without rectification it may for a need be made uſe of. I will not here tell you what I have tried, that I may be able to deprive at pleaſure the precipitate that one of the ſulphureous liquors had made, by the copious affuſion of the other; becauſe I found, though this experiment is too tickliſh to let me give a full account of it in few words, I ſhall therefore tell you, that it is not only for once, that the other above-mentioned experiment may be made, the ſame numerical parcels of liquor being ſtill employed in it. For after I have clarified the orange-coloured liquor, by the addition of as little of the oil of vitriol as will ſuffice to perform the effect, I can again at pleaſure reproduce the opacous colour, by the dropping in of freſh oil of tartar, and deſtroy it again by the re-affuſion of more of the acid menſtruum; and yet oftener, if I pleaſe, can I with theſe two contrariant liquors recall and diſperſe the colour, though by reaſon of the addition of ſo much new liquor, in reference to the mercurial particles, the colour will at length appear more dilute and faint.

An Improvement of the fortieth EXPERIMENT.

AND, *Pyrophilus*, to confirm yet further the notions that led me to think on the propoſed experiment, I ſhall acquaint you with another; which, when I had conveniency, I have ſometimes added to it, and which has to the ſpectators appeared little leſs odd than the firſt. And though becauſe the liquor, requiſite to make the trial ſucceed well, muſt be on purpoſe prepared anew a while before, becauſe it will not long retain its fitneſs for this work, I do but ſeldom annex this experiment to the other; yet I ſhall tell you how I deviſed it, and how I make it. If you boil crude antimony in a ſtrong and clear lixivium, you ſhall ſeparate a ſubſtance from it, which ſome modern chymiſts are pleaſed to call its ſulphur, but how deſervedly I ſhall not here examine, having elſewhere done it in an opportune place; wherefore I ſhall now but need to take notice, that when this ſuppoſed ſulphur (not now to call it rather a kind of crocus) is let fall by the liquor upon its refrigeration, it often ſettles in flakes, or ſuch-like parcels of a yellow ſubſtance (which being by the precedent diſſolution reduced into minute parts, may peradventure be made to take fire much more eaſily than the groſſer powder of unprepared antimony would have done.) Conſidering therefore, that common ſulphur boiled in a lixivium, may be precipitated out of it by rheniſh-wine or white-wine, which are ſouriſh liquors, and have in them, as I elſewhere ſhew, an acid ſalt; and having found alſo by trial, that with other acid liquors I could precipitate out of lixiviate ſolvents ſome other mineral concretions abounding with ſulphureous parts, of which ſort is crude antimony; I concluded it to be eaſy to precipitate the antimony diſſolved, as was lately mentioned, with the acid oil of vitriol. And though common ſulphur yields a white precipitate, which the chymiſts call *lac ſulphuris*, yet I ſuppoſed the precipitated antimony would be of a deep yellow colour, as well if made with oil of vitriol, as if made only by refrigeration and length of time. From this it was eaſy to deduce this experiment, that if you put into one glaſs ſome of the freſhly impregnated and filtrated ſolution of antimony, and into another ſome of the orange-coloured mixture (which I formerly ſhewed you how to make with a mercurial ſolution and oil of tartar) a few drops of oil of vitriol dropped into the laſt mentioned glaſs would, as I told you before, turn the deep yellow mixture into a clear liquor; whereas a little of the ſame oil dropped out of the ſame phial into the other glaſs, would preſently (but not without ſome ill ſcent) turn the moderately clear ſolution into a deep yellow ſubſtance. But this, as I ſaid, ſucceeds not well, unleſs you employ a lixivium that has but newly diſſolved antimony, and has not yet let it fall. But yet in ſummer-time, if your lixivium have been duly impregnated and well filtred after it is quite cold, it will for ſome days (perhaps much longer than I had occaſion to try) retain antimony enough to exhibit, upon the affuſion of the corroſive oil, as much of a good yellow ſubſtance as is neceſſary to ſatisfy the beholders of the poſſibility of the experiment.

Reflections upon the XLth experiment, compared with the Xth and XXth.

THE knowledge of the diſtinction of ſalts which we have propoſed, whereby they are diſcriminated into acid, volatile, or ſalfuginous (if I may for diſtinction ſake ſo call the fugitive ſalts of animal ſubſtances) and fixed or alcalizate, may poſſibly (by that little part which we have already delivered, of what we could ſay of its applicableneſs) appear of ſo much uſe in natural philoſophy (eſpecially in the practic part of it) that I doubt not but it will be no unwelcome corollary of the preceding experiment, if by the help of it I teach you to diſtinguiſh which of thoſe ſalts is predominant in chymical liquors, as well as whether any of them be ſo or not. For though in our notes upon the tenth and twentieth experiments I have ſhown you a way, by means of the tincture of *Lignum Nephriticum*, or of ſyrup of violets, to diſcover whether a propounded ſalt be acid or not; yet you can thereby only find in general that ſuch and ſuch ſalts belong not to the tribe of acids, but cannot determine whether they belong to the tribe of urinous ſalts (under which, for diſtinction ſake, I comprehend all thoſe volatile ſalts of animal or other ſubſtances that are contrary to acids) or to that of alcalies. For as well the one as the other of theſe ſalino-ſulphureous ſalts will reſtore the ceruleous colour to the tincture of *Lignum Nephriticum*, and turn that of ſyrup of violets into green. Wherefore this XLth experiment does opportunely ſupply the deficiency of thoſe. For being ſolicitous to find out ſome ready ways of diſcriminating the tribes of chymical ſalts, I found that all thoſe I thought fit to make trial of, would, if they were of a lixiviate nature, make with ſublimate diſſolved in fair water an orange tawny precipitate; whereas, if they were of an urinous nature, the precipitate would be white and milky. So that having always by me ſome ſyrup of violets and ſome ſolution of ſublimate, I can by the help of the firſt of thoſe liquors diſcover in a trice, whether the propounded ſalt or ſaline body be of an acid nature or no; if it be, I need (you know) inquire no further; but if it be not, I can very eaſily, and as readily diſtinguiſh between the other two kinds of ſalts, by the white or orange-colour that is immediately produced, by letting fall a few drops or grains of the ſalt to be examined, into a ſpoonful of the clear ſolution of ſublimate. For example, it has been ſuppoſed by ſome eminently learned, that when ſal armoniac being mingled with an alcali is forced from it by the fire in cloſe veſſels, the volatile ſalt that will thereby be obtained (if the operation be ſkilfully performed) is but a more fine and ſubtile ſort of ſal armoniac, which, it is preſumed, this operation does but more exquiſitely purify than common ſolutions, filtrations, and coagulations. But this opinion may be eaſily ſhown to be erroneous, as by other arguments, ſo particularly by the lately delivered method of diſtinguiſhing the tribes of ſalts. For the ſaline ſpirit of ſal-armoniac, as it is in many other manifeſt qualities very like the ſpirit of urine, ſo like, that it will in a trice make ſyrup of violets of a lovely green, turn a ſolution of good verdigreaſe into an excellent azure, and make the ſolution of a ſublimate yield a white precipitate; inſomuch that in moſt (for I ſay not all of the experiments) where I aim only at producing a ſudden change of colour, I ſcruple not to uſe ſpirit of ſal-armoniac when it is at hand, inſtead of ſpirit of urine, as indeed it ſeems chiefly to conſiſt (beſides the phlegm that helps to make it fluid) of the volatile urinous ſalt (yet not excluding that of ſoot) that abounds in the ſal armoniac and is ſet at liberty from the ſea-ſalt wherewith it was formerly aſſociated, and clogged, by the operation of the alcali, that divides the ingredients of ſal armoniac, and retains that ſea-ſalt with itſelf: what uſe may be made of the like way of exploration in that inquiry which puzzles ſo many modern Naturaliſts, whether the rich pigment (which we have often had occaſion to mention) belongs to the vegetable or animal kingdom, you may find in another place, where I give you ſome account of what I tried about cochineal. But I think it needleſs to exemplify here our method by any other inſtances, many ſuch being to be met with in divers parts of this treatiſe; but I will rather advertiſe you, that by this way of examining chymical liquors, you may not only in moſt caſes conclude affirmatively, but in ſome caſes negatively. As ſince ſpirit of wine, and, as far as I have tried, thoſe chymical oils which artiſts call eſſential, did not (when I uſed them as I had uſed the ſeveral families of ſalts upon that ſyrup) turn ſyrup of violets red or green, nor the ſolution of ſublimate white or yellow; I inferred it may thence be probably argued, that either they are deſtitute of ſalt, or have ſuch as belongs not to either of the three grand families often already mentioned. When I went to examine the ſpirit of oak, or of ſuch like concretes forced over through a retort, I found by this means amongſt others, that (as I elſewhere ſhow) thoſe chymiſts are much miſtaken in it, that account it a ſimple liquor, and one of their hypoſtatical principles. For not to mention what phlegm it may have, I found that with a few drops of one of this ſort of ſpirits mixed with a good proportion of ſyrup of violets, I could change the colour and make it purpliſh, by the affinity of which colour to redneſs, I conjectured that this ſpirit had ſome acid corpuſcles in it; and accordingly I found, that as it would deſtroy the blueneſs of a tincture of *Lignum Nephriticum*, ſo being put upon corals, it would corrode them, as common ſpirit of vinegar, and other acid liquors are wont to do. And farther to examine whether there were not a great part of the liquor that was not of an acid nature, having ſeparated the ſour or vinegar-like part from the reſt, which (if I miſtake not) is far the more copious; we concluded, as we had conjectured, the other or remaining part, though it had a ſtrong taſte as well as ſmell, to be of a nature differing from that of either of the three ſorts of ſalts above-mentioned, ſince it did as little as ſpirit of wine, and chymical oils, alter the colour either of ſyrup of violets or ſolution of ſublimate: whence we alſo inferred, that the change that had been made of that ſyrup into a purple colour, was effected by the vinegar, that was one of the two ingredients of the liquor, which was wont to paſs for a ſimple or uncompounded ſpirit. And, upon this account, it was of the ſpirit of oak (and the like concretes) freed from its vinegar, that I elſewhere told you, that I had not then obſerved it (and I have repeated the trial but very lately) to deſtroy the ceruleous tincture of *Lignum Nephriticum*. But this only *en paſſant*; for the chief thing I had to add was this: that by the ſame way may be examined and diſcovered divers changes that are produced in bodies, either by nature only, or by art; either of them being able, by changing the texture of ſome concretes I could name, to qualify them to operate after a new manner upon the above mentioned ſyrup, or ſolution, or both: and by this means, to tell you that, upon the by, I have been able to diſcover, that there may be made bodies, which though they run *per deliquium*, as readily as ſalt of tartar, belong in other reſpects, not to the family of alcalies, much leſs to that of ſalſuginous or that of acid ſalts. Perhaps too, I may know a way of making a highly operative ſaline body, that ſhall neither change the colour of ſyrup of violets nor precipitate the ſolution of ſublimate; and I can likewiſe, if I pleaſe, conceal by what liquors I perform ſuch changes of colour, as I have been mentioning to you, by quite altering the texture of ſome ordinary chymical productions, the exploration of which is the main uſe of the fortieth experiment, which I think teaches not a little, if it teach us to diſcover the nature of thoſe things (in reference to ſalt) that are obtained by the ordinary chymical analyſis of mixed bodies, though perhaps there may be other bodies prepared by chymiſtry, which may have the ſame effects in the change of colours, and yet be produced not from what chymiſts call the reſolution of bodies, but from their compoſition. But the diſcourſing of things of this nature is more proper for another place. I ſhall now only add, what might perhaps have been more ſeaſonably told you before; that the reaſon why the way of exploration of ſalts hitherto delivered ſucceeds in the ſolution of ſublimate, depends upon the particular texture of that ſolution, as well as upon the differing natures of the ſaline liquors employed to precipitate it. For gold diſſolved in aqua regia, whether you precipitate it with oil of tartar, which is an alcali, or with ſpirit of urine, or ſal armoniac, which belongs to the family of volatile ſalts, will either way afford a yellow ſubſtance: though with ſuch an acid liquor as, I ſay not ſpirit of ſalt, the body that yields it, being upon the matter an ingredient of aqua regis, but oil of vitriol itſelf, I did not find that I could precipitate the metal out of the ſolution, or deſtroy the colour of it; though the ſame oil of vitriol would readily precipitate ſilver diſſolved in aqua fortis. And if you diſſolve pure ſilver in aqua fortis, and ſuffer it to ſhoot into cryſtals, the clear ſolution of theſe made in fair water, will afford a very white precipitate, whether it be made with an alcali, or an acid ſpirit, as that of ſalt; whereas, which may ſeem ſomewhat ſtrange, with ſpirit of ſal armoniac (that I uſed was made of quick-lime) I could obtain no ſuch white precipitate: that volatile ſpirit, nor (as I remember) that of urine, ſcarce doing any more than ſtriking down a very ſmall quantity of matter, which was neither white nor whitiſh; ſo that the remaining liquor being ſuffered to evaporate till the ſuperfluous moiſture was gone, the greateſt part of the metalline corpuſcles with the ſaline ones that had imbibed them, concoagulated into ſalt, as is uſual in ſuch ſolutions, wherein the metal has not been precipitated.

EXPERIMENT XLI.

OF kin to the laſt or fortieth experiment is another which I remember I have ſometimes ſhewn to Virtuoſi, that were pleaſed not to diſlike it. I took ſpirit of urine made by fermentation, and with a due proportion of copper brought into ſmall parts, I obtained a very lovely azure ſolution; and when I ſaw the colour was ſuch as was requiſite, pouring into a clean glaſs about a ſpoonful of this tincted liquor (of which I uſed to keep a quantity by me) I could, by ſhaking into it ſome drops of ſtrong oil of vitriol, deprive it in a trice of its deep colour, and make it look like common water.

ANNOTATION.

THIS experiment brings into my mind this other, which oftentimes ſucceeds well enough, though not quite ſo well as the former; namely, that if into about a ſmall ſpoonful of a ſolution of good French verdigreaſe made in fair water, I dropt and ſhaked ſome ſtrong ſpirit of ſalt, or rather dephlegmed aqua fortis, the greenneſs of the ſolution would be made in a trice almoſt totally to diſappear, and the liquor held againſt the light would ſcarce ſeem other than clear or limpid, to any but an attentive eye: which is therefore remarkable, becauſe we know that aqua fortis corroding copper, which is it that gives the colour to verdigreaſe, is wont to reduce it to a green blue ſolution. But if into the other altogether or almoſt colourleſs

leſs liquor I was ſpeaking of, you drop a juſt quantity either of oil of tartar or ſpirit of urine, you ſhall find that after the ebullition is ceaſed, the mixture will diſcloſe a lively colour, though ſomewhat differing from that which the ſolution of verdigreaſe had at firſt.

EXPERIMENT XLII.

THAT the colour (*Pyrophilus*) of a body may be changed by a liquor which of itſelf is of no colour, provided it be ſaline, we have already manifeſted by a multitude of inſtances. Nor doth it ſeem ſo ſtrange, becauſe ſaline particles ſwimming up and down in liquors, have been by many obſerved to be very operative in the production and change of colours. But divers of our friends, that are not acquainted with chymical operations, have thought it very ſtrange that a white body, and a dry one too, ſhould immediately acquire a rich new colour upon the bare affuſion of ſpring-water deſtitute as well of adventitious ſalt as of tincture. And yet (*Pyrophilus*) the way of producing ſuch a change of colours may be eaſily enough lighted on, by thoſe that are converſant in the ſolutions of mercury. For we have tried, that though by evaporating a ſolution of quickſilver in aqua fortis, and abſtracting the liquor till the remaining matter began to be well, but not too ſtrongly dried, fair water poured on the remaining calx made it but ſomewhat yellowiſh; yet when we took good quickſilver, and three or four times its weight of oil of vitriol, in caſe we in a glaſs retort placed in ſand drew off the ſaline menſtruum from the metalline liquor, till there remained a dry calx at the bottom, though this precipitate were a ſnow-white body, yet upon pouring on it a large quantity of fair water, we did almoſt in a moment perceive it to paſs from a milky colour to one of the lovelieſt light yellows that ever we had beheld. Nor is the turbith mineral, that chymiſts extol for its power to ſalivate, and for other virtues, of a colour much inferior to this, though it be often made with a differing proportion of the ingredients, a more troubleſome way. For *Beguinus*, who calls it *Mercurius præcipitatus optimus*, takes to one part of quickſilver but two of liquor, and that is rectified oil of ſulphur, which is (in *England* at leaſt) far more ſcarce and dear than oil of vitriol; he alſo requires a previous digeſtion, two or three cohobations, and frequent ablutions with hot diſtilled water; with other preſcriptions, which though they may conduce to the goodneſs of the medicine, which is that he aims at, are troubleſome, and, our trials have informed you, unneceſſary to the obtaining the lemon colour, which he regards not. But though we have very rarely ſeen either in painters ſhops, or elſewhere, a finer yellow than that which we have divers times this way produced (which is the more conſiderable, becauſe durable and pleaſant yellows are very hard to be met with, as may appear by the great uſe which painters are for its colour's ſake fain to make of that pernicious and heavy mineral, orpiment;) yet I fear our yellow is too coſtly, to be like to be employed by painters, unleſs about choice pieces of work, nor do I know how well it will agree with every pigment, eſpecially, with oiled colours. And whether this experiment, though it have ſeemed ſomewhat ſtrange to moſt we have ſhewn it to, be really of another nature than thoſe wherein ſaline liquors are employed, may, as we formerly alſo hinted, be ſo plauſibly doubted, that whether the water poured on the calx, do barely by imbibing ſome of its ſaline parts alter its colour, by altering its texture, or whether by diſſolving the concoagulated ſalts, it does become a ſaline menſtruum, and, as ſuch, work upon the mercury, I freely leave to you, *Pyrophilus*, to conſider. And that I may give you ſome aſſiſtance in your inquiry, I will not only tell

Beguinus *Tyr. Chy. Lib.* 2. *cap.* 13.

tell you, that I have ſeveral times with fair water waſhed from this calx, good ſtore of ſtrongly-taſted corpuſcles, which by the abſtraction of the menſtruum, I could reduce into ſalt; but I will alſo ſubjoin an experiment, which I deviſed, to ſhew among other things, how much a real and permanent colour may be as it were drawn forth by a liquor that has neither colour, nor ſo much as ſaline or other active parts, provided it can but bring the parts of the body it imbibes to convene into cluſters diſpoſed after the manner requiſite to the exhibiting of the emergent colour. The experiment was this.

EXPERIMENT XLIII.

WE took good common vitriol, and having beaten it to powder, and put it into a crucible, we kept it melted in a gentle heat, till by the evaporation of ſome parts, and the ſhuffling of the reſt, it had quite loſt its former colour; what remained we took out, and found it to be a friable calx, of a dirty grey. On this we poured fair water, which it did not colour green or blue, but only ſeemed to make a muddy mixture with it, then ſtopping the phial wherein the ingredients were put, we let it ſtand in a quiet place for ſome days, and after many hours the water having diſſolved a good part of the imperfectly calcined body, the vitriolate corpuſcles ſwimming to and fro in the liquor, had time by their opportune occurſions to conſtitute many little maſſes of vitriol, which gave the water they impregnated a fair vitriolate colour; and this liquor being poured off, the remaining dirty powder did in proceſs of time communicate the like colour, but not ſo deep, to a ſecond parcel of clear water that we poured on it. But this experiment, *Pyrophilus*, is (to give you that hint by the way) of too luciferous a nature to be fit to be fully proſecuted, now that I am in haſte, and willing to diſpatch what remains. And we have already ſaid of it, as much as is requiſite to our preſent purpoſe.

EXPERIMENT XLIV.

IT may (*Pyrophilus*) ſomewhat contribute towards the ſhewing how much ſome colours depend upon the leſs or greater mixture, and (as it were) contemperation of the light with ſhades, to obſerve how that ſometimes the number of particles, of the ſame colour, received into the pores of a liquor, or ſwimming up and down in it, do ſeem much to vary the colour of it. I could here preſent you with particular inſtances to ſhow, how in many (if not moſt) conſiſtent bodies, if the colour be not a light one, as white, yellow, or the like, the cloſeneſs of parts in the pigments makes it look blackiſh, though when it is diſplayed and laid on thinly, it will perhaps appear to be either blue, or green, or red. But the colours of conſiſtent pigments, not being thoſe which the preamble of this experiment has led you to expect examples in, I ſhall take the inſtances I am now to give you, rather from liquors than dry bodies. If then you put a little fair water into a clear and ſlender phial (or rather into one of thoſe pipes of glaſs, which we ſhall by and by mention;) and let fall into it a few drops of a ſtrong decoction or infuſion of cochineal, or (for want of that) of brazil; you may ſee the tincted drops deſcend like little clouds into the liquor; through which, if, by ſhaking the phial, you diffuſe them, they will turn the water either of a pink colour, or like that which is wont to be made by the waſhing of raw fleſh in fair water; by dropping a little more of the decoction, you may heighten the colour into a fine red, almoſt like that which ennobles rubies; by continuing the affuſion, you may bring the liquor to a kind of a crimſon, and afterwards to a dark and opacous redneſs, ſomewhat like that of clotted blood. And in the paſſage of the liquor from one of theſe colours to the other, you may obſerve, if you conſider it attentively, divers other leſs noted colours belonging to red, to which it is not eaſy to give names; eſpecially conſidering how much the proportion of the decoction to the fair water; and the ſtrength of that decoction, together with that of the trajected light and other circumſtances, may vary the phænomena of this experiment. For the convenienter making whereof, we uſe, inſtead of a phial, any ſlender pipe of glaſs of about a foot or more in length, and about the thickneſs of a man's little finger; for, if leaving one end of this pipe open, you ſeal up the other hermetically (or at leaſt ſtop it exquiſitely with a cork well fitted to it, and overlaid with hard ſealing wax melted, and rubbed upon it;) you ſhall have a glaſs, wherein may be obſerved the variations of the colours of liquors much better than in large phials, and wherein experiments of this nature may be well made with very ſmall quantities of liquor. And if you pleaſe, you may in this pipe produce variety of colours in the various parts of the liquor, and keep them ſwimming upon one another unmixed for a good while. And ſome have marvelled to ſee, what variety of colours we have ſometimes (but I confeſs rather by chance than ſkill) produced in thoſe glaſſes, by the bare infuſion of brazil, variouſly diluted with fair water, and altered by the infuſion of ſeveral chymical ſpirits and other ſaline liquors devoid themſelves of colour: and when the whole liquor is reduced to an uniform degree of colour, I have taken pleaſure to make that very liquor ſeem to be of colours gradually differing, by filling with it glaſſes of a conical figure (whether the glaſs have its baſis in the ordinary poſition, or turned upwards). And yet you need not glaſſes of an extraordinary ſhape, to ſee an inſtance of what the various mixture of light and ſhadow can do in the diverſifying of the colour. For if you take but a large round phial, with a ſomewhat long and ſlender neck, and filling it with our red infuſion of brazil, hold it againſt the light you will diſcern a notable diſparity betwixt the colour of that part of the liquor which is in the body of the phial, and that which is more pervious to the light in the neck. Nay, I remember, that I once had a glaſs and a blue liquor (conſiſting chiefly, or only, if my memory deceive me not, of a certain ſolution of verdigreaſe) ſo fitted for my purpoſe, that though in other glaſſes the experiment would not ſucceed, yet when that particular glaſs was filled with that ſolution, in the body of the phial it appeared of a lovely blue, and in the neck (where the light did more dilute the colour) of a manifeſt green; and though I ſuſpected there might be ſome latent yellowneſs in the ſubſtance of the neck of the glaſs, which might with the blue compoſe that green, yet was I not ſatisfied myſelf with my conjecture, but the thing ſeemed odd to me, as well as to divers curious perſons to whom it was ſhown. And I lately had a broad piece of glaſs, which being looked on againſt the light ſeemed clear enough, and held from the light appeared very lightly diſcoloured; and yet it was a piece knocked off from a great lump of glaſs, to which if we rejoined it, where it had been broken off, the whole maſs was as green as graſs. And I have ſeveral times uſed bottles and ſtopples that were both made (as thoſe, I had them from, aſſured me) of the very ſame metal; and whilſt the bottle appeared but inclining towards a green, the ſtopple (by reaſon of its great thickneſs) was of ſo deep a colour, that you would hardly believe they could poſſibly be made of the ſame materials. But to ſatisfy ſome ingenious men, on another occaſion, I provided myſelf of a flat glaſs (which I yet have by me) with which if I look againſt the light with the broad ſide obverted to the eye, it appears like a good ordinary window-glaſs; but if I turn the edge of it to my eye, and place my eye in a convenient poſture in reference to the light, it may contend for deepneſs of colour with an emerald. And this greenneſs puts me in mind of a certain thickiſh, but not conſiſtent pigment I have ſometimes made, and can ſhow you when you pleaſe, which being dropped on a piece of white paper appears, where any quantity of it is fallen, of a ſomewhat crimſon colour; but being with one's finger ſpread thinly on the paper, does preſently exhibit a fair green: which ſeems to proceed only from its diſcloſing its colour upon the extenuation of its depth into ſuperficies, if the change be not ſomewhat helped by the colours degenerating upon one or other of the accounts formerly mentioned. Let me add, that having made divers trials with that blue ſubſtance, which in painters ſhops is called *Litmaſe*, we have ſometimes taken pleaſure to obſerve, that being diſſolved in a due proportion of fair water, the ſolution either oppoſed to the light, or dropped upon white paper, did appear of a deep colour betwixt crimſon and purple; and yet that being ſpread very thin on the paper, and ſuffered to dry on there, the paper was wont to appear ſtained of a fine blue. And to ſatisfy myſelf, that the diverſity came not from the paper, which one might ſuſpect capable of imbibing the liquor, and altering the colour, I made the trial upon a flat piece of purely white glazed earth (which I ſometimes make uſe of about experiments of colours) with an event not unlike the former.

And now I ſpeak of litmaſe, I will add, that having this very day taken a piece of it, that I had kept by me theſe ſeveral years, to make trials about colours, and having let fall a few drops of the ſtrong infuſion of it in fair water, into a fine cryſtal glaſs, ſhaped like an inverted cone, and almoſt full of fair water, I had now (as formerly) the pleaſure to ſee, and to ſhew others, how theſe few tincted drops variouſly diſperſing themſelves through the limpid water, exhibited divers colours, or varieties of purple and crimſon. And when the corpuſcles of the pigment ſeemed to have equally diffuſed themſelves through the whole liquor, I then by putting two or three drops of ſpirit of ſalt, firſt made an odd change in the colour of the liquor, as well as a viſible commotion among its ſmall parts, and in a ſhort time changed it wholly into a very glorious yellow, like that of a topaz. After which if I let fall a few drops of the ſtrong and heavy ſolution of pot-aſhes, whoſe weight would quickly carry it to the ſharp bottom of the glaſs, there would ſoon appear four very pleaſant and diſtinct colours; namely, a bright, but dilute colour at the picked bottom of the glaſs; a purple, a little higher; a deep and glorious crimſon (which crimſon ſeemed to terminate the operation of the ſalt upward) in the confines betwixt the purple and the yellow; and an excellent yellow, the ſame that before ennobled the whole liquor, reaching from thence to the top of the glaſs. And if I pleaſed to pour very gently a little ſpirit of ſal-armoniac upon the upper part of this yellow, there would alſo be a purple or a crimſon, or both generated there, ſo that the unaltered part of the yellow liquor appeared intercepted betwixt the two neighbouring colours.

My ſcope in this third experiment *(Pyrophilus)* is manifold, as firſt to invite you to be wary in judging of the colour of liquors in ſuch glaſſes as are therein recommended to you, and conſequently as much, if not more, when you employ other glaſſes. Secondly, that you may not think it ſtrange, that I often content myſelf to rub upon a piece of white paper the juice of bodies I would examine; ſince not only I could not eaſily procure a ſufficient quantity of the juices of divers of them, but in ſeveral caſes the trials of the quantities of ſuch juices in glaſſes would make us more liable to miſtakes, than the way that in thoſe caſes I have made uſe of. Thirdly, I hope you will by theſe and divers other particulars delivered in this treatiſe, be eaſily induced to think that I may have ſet down many phænomena very faithfully, and juſt as they appeared to me, and yet by reaſon of ſome unheeded circumſtance in the conditions of the matter, and in the degree of light, or the manner of trying the experiment, you may find ſome things to vary from the relations I make of them. Laſtly, I deſigned to give you an opportunity to free yourſelf from the amazement which poſſeſſes moſt men, at the tricks of thoſe mountebanks that are commonly called water-drinkers. For though not only the vulgar, but even many perſons that are far above that rank, have ſo much admired to ſee a man, after having drunk a great deal of fair water, to ſpurt it out again in the form of claret-wine, ſack and milk, that they have ſuſpected the intervening of magic, or ſome forbidden means to effect what they conceived above the power of art; yet having once by chance had occaſion to oblige a wanderer that made profeſſion of that and other juggling tricks, I was eaſily confirmed by his ingenuous confeſſion to me, that this ſo much admired art, indeed conſiſted rather in a few tricks, than in any great ſkill, in altering the nature and colours of things. And I am eaſy to be perſuaded, that there may be a great deal of truth in a little pamphlet printed divers years ago in Engliſh, wherein the author undertakes to diſcover, and that (if I miſtake not) by the confeſſion of ſome of the accomplices themſelves, that a famous water-drinker, then much admired in *England*, performed his pretended tranſmutations of liquors by the help of two or three inconſiderable preparations and mixtures of not unobvious liquors, and chiefly of an infuſion of brazil variouſly diluted and made pale or yellowiſh (and otherwiſe altered) with vinegar; the reſt of their work being performed by the ſhape of the glaſſes, by craft and legerdemain. And for my part, that which I marvel at in this buſineſs, is the drinkers being able to take down ſo much water, and ſpout it out with that violence; though cuſtom, and a vomit ſeaſonably taken before hand, may in ſome of them much facilitate the work. But as for the changes made in the liquors, they were but few and ſlight in compariſon of thoſe, that the being converſant in chymical experiments, and dextrous in applying them to the tranſmuting of colours, may eaſily enough enable a man to make, as even what has been newly delivered in this, and the foregoing experiment; eſpecially if we add to it the things contained in the twentieth, the thirty-ninth, and the fortieth experiments, may perhaps have already perſuaded you.

EXPERIMENT XLV.

YOU may, I preſume, (*Pyrophilus*) have taken notice, that in this whole treatiſe I purpoſely decline (as far as I well can) the mentioning of elaborate chymical experiments, for fear of frighting you by their tedioufneſs and difficulty; but yet, in confirmation of what I have been newly telling you about the poſſibility of varying the colours of liquors, better than the water-drinkers are wont to do, I ſhall add, that *Helmont* uſed to make a preparation of ſteel, which a very ingenious chymiſt, his ſon's friend, whom you know, ſometimes employs for a ſuccedaneum to the Spaw-waters, by diluting this *eſſentia martis liquida* (as he calls it) with a due proportion of water. Now that for which I mention to you this preparation (which as he communicated to me, I know he will not refuſe to *Pyrophilus*) is this, that though the liquor (as I can ſhew you when you pleaſe) be almoſt of the colour of a German (not an Oriental) amethiſt, and conſequently remote enough from green, yet a very few drops being let fall into a large proportion of good Rheniſh, or (in want of that) white-wine (which yet does not quite ſo well) immediately turned the liquor into a lovely green, as I have not without delight ſhown ſeveral curious perſons. By which phænomenon you may learn, among other things, how requiſite it is in experiments about the changes of colours heedfully to mind the circumſtances of them: for water will not, as I have purpoſely tried, concur to the production of any ſuch green, nor did it give that colour to moderate ſpirit of wine, wherein I purpoſely diſſolved it, and wine itſelf is a liquor that few would ſuſpect of being able to work ſuddenly any ſuch change in a metalline preparation of this nature. And to ſatisfy myſelf that this new colour proceeds rather from the peculiar texture of the wine, than from any greater acidity, that Rheniſh or white-wine (for that may not abſurdly be ſuſpected) has in compariſon of water; I purpoſely ſharpened the ſolution of this eſſence in fair water, with a good quantity of ſpirit of ſalt, notwithſtanding which, the mixture acquired no greenneſs. And to vary the experiment a little, I tried, that if into a glaſs of Rheniſh wine made green by this eſſence, I dropped an alcalizate ſolution, or urinous ſpirit, the wine would preſently grow turbid, and of an odd dirty colour: but if inſtead of diſſolving the eſſence in wine, I diſſolved it in fair water, ſharpened perhaps with a little ſpirit of ſalt, then either the urinous ſpirit of ſal armoniac, or the ſolution of the fixed ſalt of pot-aſhes, would immediately turn it of a yellowiſh colour, the fixed or urinous ſalt precipitating the vitriolate ſubſtance contained in the eſſence. But here I muſt not forget to take notice of a circumſtance that deſerves to be compared with ſome part of the foregoing experiment; for whereas our eſſence imparts a greenneſs to wine, but not to water, the induſtrious *Olaus Wormius* in his late *Muſæum* tells us of a rare kind of turnſol, which he calls *Bezetta Rubra*, given him by an apothecary that knew not how it was made, whoſe lovely redneſs would be eaſily communicated to water, if it were immerſed in it; but ſcarce to wine, and not at all to ſpirit of wine: in which laſt circumſtance it agrees with what I lately told you of our eſſence, notwithſtanding their diſagreement in other particulars.

Libr. 2. Cap. 34.

EXPERIMENT XLVI.

WE have often taken notice, as of a remarkable thing, that metals, as they appear to the eye, before they come to be farther altered by other bodies, do exhibit colours very different from thoſe which the fire and the menſtruum, either apart, or both together, do produce in them; eſpecially conſidering that theſe metalline bodies are after all theſe diſguiſes reducible not only to their former metalline conſiſtence, and other more radical properties, but to their colour too; as if nature had given divers metals to each of them a double colour, an external, and an internal. But though upon a more attentive conſideration of this difference of colours, it ſeemed probable to me that divers (for I ſay not all) of thoſe colours which we have juſt now called internal, are rather produced by the coalition of metalline particles with thoſe of the ſalts, or other bodies employed to work on them, than by the bare alteration of the parts of the metals themſelves; and though therefore we may call the obvious colours natural or common, and the others adventitious: yet becauſe ſuch changes of colours, from whatſoever cauſe they be reſolved to proceed, may be properly enough taken in to illuſtrate our preſent ſubject, we ſhall not ſcruple to take notice of ſome of them, eſpecially becauſe there are among them ſuch as are produced without the intervention of ſaline menſtruums. Of the adventitious colours of metalline bodies the chief ſorts ſeem to be theſe three: the firſt, ſuch colours as are produced without other additaments by the action of the fire upon metals. The next, ſuch as emerge from the coalition of metalline particles with thoſe of ſome menſtruum employed to corrode a metal or precipitate it; and the laſt, the colours afforded by metalline bodies either colliquated with, or otherwiſe penetrating into, other bodies, eſpecially fuſible ones. But theſe (*Pyrophilus*) are only, as I told you, the chief ſorts of the adventitious colours of metals, for there may others belong to them, of which I ſhall hereafter have occaſion to take notice of ſome, and of which alſo there poſſibly may be others that I never took notice of.

And to begin with the firſt ſort of colours, it is well enough known to chymiſts, that tin being calcined by fire alone is wont to afford a white calx, and lead calcined by fire alone affords that moſt common red powder we call minium: copper alſo calcined *per ſe*, by a long or violent fire, is wont to yield (as far as I have had occaſion to take notice of it) a very dark or blackiſh powder; that iron likewiſe may by the action of reverberated flames be turned into a colour almoſt like that of ſaffron, may be eaſily deduced from the preparation of that powder, which by reaſon of its colour and of the metal it is made of, is by chymiſts called *Crocus Martis per ſe*. And that mercury, made by the ſtreſs of fire, may be turned into a red powder, which chymiſts call precipitate *per ſe*, I elſewhere more particularly declare.

ANNOTATION I.

It is not unworthy the admoniſhing you (*Pyrophilus*) and it agrees very well with our conjectures about the dependence of the change of a body's colour upon that of its texture, that the ſame metal may by the ſucceſſive operation of the fire receive divers adventitious colours, as is evident in lead, which before it come to ſo deep a colour as that of minium, may paſs through divers others.

ANNOTATION II.

NOT only the calces, but the glaſſes of metals, vitrified *per ſe*, may be of colours differing from the natural or obvious colour of the metal; as I have obſerved in the glaſs of lead, made by long expoſing crude lead to a violent fire, and what I have obſerved about the glaſs or ſlag of copper (of which I can ſhow you ſome of an odd kind of texture) may be elſewhere more conveniently related. I have likewiſe ſeen a piece of very dark glaſs, which an ingenious artificer that ſhowed it me profeſſed himſelf to have made of ſilver alone by an extreme violence (which ſeems to be no more than is needful) of the fire.

ANNOTATION III.

MINERALS alſo by the action of the fire may be brought to afford colours very differing from their own, as I not long ſince noted to you about the variouſly coloured flowers of antimony; to which we may add the whitiſh grey-colour of its calx, and the yellow or reddiſh colour of the glaſs, whereinto that calx may be fluxed.

And I remember, that I elſewhere told you, that vitriol calcined with a very gentle heat, and afterwards with higher and higher degrees of it, may be made to paſs through ſeveral colours before it deſcends to a dark purpliſh colour, whereto a ſtrong fire is wont at length to reduce it. But to inſiſt on the colours produced by the operation of fire upon ſeveral minerals, would take up far more time than I have now to ſpare.

EXPERIMENT XLVII.

THE adventitious colours produced upon metals, or rather with them, by ſaline liquors, are many of them ſo well known to chymiſts, that I would not here mention them, but that beſides a not un-needed teſtimony, I can add ſomething of my own, to what I ſhall repeat about them; and divers experiments which are familiar to chymiſts, are as yet unknown to the greateſt part of ingenious men.

THAT gold diſſolved in aqua regia ennobles the menſtruum with its own colour, is a thing that you cannot (*Pyrophilus*) but have often ſeen. The ſolutions of mercury in aqua fortis are not generally taken notice of, to give any notable tincture to the menſtruum; but ſometimes when the liquor firſt falls upon the quickſilver, I have obſerved a very remarkable, though not durable greenneſs, or blueneſs to be produced; which is a phænomenon not unfit for you to conſider, though I have not now the leiſure to diſcourſe upon it. Tin corroded by aqua fortis till the menſtruum will work no farther on it, becomes exceeding white; but, as we elſewhere note, does very eaſily of itſelf acquire the conſiſtence, not of a metalline calx, but of a coagulated matter, which we have obſerved with pleaſure to look ſo like, either to curdled milk, or curdled whites of eggs, that a perſon unacquainted with ſuch ſolutions may eaſily be miſtaken in it. But when I purpoſely prepared a menſtruum that would diſſolve it as aqua fortis diſſolves ſilver, and not barely corrode it, and quickly let it fall again, I remember not that I took notice of any particular colour in the ſolution, as if the more whitiſh metals did not much tinge their menſtruums, though the conſpicuouſly coloured metals as gold, and copper, do. For lead diſſolved in ſpirit of vinegar or aqua fortis gives a ſolution clear enough, and if the menſtruum be abſtracted appears either diaphanous or white. Of the colour of iron we have elſewhere ſaid ſomething: and it is worth noting, that though if that metal be diſſolved in oil of vitriol diluted with water, it affords a ſalt or magiſtery ſo like in colour, as well as ſome other qualities, to other green vitriol, that chymiſts do not improperly call it *Vitriolum Martis*; yet I have purpoſely tried, that, by changing the menſtruum, and pouring upon the filings of ſteel, inſtead of oil of vitriol, aqua fortis (whereof, as I remember, I uſed four parts to one of the metal) I obtained not a green, but a ſaffron coloured ſolution; or rather a thick liquor of a deep but yellowiſh red. Common ſilver, ſuch as is to be met with in coins, being diſſolved in aqua fortis, yields a ſolution tincted like that of copper, which is not to be wondered at, becauſe in the coining of ſilver, they are wont (as we elſewhere particularly inform you) to give it an allay of copper, and that, which is ſold in ſhops for refined ſilver, is not (ſo far as we have tried) ſo perfectly free from that ignobler metal, but that a ſolution of it in aqua fortis will give a venereal tincture to the menſtruum. But we could not obſerve upon the ſolution of ſome ſilver, which was perfectly refined (ſuch as ſome that we have, from which eight or ten times its weight of lead has been blown off) that the menſtruum, though held againſt the light in a cryſtal phial, did manifeſtly diſcloſe any tincture, only it ſeemed ſometimes not to be quite deſtitute of a little, but very faint blueiſhneſs.

BUT here I muſt take notice, that of all the metals, there is not any, which doth ſo eaſily and conſtantly diſcloſe its unobvious colour, as copper doth. For not only in acid menſtruums, as aqua fortis and ſpirit of vinegar, it gives a blueiſh green ſolution, but if it be almoſt any way corroded, it appears of one of thoſe two colours, as may be obſerved in verdigreaſe made ſeveral ways, in that odd preparation of Venus, which we elſewhere teach you to make with ſublimate, and in the common vitriols of Venus delivered by chymiſts. And ſo conſtant is the diſpoſition of copper, notwithſtanding the diſguiſe artiſts put upon it, to diſcloſe the colour we have been mentioning, that we have by forcing it up with ſal armoniac obtained a ſublimate of a blueiſh colour. Nay, a famous Spagyriſt affirms, that the very mercury of it is green; but till he teach us an intelligible way of making ſuch a mercury, we muſt content ourſelves to inform you, that we have had a cupreous body, that was precipitated out of a diſtilled liquor, that ſeemed to be the ſulphur of Venus, and ſeemed, even when flaming, of a greeniſh colour. And indeed copper is a metal ſo eaſily wrought upon by liquors of ſeveral kinds, that I ſhould tell you, I know not any mineral, that will concur to the production of ſuch a variety of colours as copper diſſolved in ſeveral menſtruums, as ſpirit of vinegar, aqua fortis, aqua regis, ſpirit of nitre, of urine, of ſoot, oils of ſeveral kinds, and I know not how many other liquors, if the variety of ſomewhat differing colours (that copper will be made to aſſume, as it is wrought upon by ſeveral liquors) were not comprehended within the limits of greeniſh blue, or blueiſh green.

AND yet I muſt advertiſe you (*Pyrophilus*) that being deſirous to try, if I could not make with crude copper a green ſolution without the blueiſhneſs, that is wont to accompany its vulgar ſolutions, I bethought myſelf of uſing two menſtruums, which I had not known employed to work on this metal, and which I had certain reaſons to make trial of, as I ſucceſsfully did. The one of theſe liquors (if I much miſremember not) was ſpirit of ſugar diſtilled in a retort, which muſt be warily done (if you will avoid breaking your glaſſes;) and the other, oil or ſpirit of turpentine, which affords a fine green ſolution, that is uſeful to me on ſeveral occaſions. And yet to ſhew, that the adventitious colour may reſult, as well from the true and permanent copper itſelf, as the ſalts wherewith it is corroded, I ſhall add, that if you take a piece of good *Dantzick* copperas, or any other vitriol, wherein Venus is predominant, and having moiſtened it in your mouth, or with fair water, rub it upon a whetted knife, or any other bright piece of ſteel or iron, it will (as we have formerly told you) preſently ſtain the ſteel with a reddiſh colour, like that of copper; the reaſon of which we muſt not now ſtay to inquire.

ANNOTATION I.

I PRESUME you may have taken notice (*Pyrophilus*) that I have borrowed ſome of the inſtances mentioned in this 47th experiment from the laboratories of chymiſts; and becauſe in ſome (though very few) other paſſages of this eſſay, I have likewiſe made uſe of experiments mentioned alſo by ſome ſpagyrical writers, I think it not amiſs to repreſent to you on this occaſion once for all ſome things, beſides thoſe which I intimated in the preamble of this preſent experiment. For beſides, that it is very allowable for a writer to repeat an experiment, which he invented not, in caſe he improve it; and beſides that many experiments familiar to chymiſts are unknown to the generality of learned men, who either never read chymical proceſſes, or never underſtood their meaning, or never durſt believe them; beſides theſe things, I ſay, I ſhall repreſent, that, as to the few experiments I have borrowed from the chymiſts, if they be very vulgar, it would perhaps be difficult to aſcribe each of them its own author, and it is more than the generality of chymiſts themſelves can do: and if they be not of very known and familiar practice among them, unleſs the authors, wherein I found them, had given me cauſe to believe themſelves had tried them, I know not why I might not ſet them down, as a part of the phænomena of colours, which I preſent you; many things unanimouſly enough delivered as matters of fact by I know not how many chymical writers, being not to be relied on, upon the ſingle authority of ſuch authors: for inſtance, as ſome Spagyriſts deliver (perhaps amongſt ſeveral deceitful proceſſes) that *ſaccharum Saturni* with ſpirit of turpentine will afford a balſam, ſo *Beguinus* and many more tell us, that the ſame concrete (*ſaccharum Saturni*) will yield an incomparably fragrant ſpirit, and a pretty quantity of two ſeveral oils. And yet ſince many have complained, as well as I have done, that they could find no ſuch odoriferous, but rather an ill-ſcented liquor, and ſcarce any oil in their diſtillation of that ſweet vitriol, a wary perſon would as little build any thing on what they ſay of the former experiment, as upon what they aver of the latter; and therefore I ſcrupled not to mention this red balſam, of which I have not ſeen any (but what I made) among my other experiments about redneſs.

ANNOTATION II.

WE have ſometimes had the curioſity to try, what colours minerals, as tin-glaſs, antimony, ſpelter, &c. would yield in ſeveral menſtruums; nor have we forborn to try the colours of ſtones, of which that famous one (which *Helmont* calls *Paracelſus's Ludus*) though it be digged out of the earth, and ſeem a true ſtone, has afforded in menſtruums capable to diſſolve ſo ſolid a ſtone, ſometimes a yellowiſh, ſometimes a red ſolution, of both which I can ſhew you. But though I have from minerals obtained with ſeveral menſtruums very differing colours, and ſome ſuch as, perhaps, you would be ſurprized to ſee drawn from ſuch bodies; yet I muſt now paſs by the particulars, being deſirous to put an end to this treatiſe, before I put an end to your patience and my own.

ANNOTATION III.

AND yet before I paſs to the next experiment, I muſt put you in mind, that the colours of metals may in many caſes be further altered by employing, either precipitating ſalts, or other convenient ſubſtances to act upon their ſolutions. Of this you may remember that I have given you ſeveral inſtances already, to which may be added ſuch as theſe; that if quickſilver be diſſolved in aqua fortis, and precipitated out of the ſolution, either with water impregnated with ſea-ſalt, or with the ſpirit of the concrete, it falls to the bottom in the form of a white powder; whereas if it be precipitated with an alcali, it will afford a yellowiſh or tawny powder; and if there be no precipitation made, and the menſtruum be drawn off with a convenient fire, the corroded mercury will remain in the bottom, in the form of a ſubſtance, that may be made to appear of differing colours by differing degrees of heat: as I remember, that lately having purpoſely abſtracted aqua fortis from ſome quickſilver, that we had diſſolved in it, ſo that there remained a white calx, expoſing that to ſeveral degrees of fire, and afterwards to a naked one, we obtained ſome new colours, and at length the greateſt part of the calx lying at the bottom of the phial, and being brought partly to a deep yellow, and partly to a red colour, the reſt appeared elevated to the upper part and neck of the phial, ſome in the form of reddiſh, and ſome of an aſh-colour ſublimate. But of the differing colours, which by differing ways and working of quickſilver with fire, and ſaline bodies, may be produced in precipitates, I may elſewhere have ocaſion to take further notice. I alſo told you not long ſince, that if you corrode quickſilver with oil of vitriol inſtead of aqua fortis, and abſtract the menſtruum, there will remain a white calx, which by the affuſion of fair water preſently turns into a lemon colour. And even the ſuccedaneum to a menſtruum may ſometimes ſerve the turn, to change the colours of a metal. The lovely red, which painters call vermilion, is made of mercury, which is of the colour of ſilver; and of brimſtone, which is of kin to that of gold, ſublimed up together in a certain proportion, as is vulgarly known to Spagyriſts.

EXPERIMENT XLVIII.

THE third chief ſort of the adventitious colours of metals is that, which is produced by aſſociating them (eſpecially when calcined) with other fuſible bodies, and principally *Venice*, and other fine glaſs, devoid of colour.

I HAVE formerly given you an example, whereby it may appear, that a metal may impart to glaſs a colour much differing from its own, when I told you how with ſilver I had given glaſs a lovely golden colour. And I ſhall now add, that I have learned from one of the chief artificers, that ſells painted glaſs, that thoſe of his trade colour it yellow with a preparation of the calx of ſilver. Though having lately had occaſion among other trials to mingle a few grains of ſhell-ſilver (ſuch as is employed with the pencil and pen) with a convenient proportion of powdered cryſtal glaſs, having kept them two or three hours in fuſion, I was ſurprized to find the colliquated maſs to appear, upon breaking the crucible, of a lovely ſaphirine blue; which made me ſuſpect my ſervant might have brought me a wrong crucible: but he conſtantly affirmed it to be the ſame, wherein the ſilver was put, and conſiderable circumſtances countenanced his aſſertion, ſo that till I have opportunity to make farther trial, I cannot but ſuſpect, either that ſilver, which is not (which is not very probable) brought to a perfect fuſion and colliquation with glaſs, may impart to it other colours than when nealed upon it; or elſe (which is leſs unlikely) that though ſilver-beaters uſually chuſe the fineſt coin they can get, as that which is moſt extenſive under the hammer, yet the ſilver leaves, of which this ſhell-ſilver was made, might retain ſo much copper, as to enable it to give the predominant tincture to the glaſs.

FOR, I muſt proceed to tell you (*Pyrophilus*) as another inſtance of the adventitious colours of metals, that, which is ſomething ſtrange, namely, that, though copper calcined *per ſe* affords but a dark and baſely coloured calx, yet the glaſs-men do with it, as themſelves inform me, tinge their glaſs green. And I remember, that when once we took ſome crude copper, and by frequent ignition quenching it in water had reduced it to a dark and ill-coloured powder, and afterwards kept it in fuſion in about a hundred times its weight of fine glaſs, we had, though not a green, yet a blue coloured maſs; which would perhaps have been green, if we had hit right upon the proportion of the materials, and the degree of fire, and the time wherein it ought to be kept in fuſion; ſo plentifully does that metal abound in a venereal tincture, as artiſts call it, and in ſo many ways does it diſcloſe that richneſs. But though copper do, as we have ſaid, give ſomewhat near the like colour to glaſs, which it does to aqua fortis, yet it ſeems worthy to enquire, whether theſe new colours, which mineral bodies diſcloſe in melted glaſs, proceed from the coalition of the corpuſcles of the mineral with the particles of the glaſs as ſuch, or from the action (excited or actuated by fire) of the alcalizate ſalt (which is a main ingredient of glaſs) upon the mineral body, or from the concurrence of both theſe cauſes, or elſe from any other. But to return to that which we were ſaying, we may obſerve, that putty made by calcining together a proportion of tin and lead, as it is itſelf a white calx, ſo does it turn the *pitta di cryſtello* (as the glaſs-men call the matter of the purer ſort of glaſs, wherewith it is colliquated) into a white maſs; which, if it be opacous enough, is employed, as we elſewhere declare, for white amel. But of the colours, which the other metals may be made to produce in colourleſs glaſs, and other vitrifiable bodies, that have native colours of their own, I muſt leave you to inform yourſelf upon trial; or at leaſt muſt forbear to do it till another time, conſidering how many annotations are to follow, upon what has in this and the two former experiments been ſaid already.

ANNOTATION I.

WHEN the materials of glaſs being melted with calcined tin, have compoſed a maſs undiaphanous and white, this white amel is as it were the baſis of all thoſe fine concretes, that goldſmiths and ſeveral artificers employ in the curious art of enamelling. For this white and fuſible ſubſtance will receive into itſelf, without ſpoiling them, the colours of divers other mineral ſubſtances, which like it will endure the fire.

ANNOTATION II.

SO that as by the preſent (XLVIII) experiment it appears, that divers minerals will impart to fuſible maſſes colours differing from their own; ſo by the making and compounding of amels, it may appear, that divers bodies will both retain their colour in the fire, and impart the ſame to ſome others wherewith they were vitrified, and in ſuch trials as that mentioned in the ſeventeenth experiment, where I told you, that even in amels a blue and yellow will compound a green. It is pretty to behold not only that ſome colours are of ſo fixed a nature, as to be capable of mixture without receiving any detriment by the fire, that does ſo eaſily deſtroy or ſpoil thoſe of other bodies; but mineral pigments may be mingled by fire little leſs regularly and ſucceſsfully, than in ordinary dying fatts, the vulgar colours are wont to be mingled by the help of water.

ANNOTATION III.

IT is not only metalline, but other mineral bodies, that may be employed, to give tinctures unto glaſs; and it is worth noting how ſmall a quantity of ſome mineral ſubſtances will tinge a comparatively vaſt proportion of glaſs; and we have ſometimes attempted to colour glaſs, even with precious ſtones, and had cauſe to think the experiment not caſt away. And it is known by them, that have looked into the art of glaſs, that the artificers uſed to tinge their glaſs blue with that dark mineral zaffora (ſome of my trials on which I elſewhere acquaint you) which ſome would have to be a mineral earth, others of ſtone, and others neither the one nor the other, but which is confeſſedly of a dark, but not a blue colour, though it be not agreed of what particular colour it is. It is likewiſe though a familiar yet a remarkable practice among thoſe, that deal in the making of glaſs, to employ (as ſome of themſelves have informed me) what they call manganeſs, and ſome authors call *Magneſia* (of which I make particular mention in another treatiſe) to exhibit in glaſs not only other colours than its own (which is ſo like in darkneſs or blackiſhneſs to the loadſtone, that is given by mineraliſts for one of the reaſons of its Latin name) but colours differing from one another. For though they uſe it (which is ſomewhat ſtrange) to clarify their glaſs, and free it from that blueiſh greeniſh colour, which elſe it would too often be ſubject to; yet they alſo employ it in certain proportions, to tinge their glaſs both with red colour, and with a purpliſh or murry; and putting in a greater quantity, they alſo make with it that deep obſcure glaſs, which is wont to paſs for black, which agrees very well with, and may ſerve to confirm what we noted near the beginning of the 44th experiment, of the ſeeming blackneſs of thoſe bodies, that are over-charged with the corpuſcles of ſuch colours, as red, or blue, or green, *&c.* And as by ſeveral metals and other minerals we can give various colours to glaſs, ſo on the other ſide, by the differing colours, that mineral ores, or other mineral powders, being melted with glaſs, diſcloſe in it, a good conjecture may be oftentimes made of the metal or known mineral, that the ore propoſed either holds, or is moſt of kin to. And this eaſy way of examining ores may be in ſome caſes of good uſe, and is not ill delivered by *Glauber*, to whom I ſhall at preſent refer you for a more particular account of it: unleſs your curioſity command alſo what I have obſerved about theſe matters. Only I muſt here advertiſe you that great circumſpection is requiſite to keep this way from proving fallacious, upon the account of the variations of colour, that may be produced by the differing proportions, that may be uſed betwixt the ore and the glaſs, by the richneſs or poorneſs of the ore itſelf, by the degree of fire, and eſpecially by the length of time, during which the matter is kept in fuſion; as you will eaſily gather from what you will quickly meet with in the following annotation upon this 48th experiment.

ANNOTATION IV.

THERE is another way, and differing enough from thoſe already mentioned, by which metals may be brought to exhibit adventitious colours: for by this, the metal does not ſo much impart a colour to another body, as receive a colour from it, or rather both bodies do by the new texture reſulting from their miſtion produce a new colour. I will not inſiſt to this purpoſe upon the examples afforded us by yellow orpiment, and common ſea-ſalt, from which, ſublimed together, chymiſts unanimouſly affirm their white or cryſtalline arſenic to be made: but it is not unworthy our noting, that though yellow orpiment be acknowledged to be the copiouſeſt by far of the two ingredients of arſenic, yet this laſt named body being duly added to the higheſt coloured metal copper, when it is in fuſion, gives it whiteneſs both within and without. Thus *Lapis Calaminaris* changes and improves the colour of copper, by turning it into braſs. And I have ſometimes, by the help of zink duly mixed after a certain manner, given copper one of the richeſt golden colours, that ever I have ſeen the beſt true gold ennobled with. But pray have a care, that ſuch hints fall not into any hands, that may miſemploy them.

ANNOTATION V.

UPON the knowledge of the differing ways of making minerals and metals produce their adventitious colours in bodies capable of vitrification, depends the pretty art of making what chymiſts by a barbarous word are pleaſed to call *Amanſes*, that is, counterfeit or factitious gems, as emeralds, rubies, ſaphires, topazes, and the like. For in the making of theſe, though pure ſand or calcined cryſtal give the body, yet it is for the moſt part ſome metalline or mineral calx, mingled in a ſmall proportion, that gives the colour. But though I have many years ſince taken delight to divert myſelf with this pleaſing art, and have ſeen very pretty productions of it, yet beſides that I fear I have now forgot moſt of the little ſkill I had in it, this is no place to entertain you with what would rather take up an entire diſcourſe than be comprehended in an annotation. Wherefore the few things, which I ſhall here take notice of to you, are only what belong to the preſent argument, namely,

First, That I have often obſerved, that calcined lead colliquated with fine white ſand or cryſtal, reduced by ignitions and ſubſequent extinctions in water to a ſubtile powder, will of itſelf be brought by a due decoction to give a clear maſs coloured like a German amethyſt. For though this glaſs of lead is looked upon by them, that know no better way of making *Amanſes*, as the grand work of them all; yet, which is an inconvenience, that much blemiſhes this way, the calcined lead itſelf does not only afford matter to the *Amanſes*, but has alſo as well as other metals a colour of its own, which, as I was ſaying, I have often found to be like that of German (as many call them) not Eaſtern amethyſts.

Secondly, That nevertheleſs this colour may be eaſily overpowered by thoſe of divers other mineral pigments (if I may ſo call them) ſo that with glaſs of lead you may emulate (for inſtance) the freſh and lovely greenneſs of an emerald, though in divers caſes the colour, which the lead itſelf upon vitrification tends to, may vitiate that of the pigment, which you would introduce into the maſs.

Thirdly, That ſo much even theſe colours depend upon texture, that in the glaſs of lead itſelf made of about three parts of litharge or minium colliquated with one of very finely powdered cryſtal or ſand, we have taken pleaſure to make the mixture paſs through differing colours, as we kept it more or leſs in the fuſion. For it was not uſually till after a pretty long decoction, that the maſs attained to the amethyſtine colour.

Fourthly and laſtly, That the degrees of coction and other circumſtances may ſo vary the colour produced in the ſame maſs, that in a crucible that was not great I have had fragments of the ſame maſs, in ſome of which, perhaps not ſo big as a hazel-nut, you may diſcern four diſtinct colours.

ANNOTATION VI.

YOU may remember (*Pyrophilus*) that when I mentioned the three ſorts of adventitious colours of metals, I mentioned them but as the chief, not the only. For there may be other ways, which though they do not in ſo ſtrict a ſenſe belong to the adventitious colours of metals, may not inconveniently be reduced to them. And of theſe I ſhall name now a couple, without denying, that there may be more.

The firſt may be drawn from the practice of thoſe, that dye ſcarlet. For the famouſeſt maſter in that art, either in *England* or *Holland*, has confeſſed to me, that neither others nor he can ſtrike that lovely colour, which is now wont to be called the Bow-dye, without their materials be boiled in veſſels, either made of, or lined with a particular metal. But of what I have known attempted in this kind, I muſt not as yet, for fear of prejudicing or diſpleaſing others, give you any particular account.

See the latter end of the fiftieth experiment.

The other way (*Pyrophilus*) of making metals afford unobvious colours, is by imbuing divers bodies with ſolutions of them made in their proper menſtruums: as (for inſtance) though copper plentifully diſſolved in aqua fortis, will imbue ſeveral bodies with the colour of the ſolution; yet ſome other metals will not (as I elſewhere tell you) and have often tried. Gold diſſolved in aqua regia will (which is not commonly known) dye the nails and ſkin, and hafts of knives, and other things made of ivory, not with a golden, but a purple colour, which though it manifeſt but ſlowly, is very durable, and ſcarce ever to be waſhed out. And if I miſremember not, I have already told you in this treatiſe, that the purer cryſtals of fine ſilver made with aqua fortis, though they appear white, will preſently dye the ſkin and nails with a black, or at leaſt a very dark colour, which water will not waſh off, as it will ordinary ink from the ſame parts. And divers other bodies may the ſame way be dyed, ſome of a black, and others of a blackiſh colour.

AND as metalline, ſo likewiſe mineral ſolutions may produce colours differing enough from thoſe of the liquors themſelves. I ſhall not fetch an example of this, from what we daily ſee happen in the powdering of beef, which by the brine employed about it (eſpecially if the fleſh be over-ſalted) does oftentimes appear at our tables of a green, and ſometimes of a reddiſh colour (deep enough;) nor ſhall I inſiſt on the practice of ſome that deal in ſalt-petre, who (as I ſuſpected, and as themſelves acknowledged to me) do, with the mixture of a certain proportion of that, and common ſalt, give a fine redneſs, not only to neats tongues, but, which is more pretty as well as difficult, to ſuch fleſh, as would otherwiſe be purely white: theſe examples, I ſay, I ſhall decline inſiſting on, as chuſing rather to tell you, that I have ſeveral times tried, that a ſolution of the ſulphur of vitriol, or even of common ſulphur, though the liquor appeared clear enough, would immediately tinge a piece of new coin, or other clean ſilver, ſometimes with a golden, ſometimes with a deeper and more reddiſh colour, according to the ſtrength of the ſolution, and the quantity of it, that chanced to adhere to the metal; which may take off your wonder, that the water of the hot ſpring at *Bath*, abounding with diſſolved ſubſtances of a very ſulphureous nature, ſhould for a while as it were gild the new or clean pieces of ſilver coin, that are for a due time immerſed in it. And to theſe may be added thoſe formerly mentioned examples of the adventitious colours of mineral bodies: which brings into my mind, that even vegetable liquors, whether by degeneration, or by altering the texture of the body that imbibes them, may ſtain other bodies with colours differing enough from their own, of which very good herbariſts have afforded us a notable example, by affirming, that the juice of alcanna being green (in which ſtate I could never here procure it) does yet dye the ſkin and nails of a laſting red. But I ſee this treatiſe is like to prove too bulky, without the addition of further inſtances of this nature.

EXPERIMENT XLIX.

MEETING the other day, *Pyrophilus*, in an Italian book, that treats of other matters, with a way of preparing what the author calls a lacca of vegetables, by which the Italians mean a kind of extract fit for painting, like that rich lacca in Engliſh commonly called lake, which is employed by painters as a glorious red: and finding the experiment not to be inconſiderable, and very defectively ſet down; it will not be amiſs to acquaint you with what ſome trials have informed us, in reference to this experiment, which both by our Italian author, and by divers of his countrymen, is looked upon as no trifling ſecret.

Take then the root called in Latin *Curcuma*, and in Engliſh turmerick (which I made uſe of, becauſe it was then at hand, and is among vegetables fit for that purpoſe one of the moſt eaſieſt to be had): and when it is beaten, put what quantity of it you pleaſe into fair water, adding to every pound of water about a ſpoonful or better of as ſtrong a lixivium or ſolution of pot-aſhes as you can well make, clarifying it by filtration before you put it to the decocting water. Let theſe things boil, or rather ſimmer over a ſoft fire in a clean glazed earthen veſſel, till you find by the immerſion of a ſheet of white paper (or by ſome other way of trial) that the liquor is ſufficiently impregnated with the golden tincture of the turmeric; then take the decoction off the fire, and filter or ſtrain it, that it may be clean; and leiſurely dropping into it a ſtrong ſolution of roch-allom, you ſhall find the decoction as it were curdled, and the tincted part of it either to emerge, to ſubſide, or to ſwim up and down, like little yellow flakes: and if you pour this mixture into a tunnel lined with cap-paper, the liquor, that filtred formerly ſo yellow, will now paſs clean through the filtre, leaving its tincted and as it were curdled parts in the filtre, upon which fair water muſt be ſo often poured, till you have dulcified the matter therein contained, the ſign of which dulcification is (you know) when the water, that has paſſed through it, comes from it as taſteleſs as it was poured on it. And if without filtration you would gather together the flakes of this vegetable lake, you muſt pour a great quantity of fair water upon the decoction after the affuſion of the alluminous ſolution, and you ſhall find the liquor to grow clearer, and the lake to ſettle together at the bottom, or emerge to the top of the water; though ſometimes having not poured out a ſufficient quantity of fair water, we have obſerved the lake partly to ſubſide, and partly to emerge, leaving all the middle of the liquor clear. But to make this lake fit for uſe, it muſt, by repeated affuſions of freſh water, be dulcified from the adhering ſalts, as well as that ſeparated by filtration, and be ſpread and ſuffered to dry leiſurely upon pieces of cloth, with brown paper, or chalk, or bricks under them to imbibe the moiſture *.

ANNOTATION I.

WHEREAS it is preſumed, that the magiſtery of vegetables obtained this way conſiſts but of the more ſoluble and coloured parts of the plants that afford it, I muſt take the the liberty to queſtion the ſuppoſition; and for my ſo doing, I ſhall give you this account.

According to the notions (ſuch as they were) that I had concerning ſalts; allom, though to ſenſe a homogeneous body, ought not to be reckoned among true ſalts, but to be itſelf looked upon as a kind of magiſtery, in regard that as native vitriol (for ſuch I have had) contains both a ſaline ſubſtance and a metal, whether copper, or iron, corroded by it, and aſſociated with it; ſo allom, which may be of ſo near a kin to vitriol, that in ſome places of *England* (as we are aſſured by good authority the ſame ſtone will ſometimes afford both) ſeems manifeſtly to contain a peculiar kind of acid ſpirit, generated in the bowels of the earth, and ſome kind of ſtony matter diſſolved by it. And though in making our ordinary allom the workmen uſe the aſhes of a ſea-weed (vulgarly called kelp) and urine; yet thoſe, that ſhould know, inform us, that, here in *England*, there is beſides the factitious allom, allom made by nature without the help of thoſe additaments. Now, *Pyrophilus*, when I conſider this compoſition of allom, and that alcalizate ſalts are wont to precipitate what acid ſalts have diſſolved, I could not but be prone to ſuſpect, that the curdled matter, which is called the magiſtery of vegetables, may have in it no inconſiderable proportion of a ſtony ſubſtance precipitated out of the allom by the lixivium, wherein the vegetable had been decocted. And to ſhew you, that there is no neceſſity, that all the curdled ſubſtance muſt belong to the vegetable, I ſhall add, that I took a ſtrong ſolution of allom, and having filtred it, by pouring in a convenient quantity of a ſtrong ſolution of pot-aſhes, I preſently, as I expected, turned the mixture into a kind of white curds, which being put to filtre, the paper retained a ſtony calx, copious enough, very white, and which ſeemed to be of a mineral nature, both by ſome other ſigns and this, that little bits of it being put upon a live coal, which was gently blown whilſt they were on it, they did neither melt nor fly away, and you may keep a quantity of this white ſubſtance for a good while (nay, for aught I can gueſs, for a very long one) in a red hot crucible, without loſing or ſpoiling it; nor did hot water, wherein I purpoſely kept another parcel of ſuch calx, ſeem to do any more than waſh away the looſer adhering ſalts from the ſtony ſubſtance, which therefore ſeemed unlikely to be ſeparable by ablutions (though reiterated) from the precipitated parts of the vegetable, whoſe lake is intended. And to ſhew you, that there is likewiſe in allom a body, with which the fixed ſalt of the alcalizate ſolution will concoagulate into a ſaline ſubſtance differing from either of them, I ſhall add, that I have taken pleaſure to recover out of the ſlowly exhaled liquor, that paſſed through the filtre, and left the aforementioned calx behind, a body, that at leaſt, ſeemed a ſalt very pretty to look on, as being very white, and conſiſting of an innumerable company of exceeding ſlender and ſhining particles, which would in part eaſily melt at the flame of a candle, and in part fly away with ſome little noiſe. But of this ſubſtance, and its odd qualities, more, perhaps, elſewhere; for now I ſhall only take notice to you, that I have likewiſe with urinous ſalts, ſuch as the ſpirit of ſal armoniac, as well as with the ſpirit of urine itſelf, nay (if I much miſtake not) even with ſtale urine undiſtilled, eaſily precipitated ſuch a white calx, as I was formerly ſpeaking of, out of a limpid ſolution of allom: ſo that there is need of circumſpection in judging of the natures of liquors by precipitations, wherein allom intervenes; elſe we may ſometimes miſtakingly imagine, that to be precipitated out of a liquor by allom, which is rather precipitated out of allom by the liquor. And this puts me in mind to tell you, that it is not unpleaſant to behold, how quickly the ſolution of allom (or injected lumps of allom) does occaſion the ſevering of the coloured parts of the decoction from the liquor, that ſeemed to have ſo perfectly imbibed them.

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* The curious reader that deſires further information concerning lakes, may reſort to the 7th Book of *Neri's* art of glaſs, Engliſhed, (6 or 7 years ſince the writing of this 49th experiment) and illuſtrated with learned obſervations by the inquiſitive and experienced Dr. *Charles Merret*.

ANNOTATION II.

THE above-mentioned way of making lakes we have tried not only with turmeric, but alſo with madder, which yielded us a red lake; and with rue, which afforded us an extract, of (almoſt, if not altogether) the ſame colour with that of the leaves.

But in regard that it is principally the alcalizate ſalt of the pot-aſhes, which enables the water to extract ſo powerfully the tincture of the decocted vegetables, I fear, that our author may be miſtaken, by ſuppoſing, that the decoction will always be of the very ſame colour with the vegetable it is made of. For lixiviate ſalts, to which pot-aſhes eminently belong, though by piercing and opening the bodies of vegetables, they prepare and diſpoſe them to part readily with their tincture; yet ſome tinctures they do not only draw out, but likewiſe alter them, as may be eaſily made appear by many of the experiments already ſet down in this treatiſe. And though allom being of an acid nature, its ſolutions may in ſome caſes deſtroy the adventitious colours produced by the alcali, and reſtore the former; yet beſides that allom is not, as I have lately ſhown, a mere acid ſalt, but a mixt body; and beſides that its operations are languid in compariſon of the activity of ſalts freed by diſtillation, or by incineration and diſſolution, from the moſt of their earthy parts, we have ſeen already examples that in divers caſes an acid ſalt will not reſtore a vegetable ſubſtance to the colour, of which an alcalizate one had deprived it, but makes it aſſume a third very differing from both; as we formerly told you, that if ſyrup of violets were by an alcali turned green (which colour, as I have tried, may be the ſame way produced in the violet-leaves themſelves without any relation to a ſyrup) an acid ſalt would not make it blue again, but red. And though I have, by this way of making lakes, made magiſteries (for ſuch they ſeem to be) of brazil, and as I remember of cochineal itſelf, and of other things, red, yellow, or green, which lakes were ennobled with a rich colour, and others had no bad one; yet in ſome the colour of the lake ſeemed rather inferior than otherwiſe to that of the plant, and in others it ſeemed both very differing, and much worſe. But writing this in a time and place, where I cannot provide myſelf of flowers and other vegetables to proſecute ſuch trials in a competent variety of ſubjects, I am content not to be poſitive in delivering a judgment of this way of lakes, till experience, or you, *Pyrophilus*, ſhall have afforded me a fuller and more particular information.

ANNOTATION III.

AND on this occaſion, *Pyrophilus*, I muſt here (having forgot to do it ſooner) advertiſe you, once for all, that having written ſeveral of the foregoing experiments, not only in haſte, but at ſeaſons of the year, and in places wherein I could not furniſh myſelf with ſuch inſtruments, and ſuch a variety of materials, as the deſign of giving you an introduction into the hiſtory of colours required; it can ſcarce be otherwiſe, but that divers of the experiments, that I have ſet down, may afford you ſome matter of new trials, if you think fit to ſupply the deficiencies of ſome of them (eſpecially the freſhly mentioned about lakes, and thoſe that concern emphatical colours) which deficiencies, for want of being befriended with accommodations, I could better diſcern than avoid.

ANNOTATION IV.

THE uſe of allom is very great as well as familiar in the dyers trade, and I have not been ill pleaſed with the uſe I have been able to make of it, in preparing other pigments than thoſe they employ with vegetable juices. But the lucriferous practices of dyers and other tradeſmen I do, for reaſons that you may know when you pleaſe, purpoſely forbear in this eſſay, though not ſtrictly from pointing at, yet from making it a part of my preſent work explicitly and circumſtantially to deliver; eſpecially ſince I now find (though late, and not without ſome bluſhes at my prolixity) that what I intended but for a ſhort eſſay, is already ſwelled into almoſt a volume.

EXPERIMENT L.

YET here, *Pyrophilus*, I muſt take leave to inſert an experiment, though perhaps you will think its coming in here an intruſion: for I confeſs its more proper place would have been among thoſe experiments, that were brought as proofs and applications of our notions concerning the differences of ſalts: but not having remembered to inſert it in its fitteſt place, I had rather take notice of it in this, than leave it quite unmentioned: partly, becauſe it doth ſomewhat differ from the reſt of our experiments about colours, in the way whereby it is made; and partly becauſe the grounds, upon which I deviſed it, may hint to you ſomewhat of the method I uſe in defining and varying experiments about colours. And upon this account I ſhall inform you, not only what I did, but why I did it.

I CONSIDERED then, that the work of the former experiments was either to change the colour of a body into another, or quite to deſtroy it, without giving it a ſucceſſor; but I had a mind to give you alſo a way, whereby to turn a body endued with one colour into two bodies, of colours as well as conſiſtencies, very diſtinct from each other, and that by the help of a body that had itſelf no colour at all. In order to this, I remembered, that finding the acidity of ſpirit of vinegar to be wholly deſtroyed by its working upon minium (or calcined lead) whereby the ſaline particles of the menſtruum have their taſte and nature quite altered, I had, among other conjectures I had built upon that change, rightly concluded, that the ſolution of lead in ſpirit of vinegar would alter the colour of the juices and infuſions of ſeveral plants, much after the like manner that I had found oil of tartar to do; and accordingly I was quickly ſatisfied upon trial, that the infuſion of roſe-leaves would, by a ſmall quantity of this ſolution well mingled with it, be immediately turned into a ſomewhat ſad green.

AND further, I had often found, that oil of vitriol, though a potently acid menſtruum, will yet precipitate many bodies, both mineral and others, diſſolved not only in aqua fortis (as ſome chymiſts have obſerved) but particularly in ſpirit of vinegar. And I have further found, that the calces or powders precipitated by this liquor were uſually fair and white.

LAYING theſe things together, it was not difficult to conclude, that if, upon a good tincture of red roſe-leaves made with fair water, I dropped a pretty quantity of a ſtrong and ſweet ſolution of minium, the liquor would be turned into the like muddy green ſubſtance, as I have formerly intimated to you, that oil of tartar would reduce it to; and that if then I added a convenient quantity of good oil of vitriol, this laſt named liquor would have two diſtinct operations upon the mixture; the one, that it would precipitate that reſolved lead in the form of a white powder; the other, that it would clarify the muddy mixture, and both reſtore and exceedingly heighten the redneſs of the infuſion of roſes, which was the moſt copious ingredient of the green compoſition. And accordingly trying the experiment in a wine-glaſs ſharp at the bottom (like an inverted cone) that the ſubſiding power might ſeem to take up the more room, and be the more conſpicuous, I found, that when I had ſhaken the green mixture, that the coloured liquor might be the more equally diſperſed, a few drops of the rectified oil of vitriol did preſently turn the opacous liquor into one that was clear and red, almoſt like a ruby, and threw down good ſtore of a powder, which, when it was ſettled, would have appeared very white, if ſome interſperſed particles of the red liquor had not a little allayed the purity, though not blemiſhed the beauty of the colour. And to ſhew you, *Pyrophilus*, that theſe effects do not flow from the oil of vitriol, as it is ſuch, but as it is a ſtrongly acid menſtruum, that has the property both to precipitate lead, as well as ſome other concretes out of ſpirit of vinegar, and to heighten the colour of red roſe-leaves; I add, that I have done the ſame thing, though perhaps not quite ſo well, with ſpirit of ſalt; and that I could not do it with aqua fortis, becauſe though that potent menſtruum does, as well as the others, heighten the redneſs of roſes, yet it would not, like them, precipitate lead out of ſpirit of vinegar, but would rather have diſſolved it, if it had not found it diſſolved already.

And as by this way we have produced a red liquor, and a white precipitate out of a dirty green magiſtery of roſe-leaves; ſo by the ſame method, you may produce a fair yellow, and ſometimes a red liquor, and the like precipitate, out of an infuſion of a curious purple colour. For you may call to mind, that in the annotation upon the 39th experiment I intimated to you, that I had with a few drops of an alcali turned the infuſion of logwood into a lovely purple. Now if inſtead of this alcali I ſubſtituted a very ſtrong and well filtrated ſolution of minium, made with ſpirit of vinegar, and put about half as much of this liquor, as there was of the infuſion of logwood (that the mixture might afford a pretty deal of precipitate) the affuſion of a convenient proportion of ſpirit of ſalt would (if the liquors were well and nimbly ſtirred together) preſently ſtrike down a precipitate like that formerly mentioned, and turn the liquor, that ſwam above it, for the moſt part, into a lovely yellow.

But for the advancing of this experiment a little further, I conſidered, that in caſe I firſt turned a ſpoonful of the infuſion of logwood purple, by a convenient proportion of the ſolution of minium, the affuſion of ſpirit of ſal-armoniac would precipitate the corpuſcles of lead concealed in the ſolution of minium, and yet not deſtroy the purple colour of the liquor; whereupon I thus proceeded: I took about a ſpoonful of the freſh tincture of logwood (for I found, that if it were ſtale, the experiment would not always ſucceed) and having put to it a convenient proportion of the ſolution of minium to turn it into a deep and almoſt opacous purple, I then dropped in as much ſpirit of ſal armoniac, as I gueſſed would precipitate about half or more (but not all) of the lead, and immediately ſtirring the mixture well together, I mingled the precipitated parts with the others, ſo that they fell to the bottom, partly in the form of a powder, and partly in the form of a curdled ſubſtance, that (by reaſon of the predominancy of the tinged corpuſcles over the white) retained, as well as the ſupernatant liquor, a blueiſh purple colour ſufficiently deep, and then inſtantly (but yet warily) pouring on a pretty quantity of ſpirit of ſalt; the matter firſt precipitated was, by the above ſpecified figure of the bottom of the glaſs preſerved from being reached by the ſpirituous ſalt; which haſtily precipitated upon it a new bed (if I may ſo call it) of white powder, being the remaining corpuſcles of the lead, that the urinous ſpirit had not ſtruck down. So that there appeared in the glaſs three diſtinct and very differingly coloured ſubſtances; a purple or violet-coloured precipitate at the bottom, a white and carnation (ſometimes a variouſly coloured) precipitate over that, and at the top of all a tranſparent liquor of a lovely yellow, or red.

Thus you ſee, *Pyrophilus*, that though to ſome I may have ſeemed to have lighted on this (50th) experiment by chance, and though others may imagine, that to have excogitated it muſt have proceeded from ſome extraordinary inſight into the nature of colours; yet indeed the deviſing of it need not be looked upon as any great matter, eſpecially to one, that is a little verſed in the notions I have in theſe, and other papers hinted concerning the differences of ſalts. And perhaps I might add, upon more than conjecture, that theſe very notions, and ſome particulars ſcatteringly delivered in this treatiſe, being ſkilfully put together, may ſuggeſt divers matters (at leaſt) about colours, that will not be altogether deſpicable. But thoſe hinted, *Pyrophilus*, I muſt now leave ſuch as you to proſecute, having already ſpent far more time than I intended to allow myſelf, in acquainting you with particular experiments and obſervations concerning the changes of colour; to which I might have added many more, but that I hope I may have preſented you with a competent number, to make out, in ſome meaſure, what I have, at the beginning of this eſſay, either propoſed as my deſign in this tract, or delivered as my conjectures concerning theſe matters. And it not being my preſent deſign, as I have more than once declared, to deliver any poſitive hypotheſis or ſolemn theory of colours, but only to furniſh you with ſome experiments towards the framing of ſuch a theory; I ſhall add nothing to what I have ſaid already, but a requeſt, that you would not be forward to think I have been miſtaken in any thing I have delivered as matter of fact concerning the changes of colours, in caſe you ſhould not, every time you try it, find it exactly to ſucceed. For beſides the contingencies, to which we have elſewhere ſhewn ſome other experiments to be obnoxious, the omiſſion or variation of a ſeemingly inconſiderable circumſtance may hinder the ſucceſs of an experiment, wherein no other fault has been committed. Of which truth I ſhall only give you that ſingle and almoſt obvious, but yet illuſtrious inſtance of the art of dying ſcarlets: for though you ſhould ſee every ingredient, that is uſed about it; though I ſhould particularly inform you of the weight of each; and though you ſhould be preſent at the kindling of the fire, and at the increaſing and remitting of it, whenever the degree of heat is to be altered; and though (in a word) you ſhould ſee every thing done ſo particularly, that you would ſcarce harbour the leaſt doubt of your comprehending the whole art; yet if I ſhould not diſcloſe to you, that the veſſels, that immediately contain the tinging ingredients, are to be made of or to be lined with tin, you would never be able, by all that I could tell you elſe (at leaſt, if the famouſeſt and candideſt artificers do not ſtrangely delude themſelves) to bring your tincture of cochineal to dye a perfect ſcarlet. So much depends upon the very veſſel, wherein the tinging matters are boiled, and ſo great an influence may an unheeded circumſtance have on the ſucceſs of experiments concerning colours.

A ſhort Account of ſome OBSERVATIONS made by Mr. BOYLE, about a DIAMOND, that ſhines in the Dark.

Firſt incloſed in a LETTER written to a FRIEND:

And now, together with it, annexed to the foregoing Treatiſe, upon the ſcore of the affinity between LIGHT and COLOURS.

A Copy of the LETTER, that Mr. BOYLE wrote to Sir ROBERT MORRAY, to accompany the *Obſervations* touching the *Shining Diamond*.

SIR,

THOUGH Sir *Robert Morray* and Monſieur *Zulichem* be perſons, that have deſerved ſo well of the commonwealth of learning, that I ſhould think myſelf unworthy to be looked upon as a member of it, if I declined to obey them, or to ſerve them; yet I ſhould not without reluctancy ſend you the notes you deſire for him, if I did not hope, that you will tranſmit, together with them, ſome account, why they are not leſs unworthy of his peruſal: which that you may do, I muſt inform you, how the writing of them was occaſioned, which in ſhort was thus. As I was juſt going out of town, hearing that an ingenious gentleman of my acquaintance, lately returned from *Italy*, had a diamond, that being rubbed, would ſhine in the dark, and that he was not far off; I ſnatched time from my occaſions to make him a viſit; but finding him ready to go abroad, and having in vain tried to make the ſtone yield any light in the day-time, I borrowed it of him for that night, upon condition to reſtore it him within a day or two at furtheſt, at *Greſham* College, where we appointed to attend the meeting of the ſociety, that was then to be at that place. And hereupon I haſted that evening out of town, and finding after ſupper, that the ſtone, which in the day-time would afford no diſcernible light, was really conſpicuous in the dark, I was ſo taken with the novelty, and ſo deſirous to make ſome uſe of an opportunity, that was like to laſt ſo little a while, that though at that time I had no body to aſſiſt me but a foot-boy, yet ſitting up late, I made a ſhift that night to try a pretty number of ſuch of the things, that then came into my thoughts, as were not in that place and time unpracticable. And the next day, being otherwiſe employed, I was fain to make uſe of a drowſy part of the night to ſet down haſtily in writing what I had obſerved; and without having the time in the morning to ſtay the tranſcribing of it, I ordered the obſervations to be brought after me to *Greſham* College;

where you may remember, that they were, together with the ſtone itſelf, ſhown to the Royal Society, by which they had the good fortune not to be diſliked, though ſeveral things were, through haſte, omitted, ſome of which you will find in the margin of the incloſed paper. The ſubſtance of this ſhort narrative I hope you will let Monſieur *Zulichem* know, that he may be kept from expecting any thing of finiſhed in the obſervations, and be diſpoſed to excuſe the want of it. But ſuch as they are I hope they will prove (without a clinch) luciferous experiments, by ſetting the ſpeculations of the curious on work, in a diligent enquiry after the nature of light, towards the diſcovery of which, perhaps, they have not yet met with ſo conſiderable an experiment; ſince here we ſee light produced in a dead and opacous body, and that not as in rotten wood, or in fiſhes, or as in the Bolonian ſtone, by a natural corruption, or by a violent deſtruction of the texture of the body, but by ſo ſlight a mechanical operation upon its texture, as we ſeem to know what it is, and as is immediately performed, and that ſeveral ways, without at all prejudicing the body, or making any ſenſible alterations in its manifeſt qualities. And I am the more willing to expoſe my haſty trials to Monſieur *Zulichem*, and to you, becauſe he, being upon the conſideration of dioptrics, ſo odd a phænomenon relating to the ſubject, as probably he treats of, Light, will, I hope, excite a perſon to conſider it, that is wont to conſider things he treats of very well. And for you, Sir, I hope you will both recruit and perfect the obſervations you receive; for you know, that I cannot add to them, having a good while ſince reſtored to Mr. *Clayton* the ſtone, which, though it be now in the hands of a prince, that ſo highly deſerves, by underſtanding them, the greateſt curioſities; yet he vouchſafes you that acceſs to him, as keeps me from doubting, you may eaſily obtain leave to make further trials with it, of ſuch a monarch as ours, that is not more inquiſitive himſelf, than a favourer of them that are ſo. I doubt not but theſe notes will put you in mind of the motion you made to the ſociety, to impoſe upon me the taſk bringing in what I had on other occaſions obſerved concerning ſhining bodies. But though I deny not, that I ſometimes made obſervations about the Bolonian ſtone, and tried ſome experiments about ſome other ſhining bodies; yet the ſame reaſons, that reduced me then to be unwilling to receive even their commands, muſt now be my apology for not anſwering your expectations, namely, the abſtruſe nature of light, and my being already overburdened, and but too much kept employed by the urgency of the preſs, as well as by more concerning and diſtracting occaſions. But yet I will tell you ſome part of what I have met with in reference to the ſtone, of which I ſend you an account. Becauſe I find, on the one ſide, that a great many think it no rarity, upon a miſtaken perſuaſion, that not only there are a ſtore of carbuncles, of which this is one; but that all diamonds, and other gliſtering jewels, ſhine in the dark. Whereas, on the other ſide, there are very learned men, who (plauſibly enough) deny, that there are any carbuncles or ſhining ſtones at all.

And certainly, thoſe judicious men have much more to ſay for themſelves, than the others commonly plead; and therefore did deſervedly look upon Mr. *Clayton*'s diamond as a great rarity. For not only *Boëtius de Boot*, who is judged the beſt author on this ſubject, aſcribes no ſuch virtue to diamonds, but begins what he delivers of carbuncles, with this paſſage; *Magna fama eſt carbunculi. Is vulgo putatur in tenebris carbonis inſtar lucere; fortaſſis quia pyropus ſeu anthrax appellatus à veteribus fuit. Verum hactenus nemo unquam verè aſſerere auſus fuit, ſe gemmam noctu lucentem vidiſſe. Garcias ab Horto pro regis Indiæ medicus refert ſe allocutum fuiſſe, qui ſe vidiſſe affirmarent. Sed iis fidem non habuit.* And a later author, the diligent and judicious *Johannes de Laet*,

Boetius de Boot. Gem. & Lapid. Hiſtor. Lib. 2. Cap. 8.

Laet, in his chapter of carbuncles and of rubies, has this paſſage; *Quia autem carbunculi, pyropi & anthraces à veteribus nominantur, vulgo creditum fuit, carbonis inſtar in tenebris lucere, quod tamen nullâ gemmâ hactenus deprehenſum, licet à quibuſdam temerè jactetur.* And the recenteſt writer I have met with on this ſubject, *Olaus Wormius*, in his account of his well-furniſhed *Muſæum*, does, where he treats of rubies, concur with the former writers by theſe words: *Sunt, qui rubinum veterum carbunculum eſſe exiſtimant, ſed deeſt una illa nota, quod in tenebris inſtar anthracis non luceat: Aſt talem carbunculum in rerum naturâ non inveniri major pars authorum exiſtimant. Licet unum aut alterum in India apud magnates quoſdam reperiri ſcribant, cum tamen ex aliorum relatione id habeant ſaltem, ſed ipſi non viderint.* In confirmation of which I ſhall only add, that hearing of a ruby, ſo very vivid, that the jewellers themſelves have ſeveral times begged leave of the fair lady, to whom it belonged, that they might try their choiceſt rubies by comparing them with that, I had the opportunity, by the favour of this lady and her huſband (both which I have the honour to be acquainted with) to make a trial of this famous ruby in the night, and in a room well darkened, but not only could not diſcern any thing of light, by looking on the ſtone before any thing had been done to it, but could not, by all my rubbing, bring it to afford the leaſt glimmering of light.

Muſæi Wormiani, Cap. 17.

BUT, Sir, though I be very backward to admit ſtrange things for truths, yet I am not very forward to reject them as impoſſibilities; and therefore I would not diſcourage any from making further enquiry, whether or no there be really *in rerum natura* any ſuch thing as a true carbuncle or ſtone, that without rubbing will ſhine in the dark. For if ſuch a thing can be found, it may afford no ſmall aſſiſtance to the curious in the inveſtigation of light, beſides the nobleneſs and rarity of the thing itſelf. And though *Vartomannus* was not an eye-witneſs of what he relates, that the king of *Pegu*, one of the chief kings of the *Eaſt-Indies*, had a true carbuncle of that bigneſs and ſplendour, that it ſhined very glorioufly in the dark; and though *Garcias ab Horto*, the *Indian* Vice-Roy's phyſician, ſpeaks of another carbuncle, only upon the report of one, that he diſcourſed with, who affirmed himſelf to have ſeen it; yet as we are not ſure, that theſe men, that gave themſelves out to be eye-witneſſes, ſpeak true, yet they may have done ſo for aught we know to the contrary. And I could preſent you with a much conſiderabler teſtimony to the ſame purpoſe, if I had the permiſſion of a perſon concerned, without whoſe leave I muſt not do it. I might tell you, that *Marcus Paulus Venetus* (whoſe ſuppoſed fables divers of our later travellers and navigators have ſince found to be truths) ſpeaking of the king of *Zeilan*, that then was, tells us, that he was ſaid to have the beſt ruby in the world, a palm long, and as big as a man's arm, without ſpot, ſhining like a fire: and he ſubjoins, that the Great Cham, under whom *Paulus* was a conſiderable officer, ſent and offered the value of a city for it; but the king anſwered, he would not give it for the treaſure of the world, nor part with it, having been his anceſtors. And I could add, that in the relation made by two Ruſſian Coſſacks of their journey into *Catay*, written to their emperor, they mention their having been told by the people of thoſe parts, that their king had a ſtone, which lights as the ſun both day and night, called in their language *Sarra*, which thoſe Coſſacks interpret a ruby. But theſe relations are too uncertain for me to build any thing upon; and therefore I ſhall proceed to tell you, that there came hither, about two years ſince, out of *America*, the governor of one of the principal colonies there, an ancient virtuoſo, and one that has the honour to be a member of the Royal Society: this gentleman, finding ſome of the chief affairs of his country committed to another and me, made me divers

Purchas's *Pilgrim. lib.* 1. *cap.* 4. *pag.* 104.

In the year 1619.

divers viſits; and in one of them, when I enquired what rare ſtones they had in thoſe parts of the *Indies* he belonged to, he told me, that the Indians had a tradition, that in a certain hardly acceſſible hill, a pretty way up in the country, there was a ſtone, which in the night-time ſhined very vividly, and to a great diſtance; and he aſſured me, that though he thought it not fit to venture himſelf ſo far among thoſe ſavages, yet he purpoſely ſent thither a bold Engliſhman, with ſome natives, to be his guides; and that this meſſenger brought him back word, that at a diſtance from the hillock he had plainly perceived ſuch a ſhining ſubſtance as the Indians tradition mentioned; and being ſtimulated by curioſity, had ſlighted thoſe ſuperſtitious fears of the inhabitants, and with much ado, by reaſon of the difficulty of the way, had made a ſhift to clamber up to that part of the hill, where, by a very heedful obſervation, he ſuppoſed himſelf to have ſeen the light. But whether it were, that he had miſtaken the place, or for ſome other reaſon, he could not find it there; though when he was returned to his former ſtation, he did again ſee the light ſhining in the ſame place where it ſhone before. A further account of this light I expect from the gentleman, that gave me this, who lately ſent me the news of his being landed in that country. And though I reſerve to myſelf a full liberty of believing no more than I ſee cauſe, yet I do the leſs ſcruple to relate this, becauſe a good part of it agrees well enough with another ſtory, that I ſhall in the next place have occaſion to ſubjoin: in order whereunto, I ſhall tell you, that though the learned authors I formerly mentioned, tell us, that no writer has affirmed his having himſelf ſeen a real carbuncle, yet, conſidering the light of Mr. *Clayton*'s diamond, it recalled into my mind, that ſome years before, when I was inquiſitive about ſtones, I had met with an old Italian book highly extolled to me by very competent judges; and that, though the book was very ſcarce, I had purchaſed it at a dear rate, for the ſake of a few conſiderable paſſages I met with in it, and particularly one, which being very remarkable in itſelf, and pertinent to our preſent argument, I ſhall put it for you, though not word for word, which I fear I have forgot to do, yet as to the ſenſe, into Engliſh.

‘ HAVING promiſed (ſays our author) to ſay ſomething of that moſt precious ſort of jewels, carbuncles, becauſe they are very rarely to be met with, we ſhall briefly deliver what we know of them. In *Clement* the VIIth's time I happened to ſee one of them at a certain Raguſian merchant's, named *Beigoio di Bona*: this was a carbuncle white, of that kind of whiteneſs, which we ſaid was to be found in thoſe rubies, of which we made mention a little above,’ (where he had ſaid, that thoſe rubies had a kind of livid whiteneſs, or paleneſs, like that of a Calcidonian) ‘ but it had in it a luſtre ſo pleaſing, and ſo marvellous, that it ſhined in the dark, but not as much as coloured carbuncles; though it be true, that in an exceeding dark place I ſaw it ſhine in the manner of fire almoſt gone out. But as for coloured carbuncles, it has not been my fortune to have ſeen any: wherefore I will only ſet down what I learned about them, diſcourſing in my youth with a Roman gentleman of ancient experience in matters of jewels, who told me, that one *Jacopo Cola* being by night in a vineyard of his, and eſpying ſomething in the midſt of it, that ſhined like a little glowing coal, at the foot of a vine, went near towards the place, where he thought himſelf to have ſeen that fire; but not finding it, he ſaid, that being returned to the ſame place, whence he had firſt deſcried it, and perceiving there the ſame ſplendor as before, he marked it ſo heedfully, that he came at length to it, where he took up a very little ſtone, which he carried away with tranſports and joy. And the next day, carrying it about to ſhow it divers of his friends, whilſt he was relating after what manner he found it, there caſually intervened a

Benvenuto Cellini nell Arte del Gioiellare, lib. 1. *pag.* 10.

‘ Venetian embaſſador, exceedingly expert in jewels, who preſently knowing it to be ‘ a carbuncle, did craftily, before he and the ſaid *Jacopo* parted (ſo that there was no ‘ body preſent, that underſtood the worth of ſo precious a gem) purchaſe it for the ‘ value of ten crowns, and the next day left *Rome* to ſhun the being neceſſitated to ‘ reſtore it; and, as he affirmed, it was known within ſome while after, that the ſaid ‘ Venetian gentleman did, in *Conſtantinople*, ſell that carbuncle to the then Grand ‘ Signior, newly come to the empire, for a hundred thouſand crowns.’

AND this is what I can ſay concerning carbuncles; and this is not a little at leaſt as to the firſt part of this account, where our *Cellini* affirms himſelf to have ſeen a real carbuncle with his own eyes, eſpecially ſince this author appears wary in what he delivers, and is inclined rather to leſſen, than increaſe the wonder of it. And his teſtimony is the more conſiderable, becauſe though he were born a ſubject neither to the pope nor the then king of *France* (that royal virtuoſo *Francis* I.) yet both the one and the other of thoſe princes employed him much about making of their nobleſt jewels. What is now reported concerning a ſhining ſubſtance to be ſeen in one of the iſlands about *Scotland*, were very improper for me to mention to Sir *Robert Morray*, to whom the firſt information was originally brought, and from whom I expect a farther (for I ſcarce dare expect a convincing) account of it. But I muſt not omit, that ſome virtuoſi queſtioning me the other day at *Whitehall* about Mr. *Clayton*'s diamond, and meeting amongſt them an ingenious Dutch gentleman, whoſe father was long embaſſador for the *Netherlands* in *England*, I learned of him, that he is acquainted with a perſon, whoſe name he told (but I do not well remember it) who was admiral of the Dutch in the *Eaſt-Indies*, and who aſſured this gentleman, Monſieur *Boreel*, that at his return from thence, he brought back with him into *Holland* a ſtone, which though it looked but like a pale dull diamond, ſuch as he ſaw Mr. *Clayton*'s to be, yet was it a real carbuncle; and did without rubbing ſhine ſo much, that when the admiral had occaſion to open a cheſt, which he kept under deck in a dark place, where it was forbidden to bring candles for fear of miſchances, as ſoon as he opened the trunk, the ſtone would, by its native light, ſhine ſo as to illuſtrate a great part of it. And this gentleman having very civilly and readily granted me the requeſt I made him, to write to the admiral, who is yet alive in *Holland* (and probably may ſtill have the jewel by him) for a particular account of this ſtone, I hope ere long to receive it; which will be the more welcome to me, not only becauſe ſo unlikely a thing needs a clear evidence, but becauſe I have had ſome ſuſpicion, that (ſuppoſing the truth of the thing, what may be a ſhining ſtone in a very hot country, as the *Eaſt-Indies*, may perhaps ceaſe to be ſo (at leaſt in certain ſeaſons) in one as cold as *Holland*. For I obſerved in the diamond I ſend you an account of, that not only rubbing, but a very moderate degree of warmth, though excited by other ways, would make it ſhine a little. And it is not impoſſible, that there may be ſtones as much more ſuſceptible than that, of the alterations requiſite to make a diamond ſhine, as that appears to be more ſuſceptible of them than ordinary diamonds. And I confeſs to you, that this is not the only odd ſuſpicion (for they are not ſo much as conjectures) that what I tried upon this diamond ſuggeſted to me. For not here to entertain you with the changes I think may be effected even in harder ſort of ſtones, by ways not vulgar, nor very promiſing, becauſe I may elſewhere have occaſion to ſpeak of them, and this letter is but too prolix already; that which I ſhall now acknowledge to you is, that I began to doubt, whether there may not in ſome caſes be ſome truth in what is ſaid of the right turquois, that it often changes colour as the wearer is ſick or well, and manifeſtly loſes its ſplendour at his death. For when I found, that even the warmth of an affriction, that laſted not above a quarter of a minute, nay, that of my body (whoſe conſtitution, you know, is none of the hotteſt) would make a manifeſt change in the ſolideſt of ſtones, a diamond; it ſeemed not impoſſible, that certain warm and ſaline ſteams, iſſuing from the body of a living man, may by their plenty or paucity, or by their peculiar nature, or by the total abſence of them, diverſify the colour and the ſplendour of ſo ſoft a ſtone as the turquois. And though I admired to ſee, that I know not how many men, otherwiſe learned, ſhould confidently aſcribe to jewels ſuch virtues, as ſeem no way compatible to inanimate agents, if to any corporeal ones at all; yet as to what is affirmed concerning the turquois's changing colour, I know not well how to reject the affirmation of ſo learned (and which in this caſe is much more conſiderable) ſo judicious a lapidary as *Boetius de Boot* *, who upon his own particular and repeated experience delivers ſo memorable a narrative of the turquois's changing colour, that I cannot but think it worth your peruſal; eſpecially ſince a much later and very experienced author, *Olaus Wormius*, where he treats of that ſtone, confirms it with this teſtimony: *Imprimis memorandum exemplum, quod Anſhelmus Boëtius de ſeipſo refert, tam mutati coloris quam à caſu preſervationis. Cui & ipſe haud diſſimile adferre poſſum, niſi ex Anſhelmo petitum quis putaret.* I remember, that I ſaw two or three years ſince a turquois (worn in a ring) wherein there were ſome ſmall ſpots, which the virtuoſo, whoſe it was, aſſured me he had obſerved to grow ſometimes greater, ſometimes leſs, and to be ſometimes in one part of the ſtone, ſometimes in another. And I having encouraged to make pictures from time to time of the ſtone and of the ſituation of the cloudy parts, that ſo their motion may be more indiſputable, and better obſerved, he came to me about the middle of this very week, and aſſured me, that he had, as I wiſhed, made from time to time ſchemes or pictures of the differing parts of the ſtone; whereby the ſeveral removes and motions of the above mentioned clouds are very manifeſt, though the cauſe ſeemed to him very occult. Theſe pictures he has promiſed to ſhew me, and is very ready to put the ſtone itſelf into my hands. But the ring having been the other day caſually broken upon his finger, unleſs it can be taken out, and ſet again without any conſiderable heat, he is loth to have it meddled with, for fear its peculiarity ſhould be thereby deſtroyed. And poſſibly his apprehenſion would have been ſtrengthened, if I had had opportunity to tell him what is related by the learned *Wormius* of an acquaintance of his, that had a nephritic ſtone, of whoſe eminent virtues he had often experience even in himſelf, and for that cauſe wore it ſtill about his wriſt; and yet going upon a time into a bath of fair water only wherein certain herbs had been boiled, the ſtone, by being

Olaus Wormius *in Muſæ.* 18 pag. 186.

Muſæum Worm. pag. 99.

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* *The narrative in the author's own words, is this:* Ego *(ſays he)* ſanctè affirmare poſſum me unam aureo annulo incluſam perpetuo geſtare, cujus facultatem (ſi gemmæ eſt) nunquam ſatis admirari potui. Geſtaverat enim ante triginta annos Hiſpanus quidem non procul a paternis ædibus habitans. Is cum vitâ functus eſſet, & ipſius ſupellex (ut moris apud nos eſt) venum expoſita eſſet, inter cætera etiam turcois exponebatur. Verum nemo (licet complures eo concurriſſent, ut eam propter coloris elegantiam, quam vivo domino habuerat, emerent) ſibi emptam voluit, priſtinum enim nitorem & colorem prorſus amiſerat, ut potius malachites, quam turcois videretur. Aderat tum temporis gemmæ habendæ deſiderio etiam parens & frater meus, qui antea ſæpius gratiam & elegantiam ipſius viderant, mirabundi eam nunc tam eſſe deformem: emit eam nihilominus pater, ſatiſque vili pretio, qua omnibus contemptui erat, ac præſentes non eam eſſe quam Hiſpanus geſtarat, arbitrarentur. Domum reverſus pater, qui tam turpem gemmam geſtare ſibi indecorum putabat, eam mihi dono dat, inquiens; Quandoquidem, fili mi, vulgi fama eſt, turcoidem, ut facultates ſuas exercere poſſit, dono dari debere, tibi eam devoveo: ego acceptam gemmam ſculptori trado, ut gentilitia mea inſignia illi, quemadmodum fieri ſolet, in jaſpide chalcedonio, aliiſque ignobilioribus gemmis, inſculperet Turpe enim exiſtimabam, hujuſmodi gemmâ ornatus gratia, dum gratiam nullam haberet, uti. Paret ſculptor, redditque gemmam, quam geſto pro annulo ſignatorio. Vix per menſem geſtaram, redit illi priſtinus color, ſed non ita nitens propter ſculpturam, ac inæqualem ſuperficiem. Miramur omnes gemmam, atque id præcipuè, quod color indies pulchrior fieret. Id quià obſervabam, nunquam fere eam a manu depoſui, ita ut nunc adhuc eandem geſtem.

wetted with this decoction, was deprived of all its virtue; whence *Wormius* takes occaſion to advertiſe the ſick, to lay by ſuch ſtones, whenſoever they make uſe of a bath. And we might expect to find turquois likewiſe, eaſily to be wrought upon in point of colour, if that were true, which the curious *Antonino Neri*, in his ingenious *Arte Vetraria*, teaches of it; namely, that turquoiſes diſcoloured, and grown white, will regain and acquire an excellent colour, if you but keep them two or three days at moſt covered with oil of ſweet almonds, kept in a temperate heat by warm aſhes: I ſay, if it were true, becauſe I doubt whether it be ſo, and have not as yet had opportunity to ſatisfy myſelf by trials; becauſe I find, by the confeſſion of the moſt ſkilful perſons, among whom I have laid out for turquoiſes, that the true ones are great rarities, though others be not at all ſo. And therefore I ſhall now only mind you of one thing, that you know as well as I, namely, that the rare ſtone, which is called *Oculus Mundi*, if it be good in its kind, will have ſo great a change made in its texture by being barely left a while in the languideſt of liquors, common water, that from opacous it will become tranſparent, and acquire a luſtre, of which it will again be deprived, without uſing any other art or violence, by leaving it a while in the air. And before experience had ſatisfied us of the truth of this, it ſeemed as unlikely, that common water or air ſhould work ſuch great changes in that gem, as it now ſeems that the effluviums of a human body ſhould effect leſſer changes in a turquois, eſpecially if more ſuſceptible of them, than other ſtones of the ſame kind. But both my watch and my eyes tell me, that it is now high time to think of going to ſleep; matters of this nature will be better, as well as more eaſily, cleared by conference than writing. And therefore ſince I think you know me too well to make it needful for me to diſclaim credulity, notwithſtanding my having entertained you with all theſe extravagancies; for you know well, how wide a difference I am wont to put betwixt things that barely may be, and things that are; and between thoſe relations that are but not unworthy to be enquired into, and thoſe that are not worthy to be actually believed; without making apologies for my ravings, I ſhall readily comply with the drowſineſs, that calls upon me to releaſe you: and the rather becauſe Monſieur *Zulichem* being concerned in your deſire to know the few things I have obſerved about the ſhining ſtone; to entertain thoſe with ſuſpicions, that are accuſtomed not to acquieſce but in demonſtrations, were a thing, that cannot be looked upon as other than very improper by

Arte Vetraria, lib. 7. cap. 102.

SIR,

Your moſt affectionate and

moſt faithful Servant,

R. BOYLE.

OBSERVATIONS made * this 27th of *October*, 1663, about Mr. *Clayton*'s Diamond.

BEING looked on in the † day-time, though in a bed, whoſe curtains were carefully drawn, I could not diſcern it to ſhine at all, though well rubbed; but about a little after ſun-ſet, whilſt the twilight yet laſted, nay, this morning a pretty while after ſun-riſing (but before I had been abroad in the more freely enlightened air of the chamber) I could upon a light affriction eaſily perceive the ſtone to ſhine.

2. THE candles being removed, I could not in a dark place diſcern the ſtone to have any light, when I looked on it, without having rubbed or otherwiſe prepared it.

3. BY two white pebbles, though hard rubbed, one againſt another, nor by the long and vehement affriction of rock cryſtal againſt a piece of red cloth, nor yet by rubbing two diamonds ſet in a ring, as I had rubbed this ſtone, I could produce any ſenſible degree of light.

4. I FOUND this diamond hard enough, not only to enable me to write readily with it upon glaſs, but to grave on rock-cryſtal itſelf.

5. I FOUND ‡ this to have, like other diamonds, an electrical faculty.

6. BEING rubbed upon my clothes, as is uſual for the exciting of amber, wax, and other electrical bodies, it did in the dark manifeſtly ſhine like rotten wood, or the ſcales of whitings, or other putrified fiſh.

7. BUT this conſpicuouſneſs was fainter than that of the ſcales and ſlabber (if I may ſo call it) of whitings, and much fainter than the light of a glow-worm, by which I have been ſometimes able to read a ſhort word; whereas after an ordinary affriction of this diamond, I was not able to diſcern diſtinctly by the light of it any of the neareſt bodies. And this glimmering alſo did very manifeſtly and conſiderably decay preſently, upon the ceaſing of the affriction, though the ſtone continued viſible ſome while after.

8. BUT if it were rubbed upon a convenient body for a pretty while, and briſkly enough, I found the light would be for ſome moments much more conſiderable, almoſt like the light of a glow-worm; inſomuch that after I ceaſed rubbing, I could with the chafed ſtone exhibit a little luminous circle, like that, but not ſo bright as that, which children make by moving a ſtick fired at the end: and in this caſe it would continue viſible about ſeven or eight times as long as I had been in rubbing it.

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* Theſe were brought in and read before the Royal Society, (the day following) Oct. 28, 1663.

The ſtone itſelf being to be ſhown to the Royal Society, when the obſervations were delivered, I was willing (being in haſte) to omit the deſcription of it; which is in ſhort, that it was a flat or table diamond, of about a third part of an inch in length, and ſomewhat leſs in breadth; that it was a dull ſtone, and of a very bad water, having in the day-time very little of the vividneſs of even ordinary diamonds, and being blemiſhed with a whitiſh cloud about the middle of it, which covered near a third part of the ſtone.

† Haſte made me forget to take notice, that I went abroad the ſame morning, the ſun ſhining forth clear enough, to look upon the diamond through a microſcope, that I might try whether by that magnifying glaſs any thing of peculiar could be diſcerned in the texture of the ſtone, and eſpecially of the whitiſh cloud, that poſſeſt a good part of it. But for all my attention I could not diſcover any peculiarity worth mentioning.

‡ 5. For it drew light bodies like amber, jet, and other concretes, that are noted to do ſo: but its attractive power ſeemed inferior to theirs.

9. I FOUND, * that holding it a while near the flame of a candle (from which yet I was careful to avert my eyes) and being immediately removed into the dark, it diſcloſed ſome faint glimmering, but inferior to that it was wont to acquire by rubbing. And afterward holding it near a fire, that had but little flame, I found the ſtone to be rather leſs than more excited, than it had been by the candle.

10. I LIKEWISE endeavoured to make it ſhine, by holding it a pretty while in a very dark place, over a thick piece of iron, that was well heated, but not to that degree as to be viſibly ſo. And though at length I found, that by this way alſo the ſtone acquired ſome glimmering, yet it was leſs than by either of the other ways abovementioned.

11. I ALSO brought it to ſome kind of glimmering light, by taking it into bed with me, and holding it a good while upon a warm part of my naked body.

12. To ſatisfy myſelf, whether the motion introduced into the ſtone did generate the light upon the account of its producing heat there, I held it near the flame of a candle, till it was qualified to ſhine pretty well in the dark; and then immediately I applied a ſlender hair, to try whether it would attract it, but found not that it did ſo: though if it were made to ſhine by rubbing, it was, as I formerly noted, electrical. And for further confirmation, though I once purpoſely kept it ſo near the hot iron I juſt now mentioned, as to make it ſenſibly warm, yet it ſhined more dimly than it had done by affriction, or the flame of a candle, though by both thoſe ways it had not acquired any warmth that was ſenſible.

13. HAVING purpoſely rubbed it upon ſeveral bodies differing as to colour and as to texture, there ſeemed to be ſome little diſparity in the excitation (if I may ſo call it) of light. Upon white and red cloaths it ſeemed to ſucceed beſt, eſpecially in compariſon of black ones.

14. BUT to try what it would do rubbed upon bodies more hard, and leſs apt to yield heat, upon a light affriction, than cloth, I firſt rubbed it upon a white wooden box, by which it was excited, and afterwards upon a piece of purely glazed earth; which ſeemed, during the attrition, to make it ſhine better than any of the other bodies had done, without excepting the white ones; which I add, leſt the effect ſhould be wholly aſcribed to the diſpoſition white bodies are wont to have to reflect much light.

15. HAVING † well excited the ſtone, I nimbly plunged it under water, that I had provided for that purpoſe, and perceived it to ſhine whilſt it was beneath the ſurface of that liquor, and this I did divers times. But when I endeavoured to produce a light by rubbing it upon the lately mentioned cover of the box, the ſtone and it being both held beneath the ſurface of the water, I did not well ſatisfy myſelf in the event of the trial: but this I found, if I took the ſtone out, and rubbed it upon a piece of cloth, it would not, as elſe it was wont to do, preſently acquire a luminouſneſs, but needed to be rubbed manifeſtly much longer, before the deſired effect was found.

16. I ALSO ‡ tried ſeveral times, that by covering it with my warm ſpittle (having no warm water at hand) it did not loſe its light.

17. FINDING

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* 9. We durſt not hold it in the flame of a candle, no more than put it into a naked fire; for fear too violent a heat (which has been obſerved to ſpoil many other precious ſtones) ſhould vitiate and impair a jewel, that was but borrowed, and was ſuppoſed to be the only one of its kind.

† 15. We likewiſe plunged it, as ſoon as we had excited it, under liquors of ſeveral ſorts, as ſpirit of wine, oils both chymical and expreſſed, an acid ſpirit, and, as I remember, an alcalizate ſolution; and found not any of thoſe various liquors to deſtroy its ſhining property.

‡ 16. Having found by this obſervation, that a warm liquor would not extinguiſh light in the diamond, I thought fit to try, whether, by reaſon of its warmth, it would not excite it; and divers times I found, that

17. FINDING that by rubbing the ſtone with the flat ſide downwards, I did, by reaſon of the opacity of the ring, and the ſudden decay of light upon the ceaſing of the attrition, probably loſe the ſight of the ſtone's greateſt vividneſs; and ſuppoſing that the commotion made in one part of the ſtone will be eaſily propagated all over; I ſometimes held the piece of cloth, upon which I rubbed it, ſo that one ſide of the ſtone was expoſed to my eye, whilſt I was rubbing the other; whereby it appeared more vivid than formerly, and to make luminous tracts by its motions to and fro. And ſometimes holding the ſtone upwards, I rubbed its broad ſide with a fine ſmooth piece of tranſparent horn, by which means the light through that diaphanous ſubſtance did, whilſt I was actually rubbing the ſtone, appear ſo briſk, that ſometimes, and in ſome places, it ſeemed to have little ſparks of fire.

18. I TOOK alſo a piece of flat blue glaſs, and having rubbed the diamond well upon a cloth, and nimbly clapt the glaſs upon it, to try whether, in caſe the light could pierce it, it would by appearing green, or of ſome other colour than blue, aſſiſt me to gueſs, whether itſelf were ſincere or no. But finding the glaſs impervious to ſo faint a light, I then thought it fit to try, whether hard bodies would not by attrition increaſe the diamond's light, ſo as to become penetrable thereby: and accordingly when I rubbed the glaſs briſkly upon the ſtone, I found the light to be conſpicuous enough, and ſomewhat dyed in its paſſage; but found it not eaſy to give a name to the colour it exhibited.

LASTLY, To comply with the ſuſpicion I had upon the whole matter, that the chief manifeſt change wrought in the ſtone was by compreſſion of its parts, rather than incaleſcence; I took a piece of white tile well glazed, and if I preſſed the ſtone hard againſt it, it ſeemed, though I did not rub it to and fro, to ſhine at the ſides. And however it did both very manifeſtly and vigorouſly ſhine, if, whilſt I ſo preſſed it, I moved it any way upon the ſurface of the tile, though I did not make it draw a line of above a quarter of an inch long, or thereabouts, and though I made it not move to and fro, but only from one end of the ſhort line to the other, without any return or lateral motion. Nay, after it had been often rubbed, and ſuffered to loſe its light again, not only it ſeemed more eaſy to be excited than at the beginning of the night; but if I did preſs hard upon it with my finger, at the very inſtant that I drew it briſkly off, it would diſcloſe a very vivid but exceeding ſhort-lived ſplendor, not to call it a little coruſcation *. So that a Carteſian would ſcarce ſcruple to think, he had found in this ſtone no ſlight confirmation of his ingenious maſter's hypotheſis, touching the generation of light in ſublunary bodies, not ſenſibly hot.

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that if it were kept therein, till the water had leiſure to communicate ſome of its heat to it, it would often ſhine as ſoon as it was taken out; and probably we ſhould have ſeen it ſhine more, whilſt it was in the water, if ſome degree of opacity, which heated water is wont to acquire, upon the ſcore of the numerous little bubbles generated in it, had not kept us from diſcerning the luſtre of the ſtone.

* I after bethought myſelf of employing a way, which produced the deſired effect, both ſooner and better. For holding betwixt my fingers a ſteel bodkin, near the lower part of it, I preſſed the point hard againſt the ſurface of the diamond, and much more if I ſtruck the point againſt it, the coruſcation would be extremely ſudden, and very vivid, though very vaniſhing too: and this way, which commonly much ſurprized and pleaſed the ſpectators, ſeemed far more proper than the other, to ſhew, that preſſure alone, if forcible enough, though it were ſo ſudden and ſhort, that it could not well be ſuppoſed to give the ſtone any thing near a ſenſible degree of warmth, as may be ſuſpected of rubbing, yet it is ſufficient to generate a very vivid light.

A POSTSCRIPT, *annexed ſome Hours after the* Obſervations *were written.*

SO * many particulars taken notice of in one night, may make this ſtone appear a kind of prodigy; and the rather, becauſe having tried, as I formerly noted, not only a fine artificial cryſtal, and ſome alſo that is natural, but a ruby and two diamonds, I did not find, that any of theſe diſcloſed the like glimmering of light; yet after all, perceiving by the hardneſs, and the teſtimony of a ſkilful goldſmith, that this was rather a natural than artificial ſtone; for fear leſt there might be ſome difference in the way of ſetting, or in the ſhape of the diamonds I made uſe of, neither of which was like this, a flat table-ſtone, I thought fit to make a further trial of my own diamonds, by ſuch a briſk and aſſiduous affriction, as might make amends for the diſadvantages above-mentioned, in caſe they were the cauſe of the unſucceſsfulneſs of the former attempts. And accordingly I found, that by this way I could eaſily bring a diamond I wore on my finger to diſcloſe a light, that was ſenſible enough, and continued ſo, though I covered it with ſpittle, and uſed ſome other trials about it. And this will much leſſen the wonder of all the formerly mentioned obſervations, by ſhewing, that the properties, that are ſo ſtrange, are not peculiar to one diamond, but may be found in others alſo, and, perhaps, in divers other hard and diaphanous ſtones, Yet I hope, that what this diſcovery takes away from the wonder of theſe obſervations, it will add to the inſtructiveneſs of them, by affording pregnant hints towards the inveſtigation of the nature of light.

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* We afterwards tried precious ſtones, as diamonds, rubies, ſaphires, and emeralds, &c. but found not any of them to ſhine, except ſome diamonds; and of theſe we were not, upon ſo little practice, able to foretel beforehand, which would be brought to ſhine, and which would not: for ſeveral very good diamonds either would not ſhine at all, or much leſs than others, that were far inferior to them. And yet thoſe ingenious men are miſtaken, that think a diamond muſt be foul and cloudy, as Mr. *Clayton's* was, to be fit for ſhining; for as we could bring ſome ſuch to afford a glimmering light, ſo with ſome clear and excellent diamonds we could do the like. But none of thoſe many, that we tried of all kinds, were equal to the diamond, on which the obſervations were made, not only conſidering the degree of light it afforded, but the eaſineſs wherewith it was excited, and the comparatively great duration of its ſhining.

End of the FIRST VOLUME.

MVSEVM
BRITAN
NICVM